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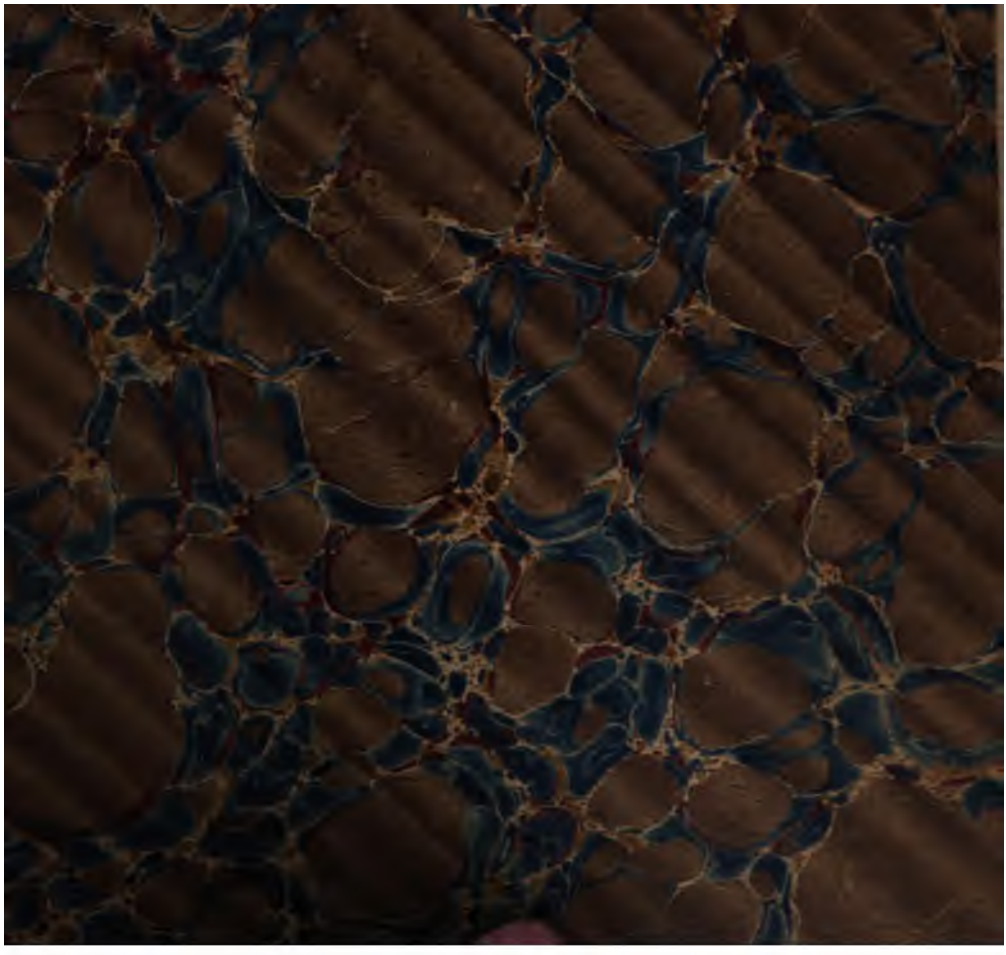

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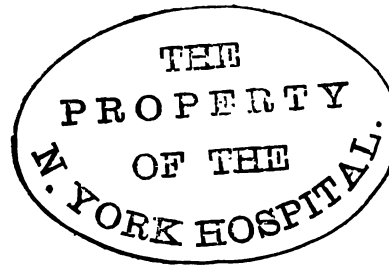

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DISEASES OF CHILDREN.

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BY THE PRESIDENT,

DR. CHARLES WEST.

GENTLEMEN,—My first duty on taking this chair is a most pleasant one. It is to express my deep sense of the honour done me by my countrymen when they selected me as not unworthy to represent that department of medicine in England which we all assembled here more especially cultivate. The honour, too, was enhanced by the fact that at the time when it was conferred I was on the point of leaving London in search of what I am thankful to say I found—perfect health in a land of constant sunshine. That I have found there, too, a second home, I owe it to the kindness of you, my French friends, who received me so cordially and treated me so graciously. You did not regard me as a stranger, but as a fellow-member of that great *Société Internationale* which has for its object, not the upsetting of thrones nor the changing of Governments in quest of some grand social regeneration, to be accomplished in a few days by violence and bloodshed, but the improvement of mankind by gentle means. The one, like the thunderstorm and the torrent, does but lay waste; the other is like the silent dew, which falls unseen and fertilizes the land. But while I thank you most heartily for all your goodness to me in what I may now call my adopted country, you will, I am sure, find it but natural that I rejoice in returning

once more to my native land; in seeing again the old familiar faces; and revisiting the spots where I studied as a youth or where I laboured as a grown man.

"Cœlum, non animam, mutant,
Qui trans mare currunt."

And my French sympathies are not one jot lessened because I still feel myself altogether an Englishman.

With these words, gentlemen, I should have wished to stop, and to have invited you to pass at once to the business for which we are met. Some three weeks ago, however, I learned to my dismay that the Executive Committee desired that the President of each Section should open its meetings by a short address bearing on its special objects. Far away from my books, moving each day from place to place, I felt my utter inability to do anything worthy of the occasion. Moreover, there came to my recollection an anecdote which did not help to cheer me. Dr. Johnson and his friend Boswell dined one day with a gentleman by special invitation. The next day Dr. Johnson complained to his friend of the meagreness of the entertainment. "Well, sir," said Boswell, "but it was a good dinner." "Yes," replied Johnson, "a good enough dinner, but it was not a dinner to ask a man to." And so, how scanty and commonplace what I say is, pray remember, gentlemen, the entertainment is not one which, had I been left to myself, I should have thought good enough to ask you to.

One accusation which I have heard brought against a meeting like the present is, that it is apt to resolve itself into a mutual admiration society, each member praising what the other has done, all joining to extol what their own generation has accomplished; and that the gratification of personal vanities, not the promotion of science, is the chief outcome of the whole. But just as travellers on a long journey halt from time to time, and looking back on the road they have traversed, take courage to go further, so may we, with no feeling of undue self-gratulation, rejoice over what has been accomplished, even in our own day, as an earnest and as a pledge of further progress, an inducement to more unwearied effort. Thirty years ago, throughout the whole of England and America there was not a single hospital set apart for children. It was but rarely that one saw these little waifs and strays in the wards of our general hospitals; for the maxim "De minimis non curat lex" held good in medicine as in law. Germany, too, was in but little better case, and one was forced to go to Paris to study on a large scale those diseases which men like Guersant, and Blache, and Baron, and Trousseau and Roger, investigated with untiring zeal, and, in spite of the hospital arrangements most painfully defective, strove to cure. We all know how this is altered now. In London there are six separate children's hospitals, each, I believe, with its convalescent branch; and children's wards are to be found in every one of the large London hospitals. There are special children's hospitals in every large town in England; America and Germany have followed the same example; and everywhere throughout Europe the opportunities for the study of children's diseases are almost as numerous as for those of the adult. Nor has this wide field been without abundant husbandmen to till it, and we may count with satisfaction the fruit of their labours. The vague phraseology which served for years to conceal our ignorance, even from ourselves, has been to a great degree done away with. We talk no longer of worm fever, remittent fever, gastric fever, and so on, for under these

various names we recognise the one disease, typhoid fever, varying in severity, but marked always by its own characteristic symptoms. Half a page in a handbook was all that was to be found thirty years ago concerning heart disease in childhood, while at the present time the frequency of heart disease has been fully recognised, and it has been studied with as much care in the child as in the adult. The various inflammations of the respiratory organs are no longer looked on as a whole, but each is referred to its proper class, and we distinguish lobar and lobular pneumonia, bronchitis and capillary bronchitis, and assign to each its proper place and its characteristic symptoms. Nor have our therapeutics lagged behind. I remember the hesitation with which, some forty years ago, my dear friend and master, the late Dr. Latham, decided on tapping the chest of a boy eight years of age, who was received into St. Bartholomew's Hospital on account of a pleurisy which had terminated in empyema; and the delight—the wonderment almost—with which we regarded the successful issue of the operation in a child so young. A few months ago I communicated to the Medical Society of Nice the particulars of fifty cases in my own practice where paracentesis of the chest had been performed at my desire, and several of you gentlemen could relate as many cases or more. That once almost unrecognised disease, diphtheria, has been studied with the greatest care; its relation to membranous croup has been investigated; the close connection of the two has been demonstrated. I for my part should not hesitate to say their absolute identity has been established. Much light has been thrown on various diseases of the nervous system. That once enigmatical affection, the so-called essential paralysis of infancy and childhood, has been shown (in the first instance by the researches of my friend M. H. Roger, and his able coadjutor M. Damaschino) to be due to an acute inflammatory softening of the grey matter of the anterior columns of the spinal cord; and twenty-five recorded observations since that time attest the truth of their discovery. Though, strictly speaking, perhaps not a disease of the nervous system, the pseudo-hypertrophic muscular paralysis of Duchenne claims mention here as a new and important addition to our knowledge of the pathology of early life.

I fear to weary you by further enumeration, else it would not be difficult to increase largely the instances of new and most important knowledge added to our stores since my student-days. In estimating the value of these gains, too, it must not be forgotten that each truth established means an error exploded; so much base metal, so much counterfeit coin withdrawn from circulation; or, to put it differently, so much sterling gold substituted for inconvertible paper money. In this process surgery has, as everywhere, borne a large part. The treatment of hip-disease, the excision of scrofulous joints, the new modes of treatment of spinal curvature—some, indeed, still on their trial—the operation for the cure of genu valgum, which one cannot mention without a fresh tribute of thanks to Joseph Lister, who in this instance has rendered a proceeding safe and salutary from which, but a few years since, the common sense of the surgeon would have recoiled, are so many fresh instances of progress made during a period of little more than the half of my professional life. I take it, however, that the great use of meetings such as the present is to take stock far less of what we know than of what we do not know, or know at best but imperfectly. A few of these problems have been submitted to you in the list of subjects for discussion. To some it is probable that the combined experience of so many and such distinguished men as are here present may furnish definite and conclusive answers. Other questions are introduced in the hope of gaining fresh infor-

mation on points concerning which our knowledge is fragmentary; while there are many other problems still unsolved on which it is hoped that fresh light will be thrown during the time of our meeting here.

And now, with your permission, I will conclude with an old apologue, which tells how, when the fabled Arabian bird renewed each hundred years its vigour and eternal youth, the birds of the air all helped to build its nest. The eagle and the wren contributed alike to this labour of love and duty; each brought what he could, nor ceased till the task was done. And surely science and art, especially our science and art, are old and new, renewing day by day, and burning by a voluntary self-cremation old theories, half facts, hasty conclusions, and substituting more accurate observations, truer inferences, more solid judgments. To this great end we may all do something; but, labour as we may, our task will never be finished, for not once in a hundred years, as the fable runs, but every day and all day long the process goes on—a daily death, a daily renewal, as in our bodies' growth—a death of error, a development of truth.

On the Existence of Two Distinct Forms of Eruptive Fever, usually included under the head of Measles, and the Relation to them of so-called Rubeola or Rötheln.

Dr. W. B. Cheadle, London.

One of the main points laid down as distinctive of the group of contagious exanthemata, from the time of Cullen downwards, is that each disease occurs in the same individual but once in a lifetime—that is, that one attack of any of these eruptive fevers confers on the individual who suffers it immunity from a second; and the rule has been found to hold good, not, indeed, as absolutely invariable, but with exceptions so few and rare as not seriously to invalidate the general law.

One of the chief members of the group—viz., measles—has been found to respond to the test with striking uniformity whenever an outbreak has occurred under conditions favourable to exact and comprehensive observations; as, for example, in the two well-known epidemics in the Faroe Islands, when all who had had the disease in the first, without a single exception, escaped the second.*

Willan † had never met with a case of measles occurring a second time in the same person, and Thomas ‡ says that a second attack of measles is exceedingly rare, as rare as a second attack of smallpox or scarlatina. I might quote a large number of authorities to show that, although a few exceptions have been recorded, the law holds that one attack of measles affords almost certain pro-

* Dr. Panum, *Archives Générales de Médicine*, April, 1851, quoted by Sir T. Watson.

† Dr. Churchill ("Diseases of Children," p. 723, 3rd edition) sums up the evidence on this point to the following effect:—

Rosenstein never met with a second attack in forty years' practice.

Willan and others give the same as the result of twenty years' experience.

Dr. Baillie shows it may recur.

Dr. Dewees is doubtful, except where the first was rubeola sine catarrho.

Eberle mentions one example.

Rayer, three cases under his own observation.

Guersant and Blache give examples of the recurrence of the disease twice in same year.

Churchill doubtful, but gives one example in his own child.

Genovesi attended forty-six in Santa Cruz who had had the disease before, and Dubosq de la Roberdière prescribed in 1777 for persons he had treated in 1773.

‡ Ziemssen's "Cyclopædia," art. Measles.

tection against a second. My own experience was entirely in accord with this until a short time ago, when my confidence in this comfortable creed received a rude, although happily only a temporary, shock.

An epidemic of measles broke out in the district of London in which I live in December, 1878. Three of my own children caught the disease, and many other cases came under my observation at the time. The disorder exhibited all the symptoms of true measles. It was ushered in with incessant sneezing, profuse running at the eyes and nose, suffusion of the conjunctivæ, slight hoarseness, frequent but not severe cough, some diarrhœa, plentiful and well-marked crescentic eruption coming out on the morning of the fourth day, and a temperature of 102° to 103° or 103·5° during the stage of eruption. In a word, the majority of cases which came under my notice in the epidemic of 1878 were fairly typical examples of measles of more than average severity.

In November, 1879, or not quite a year later, measles broke out in the school which two of my boys attended, and appeared in a form far more serious than that of the previous epidemic. The boys in question were two of the three who had measles in the last outbreak, eleven months before, and I congratulated myself that they at least were safe from attack. But these two supposed protected ones forthwith took the disease, and transmitted it to my three other children, one of whom had also previously had measles in 1878.

Following immediately upon this startling recurrence of the disease in my own family came other instances, showing in an equally forcible manner the complete absence of any protective power exerted by the previous epidemic.

In two other families, numbering seven children in all, six had measles in the first epidemic, eleven months before, at the same time, and contracted from the same source, as my own children; the seventh child had had the complaint a year earlier. These seven children were all exposed in like manner to the contagion of the new epidemic; all took the disease again without exception—not one escaped. Again, three children of another family, under my immediate observation, who had had measles of severe type three years previously, were exposed to the contagion of the second epidemic, and from the same source as those above mentioned. All took the complaint and had it severely.

Thus it came about that, of the first fifteen cases of the second outbreak which came under my notice, no less than thirteen had had measles before, and nine of the thirteen under my own eye within the year.

Altogether I have been able to obtain a trustworthy history of thirty cases. Of these, eighteen came under my own observation at the time; the remaining eleven, although not seen at the time of eruption, occurred in individuals with whom I am acquainted, have been carefully investigated, and are thoroughly reliable. Out of this total of thirty cases of the second epidemic, twenty-two had certainly had measles previously. The protected individuals suffered equally with those who were exposed to the contagion for the first time. They took the disease just as readily; they had it just as severely.

At the outset of this second epidemic the character of the exanthem was one of grave and even dangerous type.* No instance in which the disease proved fatal to an individual previously healthy came under my own observation; but in three cases under my care the condition was for some time critical, and four deaths occurred in children suffering from other diseases at the time. Three were killed directly by the measles during the eruptive stage; the fourth died from the effects of broncho-pneumonia set up by it.†

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* The first twenty cases might be classed as severe; the remaining ten, all occurring at the close of the epidemic, were of milder type, and in some the constitutional disturbance was extremely slight.

† Three cases of empyema (two convalescent) and one of tetany.

The course and symptoms of the disease in this second epidemic of 1879 showed appreciable variations from those of the disorder which prevailed the year before, which might be taken as a typical example of the ordinary form. Yet it may be said, I think, that all the phenomena were such as have been described, at one time or other, as arising in the varying phases of true measles, and the outbreak was generally regarded, without question, as genuine measles of an unusually severe kind. If that were so, then in all cases previous attacks of measles absolutely failed to give the protection which experience proves them to confer, almost without exception, against a second attack of the same disease. It is impossible to accept this solution without question. The suspicion arises that the disease of the second epidemic, although at first sight apparently identical with measles, might in reality be as specifically distinct from it as typhus from typhoid or variola from varicella. Slight differences may indicate a generic distinction, masked by a general superficial agreement in more obvious characteristics, and a careful analysis of the phenomena of the second epidemic shows certain points of difference and some special features which, taken together, lend support to the theory of its specific distinctness.

I.—*The Period of Incubation.*—This was accurately ascertained in two cases where there was a single exposure to infection, limited to a few hours. In one,* the first symptoms showed themselves on the eighth day; the eruption on the eleventh. In the other,† the first signs were observed on the ninth day; the eruption was out on the morning of the twelfth day after exposure.

The period of incubation was ascertained approximately in five other cases. In two,‡ where the exposure lasted three days, this could not have been more than twelve days or less than ten, the period from eruption to eruption being ten days; and in the remaining three§ the period from eruption to eruption was uniformly twelve days.

No opportunity arose for testing the incubation period in the first epidemic, but I have ascertained it in two cases of ordinary measles where there was a single short exposure.‖ The first symptoms were observed on the tenth day; the rash appeared in both instances in the night between the thirteenth and fourteenth days. These conclusions are in exact correspondence with the observations of Panum in the Faroe Island epidemics, and are confirmed by other accurate records, as shown by Thomas in a very complete examination of the subject.¶

The period of incubation in true measles appears to have a duration of extreme constancy—viz., thirteen to fourteen days to the appearance of the eruption, and usually, but much less uniformly, of ten days to the first symptoms; so that in the epidemic we are considering the period of incubation was shorter than the normal one of true measles by from one to two days.

II.—*The Period of Invasion* was also shorter than normal by a day, although, as this appears to be liable to variation in true measles, much stress cannot be laid upon the difference in the eight cases in which this point was noted with exactness. In seven the rash appeared on the third day after the initial

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* L. M., exposed for a few hours, November 22, 1879. First symptoms, November 30. Rash, night of December 2-3.

† ———, child in Great Ormond Street Hospital, exposed January 7. First symptoms, January 16. Rash, night of January 18-19.

‡ W. W. C. and F. M. C., exposed to contagion November 11, 12, and 13. (Rash out in infector, 13th). First symptoms in two infected, 21st. Rash out in two infected, 23rd.

§ Ashwell, Coates, Parkins (empyemas). Eruption in infector, November 10; in infected, November 22.

‖ W. F., visited Brighton for a few hours. Exposed to measles contagion. Sickened tenth day. Rash thirteenth to fourteenth day.

Una B., slept one night with girl with measles. Sickened tenth day. Rash thirteenth to fourteenth day.

¶ Ziemssen's "Cyclopædia," art. Measles.

symptoms;* in the eighth† (one of empyema), not till the sixth day after access of febrile symptoms, but the twelfth after exposure.

III.—*The Catarrhal Symptoms and Affection of the Air Passages* exhibited certain special features. There was injection of the conjunctivæ; usually slight sneezing at the onset, which soon passed off; little running at the eyes and nose, in many cases none; no intestinal flux. On the other hand, the hoarseness and irritation of the throat were extreme, and the cough incessant, laryngeal, croup-like. In one instance this was harassing beyond anything I have ever seen. The patient (a boy of ten years old), could not be kept in bed, but stamped about the room in distress so intolerable that chloroform had to be freely given to relieve it.

In one case the respiration became difficult and stridulous; the tonsils, fauces, and soft palate were dusky red, much swollen, and covered with tenacious mucus. Enlargement of the glands at the angle of the jaw was observed in one or two instances. In one there was a film of coherent membrane on the tonsils, and albumen in the urine.‡ Marked bronchitis, evidenced by sibilant respiration and abundant fine rales at the bases, was present in all but the mildest cases. In two, broncho-pneumonia supervened.

The contrast between the two epidemics in regard to these symptoms was striking. In the first, incessant sneezing, coryza, lachrymation, comparatively slight hoarseness, frequent but not severe cough, some diarrhœa. In the second, little or no sneezing, coryza, or lachrymation, extreme hoarseness, incessant severe croup-like cough, marked implication of the larynx and bronchi, entire absence of catarrh of the intestinal mucous membrane.

IV.—*The Eruption* presented some points of divergence from the normal measles' rash. It was more raised, more coarse and papular; the grouping of the stigmata was not crescentic, but in irregular blotches, and of a darker, more purple hue. In the severest cases it was confluent on the face and backs of the wrists and hands, accompanied by much swelling. In one instance there were petechiæ and small purpuric patches on the extremities; and in this case the livid papular petechial rash and the swollen face and hands bore at first sight a strong resemblance to the condition in severe smallpox.

In two instances of more moderate intensity, but contracted from one of the more virulent cases, the confluent eruption on the face and limbs, in place of being purple and papular, was diffuse and rosy—closely simulating in these points the rash of scarlatina—elsewhere of the patchy, purple, measles form. In all these cases the eruption did not reach its maximum until the third day, the most intense not beginning to decline until sixty hours, or even more, after its first appearance—*i.e.*, at the close of the third or beginning of the fourth day.

V.—*The Eruptive Fever* ran high. The temperature went up to 103° as the rash came out, and continued to rise a little until the eruption reached its height, and ran up to 104° or 104·5° at the maximum. In the majority of instances the temperature began to fall on the fourth day of eruption; but in two, in which there was no complication, it remained at 103° to 104° for two days later, or the fall did not fairly commence until the sixth day.

The pulse ranged from 130° to 160° at the maximum. In one case (A. C.)

* W. and F. C., L. M., L. and M. F. (private cases). Coates, Parkins (Children's Hospital).

† Ashwell (Children's Hospital.) Exposed, November 10. First symptoms, November 17 (rise of temp. to 100·8°, continuing up to eruption).

‡ In an epidemic of measles which prevailed contemporaneously at Folkestone, and which probably was of the same generic form, the affection of the throat was equally prominent. I learn from Mr. Tyson of that town, who has kindly written to me with this information, that in three cases the laryngeal obstruction was so great that tracheotomy had to be performed; in another the operation was threatened, and that other cases of a similar kind had occurred.

without complication it remained 130° to 140° for forty-eight hours after the eruption reached its maximum, and was feeble and irregular.

During this period the tongue was much coated and became dry; there was night delirium; and, as the eruption came out, drowsiness extreme beyond all precedent, lasting for some days after the rash reached its height.

VI.—*Vomiting* was another salient feature present in all the severe cases. It was noted in thirteen, and generally showed this peculiarity:—In some instances it began with the first fever, and recurred at intervals until the eruption declined; but more usually it did not usher in the initial symptoms, as in most eruptive fevers, nor occur later in the stage of invasion *before* the appearance of the eruption, as observed sometimes in true measles, but *afterwards*, during the eruptive stage, and most commonly about the second day. This vomiting was not the result of the violent cough, but quite independent of it—a distinct effect of the measles poison.

VII.—Another symptom, present in all severe cases, with a constancy I have never observed before, was *earache*. It was noted in ten of the cases of which I had personal knowledge, and may have occurred in many more of which I have less complete records. It came on, with the greatest uniformity, about forty-eight hours after the eruption began to decline; lasted with much severity for several hours, and then disappeared. In no case was there any discharge from the external meatus, and in one only any persistent deafness afterwards.

Such were the symptoms observed in severe cases in the earlier part of the outbreak of 1879. Towards its close, the cases which arose were of exceedingly mild type. The period of invasion was extremely short, often not more than twenty-four or thirty-six hours, marked only by slight malaise, catarrh, hoarseness and cough; little or no sneezing, or flux from the eyes and nose. The temperature rose to 101° or 102° for a short period as the eruption came out, with rapid defervescence. The patients, in marked contrast to those attacked at the outset, were hardly ill.

The special features observed in the epidemic of 1879, added to the evidence afforded by the absence of any protection conferred by previous attack of common measles, confirm the view that the disease in this outbreak was specifically distinct. And if so, the question arises whether it is a new and previously unrecognised exanthem, or a severe form of so-called German measles rubeola or rötheln.

At first sight the supposition that it might be rötheln would seem to be negatived by the great severity of the majority of cases.

As far as my researches go, authorities who recognise it at all are almost unanimous in regarding it as a disease of uniformly mild type.

It is said, indeed,* to be specially distinguished from true measles by its slightness and want of character.

Trousseau † estimates it as the mildest of the eruptive fevers, standing in the same relation to measles as chicken-pox to smallpox.

Vogel speaks lightly of it as without fever or catarrh.

And thus throughout the whole list of writers on the subject, ‡ except

* Bristowe's "Theory and Practice of Medicine," p. 153.

† "Clinique Médicale," Sydenham Soc. trans., vol. ii. p. 237.

‡ Wunderlich ("Temperature in Disease") says the elevations of temperature are generally sub-febrile, or at the most moderately febrile; and that although in isolated cases more considerable elevations of temperature may be met with, they depend no doubt either upon complications or on that peculiar mobility of temperature characteristic of very young children.

Thomas (Ziemssen's "Cyclopædia," art. Rubeola) states that in the majority of cases there is no fever, but that the course of temperature is a varying one. There are cases with fever and rapid initial increase, *with defervescence at or before* the disappearance of the eruption.

Aitken,* who states that fatal cases have occurred, and describes post-mortem appearances.

Now these descriptions of the character and symptoms of rötheln apply accurately enough to the slighter cases towards the close of the second epidemic, but to them alone.

On the other hand, there is an obvious resemblance between the severe cases and those described by authors as a malignant form of common measles—the rubeola nigra of Willan, the rubéole boutonneuse and ecchymotic measles noted by Trousseau,† and the similar variety graphically described by our President, Dr. West.‡

So that, in this epidemic in question, we have cases in the first fierceness of its outbreak corresponding closely with those ordinarily classed as abnormally grave and exceptional cases of common measles, and later others of less marked characteristics, directly bred from and lineally descended from the first, corresponding in all respects with those regularly recognised as rubeola or rötheln.§

Not only were the mild cases occurring at the close of the epidemic in West London generally regarded as rötheln, but rötheln was prevalent at the time, shortly before or shortly after, in various parts of the country, as appeared from numerous reports published in the medical journals at this juncture. ‖

Further, the severe cases agree in character with that which one would naturally expect an intensified rötheln to present:—Coryza slight or absent; papular non-crescentic rash, in some cases confluent on the face and extremities, sometimes scarlatiniform there; prominent throat symptoms; absence of intestinal affection; in some cases enlargement of glands at the angle of the jaw; with a shorter period of invasion and incubation than in ordinary measles. *The general features the same as in recognised rötheln, but certain of them increased and exaggerated.*

It seems impossible to avoid the conclusion that the disease in the second epidemic which I have described was that variously known as rubeola, epidemic roseola, or rötheln, which exists not only in the slight and unimportant form generally recognised, but as an eruptive fever of considerable severity, which may assume a dangerous, and even malignant type.

No other hypothesis will, to my mind, satisfactorily explain all the facts which I have adduced.

As rötheln is distinct from true measles, so, I believe, is it distinct from scarlatina. Eight of the cases recorded had, to my own knowledge, previously suffered from scarlatina, and it has been shown repeatedly by others that previous scarlatina possesses no more protective power against rötheln than a previous attack of measles.

No connection has been traced between rötheln on the one hand and scarlatina and measles on the other. The hypothesis that rötheln is a hybrid between the two rests on no better foundation than that it presents certain superficial points of resemblance to each of them.

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* Aitken, "Practice of Medicine," art. Rötheln.

† Trousseau, "Clinical Medicine," Sydenham Soc. trans., vol. ii. p. 216.

‡ "Diseases of Infancy and Childhood," 6th edition, p. 808.

§ L. M., a boy of six, caught the disease by a solitary exposure of three hours to the contagion of two of the most extreme cases under my observation; he had the exanthem severely, and of the same form as those from whom he took it. He communicated the disease to two sisters; in them the eruption was less severe, and had all the features of typical rötheln—*i.e.*, roseolous eruption on the face and extremities, elsewhere measles-like; no catarrh; red, swollen fauces; some tumefaction of the glands under the jaw.

‖ In Manchester, February 1880, Dr. Tomkins, *Brit. Med. Jour.*, May 29, 1880.
In Bradford, end of 1879 and beginning of 1880, Dr. Burnie, ibid., June 5, 1880.
In Bolton, spring of 1880, Dr. Robinson, ibid., June 19, 1880.
In Exeter, spring of 1880, Dr. Wordman, ibid., June 26, 1880.
In Wansford, autumn of 1879, Dr. Brown, ibid., July 3, 1880.

Finally, as the outcome of these observations, I would venture to draw th following conclusions :—

1.—That rötheln is a specific contagious exanthem, distinct from eith measles or scarlatina.

2.—That the period of incubation is from eleven to twelve days; the peri of invasion from two to three days; but in mild cases this may not be mo than twenty-four hours. On these points, however, more extended observatio are desirable for their precise estimation.*

3.—That other features, which, not singly, but taken together as a clinic group, may serve to distinguish severe cases of rötheln from severe cases ordinary measles, with which they are liable to be confounded, are—

The slightness or absence of sneezing and coryza.

The greater severity and frequency of the cough; its hoarseness and lary geal character.

The more marked catarrh of the larynx and bronchi.†

The absence of intestinal catarrh, as evidenced by absence of diarrhœa.

The more papular character of the eruption; its more purple hue; t absence of any crescentic arrangement; its confluence on the face a extremities, where, in such case, it may be swollen, purple, purpur or scarlatiniform.

The higher range of temperature and its longer persistence.

The extreme drowsiness during the eruptive stage.

The occurrence of vomiting when the eruption approaches its maximum.

The occurrence of earache during its decline.

In addition to these more constant symptoms, there may be, in extre cases, exudation on the fauces, and probably in the larynx, and albumen in t urine.‡

Die wirkliche Stellung der sogenannten Rubeola, Rötheln oa "German measles" und die Verwandtschaft derselben n Scharlachfieber und Masern.

Dr. M. Kassowitz, Vienna.

Ich fühle mich in einer gewissen Hinsicht verpflichtet, mein Votum dieser von der Section für Kinderkrankheiten des internationalen-medizinisc Congresses aufgestellten These abzugeben, weil ich in einem vor 7 Jahren schienenen Aufsatze (über einige seltenere Vorkommnisse bei Masern—ös reichisches Jahrbuch für Pädiatrick, 1874, S. 79) die Erklärung abgege hatte, dass ich mich bis dahin noch nicht selber von der Existenz der Rubeo als einer selbständigen, von den Masern unabhängigen Krankheitsform h überzeugen können.

Die Gründe, die mich damals bewogen hatten, mich in dieser Frage so servirt auszusprechen, waren im Grossen und Ganzen folgende :—

* The incubation period of rötheln appears to be not clearly ascertained.
Bristowe ("Principles and Practice of Medicine") says about one week.
Aitken does not state it.
Thomas says it is uncertain, but probably two and a half to three weeks.

† Aitken ("Practice of Medicine," art. Rötheln) says the throat is sometimes so swo that fluids regurgitate through the nose on swallowing, and that in fatal cases death oc from coma or bronchitis.

‡ In addition to the case noted by me, Dr. Duckworth (*Lancet*, 1880 vol. i.) reports in which there was transient albuminuria of probably three days.

1.—Ich hatte bis dahin ausser einigen isolirten Fällen, die ganz ausser Zusammenhang mit jeder Epidemie gestanden waren, und ausser einigen in jenem Aufsatze beschriebenen, welche nur mitten in einer heftigen Masern-Epidemie zur Beobachtung kamen, niemals eine Häufung von Erkrankungen, auf welche die von den Autoren gegebene Charakteristik der Rubeolen gepasst hätte, in einer gewissen Zeit, oder in einer Familie, ja nicht einmal bei 2 Mitgliedern einer Familie neben oder nach einander beobachtet. Ich gab damals ausdrücklich zu, dass dies auf einem Zufall beruhen könne; doch sei es immerhin auffallend, dass nicht nur mir selbst in den vielen Jahren, in denen mir das grosse Materiell des Kinderkranken-Instituts zur Verfügung steht, ein solcher Fall nicht vorgekommen sei, sondern darnach in den Berichten der grossen Wiener Kinderspitäler und der Ambulatorien die Diagnose "Rubeola" vollständig vermisst werde.

2.—Hatte ich bei dem Vergleiche der in den letzten Jahren über Rubeolen erschienenen Abhandlungen aus dem Grunde kein ganz bestimmtes Bild von dieser Krankheit gewinnen können, weil die Angaben der Autoren in einigen wichtigen Punkten stark differirten. So hatte, um nur ein Beispiel anzuführen, kurz zuvor Nymann sich dahin geäussert, die Contagiosität sei bei den Rötheln fast Null, und auch Steiner hatte sie für nicht contagiös erklärt, während alle übrigen Autoren dieselben den contagiösen acuten Exanthemen an die Seite stellten.

3.—Erschien mir eines der hauptsächlichsten Argumente der Vertheidiger der Selbständigkeit der Rötheln, nämlich die häufige Beobachtung, dass kürzlich Durchmaserte, an Rötheln Erkrankte, und Individuen, welche Rötheln überstanden haben, später wieder an Masern erkranken, aus dem Grunde nicht vollkommen beweisend, weil ich gerade in der damals von mir erwähnten Masernepidemie mehrere Fälle beobachtet hatte, in denen dasselbe Individium in ganz kurzen Zwischenräumen zweimal an ächten zweifellosen Masern erkrankt war, und ich es daher für umsoweniger auffallend halten konnte, wenn nach einer abgeschwächten Masernform bei demselben Individium nach einiger Zeit eine heftigere Erkrankung oder umgekehrt beobachtet würde.

Weit entfernt jedoch, etwa die Existenz der Rötheln zu leugnen, sagte ich damals wörtlich:—

"Es lasse sich aus allen diesen sehr genauen und offenbar höchst gewissenhaften Beobachtungen und Schilderungen nicht *mehr* mit Bestimmtheit abstrahiren, als dass es eine, den Masern in allen Punkten ähnliche, aber sowol bezüglich der Höhe des Fiebers und der dadurch bedingten Schwere der Allgemeinerkrankung, als bezüglich der Dauer des Fiebers und des Ausschlages, endlich hauptsächlich bezüglich der Intensität des Exanthems ausserordentlich abgeschwächte Krankheitsform giebt, die manchmal isolirt (d. h. ohne sichtbaren Zusammenhang mit anderen ähnlichen Erkrankungen), manchmal in kleinen Epidemien, in denen alle Fälle gleich leicht verlaufen, vorkommt, dass aber auch, wie Gerhardt ausdrücklich sagt, einzelne Angehörige der Epidemie öfter, manchmal auch die ganze Epidemie den Uebergang in ächte Masernformen zeigen.

Ich muss gestehen, dass mich namentlich der letztere Ausspruch Gerhardt's, eines Vertreters der Selbständigkeit der Rubeolen (Lehrbuch der Kinderkrankheiten, 2. Auflage, S. 76), stutzig gemacht, und mir hauptsächlich jene Bedenken eingeflösst hatte, denen ich in der eben citirten Weise Ausdruck gegeben habe; denn wenn sich wirklich solche Uebergänge in einzelnen Fällen und in ganzen Epidemien zeigen würden, dann wäre es ja eigentlich um die Selbständigkeit der Rubeolen geschehen.

Ich bin jedoch nunmehr in der angenehmen Lage, *aus eigener Beobachtung über eine sehr intensive Röthelnepidemie berichten zu können*, welche etwa im

December vorigen Jahres hier in Wien ihren Anfang nahm, und auch jetzt noch (Ende April) in etwas abgeschwächtem Masse fortdauert, und ich gestehe sofort ein, dass ich meinem jetzt Bedenken zum grössten Teile fallen lassen kann und zwar hauptsächlich aus dem Grunde, weil ich, trotzdem ich im Ganzen 61 Fälle in 23 Familien beobachtet habe, dennoch *nicht ein einziges Mal* [illegible] *zusammen* constatiren konnte.

Sämmtliche diese Beobachtungen stammen aus der Privatpraxis, und ich muss das interessante Faktum constatiren, dass mir trotz der zweifellos sehr bedeutenden Extensität der Roseolaepidemie in dem Ambulatorium des Kinderkrankeninstitutes, welches jährlich von mehr als 3000 kranken Kindern frequentirt wird, noch kein einziger Fall vorgekommen ist. Ich kann dies nicht anders erklären, als dass die armeren Leute wegen der so geringfügigen Veränderung, in dem Verhalten ihrer Kinder das Exanthem entweder gar nicht bemerken oder es nicht für nothwendig halten, ärztlichen Rat einzuholen. Ob diese Erklärung auch für alle Fälle der Beobachtungen in den früheren Jahren und in anderen Wiener Krankenanstalten ausreicht, oder ob diese Krankheit früher in Wien überhaupt noch nicht epidemisch aufgetreten ist, muss ich dahingestellt sein lassen."

Bezüglich der Symptome und des ganzen Verlaufs der Krankheit habe ich in den Angaben der meisten Autoren und speciell der monographischen Darstellung von Immermann in seinem bekannten Handbuche nichts Wesentliches hinzuzufügen. Hervorheben möchte ich noch folgenden Daten Raum geben.

Das *Fieber* war in weitaus der grössten Zahl der Fälle sehr geringfügig; doch wurde eine Steigerung auf 38·5 C. in der Achselhöhle im *Beginne* der Eruption in keinem Falle vermisst, wo eine Messung vorgenommen wurde, ebenso betrug in allen Fällen die Temperaturabendsteigerung 38·6, in einem anderen Falle 39·0 C. an dem Abende des ersten Krankheitstages. In allen Fällen war jedoch am zweiten oder spätestens am dritten Tage die Entfieberung eine vollständige.

Der *Katarrh* der Nase, Augenbindehaut und der Conjunctiva mit lebhafter Röthung und mässiger Schwellung, Reizung in den Augen fehlte in keinem Falle, häufig war auch ein mässiger Husten Reiz zu beobachten. Alle diese Erscheinungen schwanden gewöhnlich am zweiten, spätestens am dritten Krankheitstage vollständig.

In einzelnen Fällen, aber nicht in allen, waren auch leichte *Anschwellungen der Lymphdrüsen* am Halse und besonders [illegible] mit ziemlicher Empfindlichkeit dieser Anschwellungen auftraten. Die Rückbildung derselben erfolgte häufig erst mehrere Tage nach Abfall [illegible] Erscheinungen.

Das *Exanthem* war in den Mehrzahl [illegible] sehr schwaches, sowol was die Intensität der Farbe als die Zahl der [illegible] auch ihre Verbreitung über die Körperoberfläche anbelangte. Am [illegible] standen die Flecken gewöhnlich [illegible] und [illegible] am ersten Tage wie gedunsen erschienen. Als Stadium [illegible] Blüthe [illegible] sehr zahlreiche Flecken [illegible] sehr [illegible] Flecken [illegible] grösser, [illegible] Extremitäten [illegible] über das Niveau [illegible] Weise [illegible] Ausnahmsfälle [illegible] Ausnahmen bedeckt, welche [illegible] Gesicht [illegible] mit [illegible] Rücksicht auf [illegible] Verlauf der Krankheit [illegible] in derselben [illegible] Intensität [illegible] Am dritten Tage [illegible] Tage nach [illegible] Tage [illegible] [illegible] Genesung.

[illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] kranke Kinder

sung, eine deutliche Marmorirung der Haut, insbesondere an den unteren Extremitäten.

Isolirte rote *Flecken an der Schleimhaut des harten Gaumens* beobachtete ich in einer geringen Anzahl von Fällen am ersten Tage der Erkrankung.

Die *Contagiosität* war eine zweifellose und sehr bedeutende, und lässt sich durchaus jener der Masernkrankheit an die Seite stellen. Nur in einer einzigen Familie, in welcher der *Paterfamilias* von aussen her bei einer genau constatirten Begegnung mit einem ambulanten Röthelkranken inficirt worden war, blieben die übrigen Familienglieder, inclusive die Kinder, verschont. In allen übrigen Familien folgte auf die erste Erkrankung nach einer bestimmten Incubationsdauer, welche zwischen 14 und 20 Tagen schwankte, aber innerhalb dieses Spielraums sich viel häufiger der niederen Zahl näherte, *jedesmal* eine Erkrankung eines oder mehrer anderer Familienglieder. Nur ein einziger Fall blieb noch isolirt, wo nämlich ein einziges 4 Monate altes Kind erkrankt war, welches noch niemals die Wohnung verlassen und auch sicher keinen Besuch von einem Rubeolenkranken erhalten hatte. Ich halte es für möglich, dass in diesem Falle *ich selbst* es war, der, ohne selbst erkrankt gewesen zu sein, bei einem früheren Besuche den Krankheitskeim eingeschleppt hatte.

Unter den 64 Fällen betrafen 5 Fälle Erwachsene. Letztere scheinen sich demnach einer gewissen relativen Immunität zu erfreuen. Sehr häufig kam es vor, dass in einer Familie sämmtliche Kinder und nicht eines der erwachsenen Familienglieder erkrankte.

Wenn ich mich nun zu dem zweiten Teile der These, nämlich zu der *Verwandtschaft dieser Krankheitsform mit Scharlach oder Masern* wende, so kann, wenigstens so weit meine eigenen Beobachtungen reichen, und soweit mir die Schilderungen anderer Beobachter bekannt sind, von einer Verwandtschaft mit Scharlach kaum ernsthaft die Rede sein. Es fehlt nicht nur der Charakter des Scharlachexanthems, sondern auch die Neigung zu den, den Scharlach charakterisirenden Erkrankungen der Rachengebilde und der Nieren. Dagegen lässt sich sowol bezüglich des Hautausschlages, als auch insbesondere in Bezug auf die concomitirenden Erscheinungen, eine *sehr bedeutende Aehnlichkeit mit der Masernkrankheit* niemals verkennen; und wenn nun auch der Umstand, dem eine bedeutende Häufung ähnlicher Erkrankungen ohne Uebergang in wirkliche wolcharakterisirte Masern stattgefunden hat, die Annahme einer eigenen Krankheitsspecies gerechtfertigt erscheinen lässt, so wird man andrerseits nicht übersehen dürfen, *dass die Rötheln den Masern viel näher stehen, als jeder anderen uns bekannten Krankheit.* Vielleicht wird sich, wenn man einmal dahin gelangt sein wird, jede dieser beiden Krankheiten auf einen bestimmten Mikroorganismus als Krankheitserreger zurückzuführen, der Schlüssel zu dieser Aehnlichkeit in einer nahen Verwandtschaft dieser beiden Organismen finden lassen. Man wird möglicher Weise constatiren können, dass die eine Species sich allgemach aus der anderen entwickelt und sich endlich so weit von ihrer ursprünglichen Stammform entfernt hat, dass sie nunmehr selbst unter günstigen Bedingungen im menschlichen Körper nicht mehr die ursprüngliche, sondern immer wieder nur die modificirte Krankheitsform hervorbringen kann, und dass die eine Krankheitsform nicht einmal mehr befähigt ist, die Immunität gegen die andere zu gewähren.

Es scheint mir zwischen den Masern und den Rötheln ein ähnliches Verhältniss zu bestehen, wie zwischen *Variola und Varicella.* Auch hier auf der einen Seite eine geradezu frappante Aehnlichkeit in der Form und noch mehr in der Anordnung der Eruptionen auf der Haut und den Schleimhäuten; dabei die concomitirenden Erscheinungen bei der Varicella in hohem Grade abgeschwächt, aber manchmal noch teilweise erkennbar;—auf der andern Seite aber

keine gegenseitige Immunitätswirkung zwischen Variola—Vaccine und zwischen Varicella, und endlich, wenigstens in der überwiegenden Mehrzahl aller Beobachtungen, auch keine Uebertragung der Varicella als Variola auf ein anderes Individium oder umgekehrt. Allein gerade in Bezug auf den letzteren Punkt gebieten mir meine eigenen Erfahrungen eine gewisse Vorsicht. Ich habe nämlich Beobachtungen verzeichnet (*Jahrbuch für Kinderheilkunde*, VI. Band, 1871), in welchen einmal von 2 Geschwistern das eine an Varicella, und nach Verlauf von 12 Tagen das andere ungeimpfte an wolcharakterisirten Variolen erkrankte, und in einem zweiten Falle auf die Varicellenerkrankung eines Kindes nach 14 und 16 Tagen eine zweite Varicellenerkrankung des Bruders und eine Variolenerkrankung der Mutter gefolgt waren. Auch andere Beobachter haben vereinzelte ähnliche Beobachtungen verzeichnet. Dem gegenüber will ich keineswegs in Abrede stellen, dass auch ich in der weitaus überwiegenden Anzahl meiner Beobachtungen aus Varicella wieder nur Varicella, auch bei nicht geimpften Kindern, habe entstehen gesehen, und ich stehe daher nicht an, auch für die Varicella einen gewissen sehr bedeutenden Grad von Selbständigkeit gegenüber der Variola zu zugestehen; aber ich glaube dennoch, dass man die Acten über diese Frage noch nicht schliessen darf. Auch für die Rubeolen kann ich ja nur mit Rücksicht auf die hier mitgeteilte, immerhin doch beschränkte Zahl von Beobachtungen constatiren, dass *ich* eine Uebertragung von Rubeola auf ein anderes Individium in der Form von echten Masern nicht gesehen habe. Dass aber eine solche Uebertragung niemals stattfindet, möchte ich, insbesondere mit Rücksicht auf die oben citirte Aeusserung von Gerhardt noch nicht behaupten.

Es mag also vielleicht für die klinische Seite der Frage von Wert sein, die Trennung ganz absolut hinzustellen; vom Standpunkte des Naturforschers möge man aber den Faden zwischen Rubeola und Morbilli, und zwischen Variola und Varicella vorläufig noch nicht durchschneiden.

Contributions to the Study of Rötheln.

Dr. J. Lewis Smith, New York.

At the close of the epidemic of rötheln which visited New York in 1873–74, I read before one of the medical societies, and afterwards published, statistics which I had collected, together with such observations as seemed to me useful. The paper then prepared is made the basis of the following communication to the Pædiatric Section of the International Congress, with such emendations and additions as I thought proper to make after having recently witnessed another and very similar epidemic.

The disease known as rötheln has heretofore been rare in America. In the Eastern continent, on the other hand, it appears to have been known for many years, and American physicians frequently designate it German or French measles. Meagre and imperfect descriptions of this malady have appeared in some of the British journals, and cases, quite fully detailed, have been published by British physicians.

Rötheln is not entirely a new disease in this country, though most American physicians never saw a case of it until within the last decade. Cases occurring in and about Boston were described by Dr. Homans, sen., in 1845; and at a later date, namely, in 1853 and 1871, B. E. Cotting, M.D., Harvard, saw cases and described them in papers read before local societies (see *Boston Medical and Surgical Journal*, March 15, 1873). In 1874, Dr. Caleb Green of Homer,

Cortland County, New York, an accurate and intelligent observer, also witnessed an epidemic.

This hitherto rare and interesting malady occurred in New York city as an epidemic in 1873 and 1874, attaining its maximum prevalence in March and April of the latter year, after which it declined, occasional cases occurring throughout May. This, so far as I can learn, was the first occurrence of rötheln in this locality. In a general practice of more than twenty years, extending over a considerable portion of this city, I had previously seen nothing like it; and other older physicians, having a large general practice, have informed me that they consider it an entirely new disease with us. Those who believed that they had occasionally observed isolated cases of it previous to the epidemic probably referred to roseola.

The first case which I met with occurred in the middle of December, 1873, in West Seventy-first Street, in the northern suburbs of this city. A few weeks later cases were so numerous in the more thickly populated sections of New York as to attract the attention of many physicians. It was evident that a disease had appeared with which we were not familiar, and as the eruption occurred in points and small circumscribed patches, it was usually designated by the physician, in want of a more accurate name, epidemic roseola, or was spoken of as a spurious measles. Those physicians who were familiar with foreign medical literature saw the resemblance between these cases and those of rötheln as described by British and Continental writers; but in certain, at least, of the foreign cases the duration of the rash was said to be seven days (Liveing, London *Lancet*, March 14, 1874, and *Medical News and Library*, May, 1874), whereas, in the cases in New York, it commonly disappeared by the fourth day. This discrepancy, however, was not sufficient to invalidate the belief in the identity of the New York disease with the foreign rötheln. It was readily explained by the difference in the seasons in which the cases occurred, for Liveing observed his cases in June and July, and, as we shall see, the greater the external heat the longer the duration of the eruption.

Between the middle of December, 1873, and May 1, 1874, I had observed and treated this malady in eighteen families. Cases occurred in three other families living in the same houses with some of those which I attended, and as they were fully and clearly described to me, so that there could be no doubt as to their nature, I have included them in my statistics. The total number of cases in these twenty-one families was forty-eight. During May, when the epidemic was declining, I saw six additional cases, occurring singly, making a total of fifty-four. Their ages are given in the following table—

AGE.	CASES.
From 8 months to 1 year	2
„ 1 year to 2 years	4
„ 2 years to 5 „	16
„ 5 „ 10 „	23
„ 10 „ 15 „	3
„ 15 „ 30 „	6
Total number of cases	54

The age of the youngest patient was eight months, and that of the oldest thirty years. Seventy-two per cent. of the total number were between the ages of two and ten years, so that rötheln is pre-eminently a disease of childhood. Individuals in and beyond the middle period of life seem to have nearly an immunity from it. The age of the oldest patient of whom I was informed, in the epidemic of 1872 and 1873, was about forty years.

On March 25, 1873, during my attendance in the New York Foundling Asylum, rötheln appeared in a boy of four years. In the following month about thirty more cases occurred in this institution, all children, while among the large number of female nurses and employees, who were chiefly between the ages of twenty and thirty years, all but three escaped.

From 1874 to 1880 rötheln did not occur in New York, unless now and then an isolated or sporadic case, the nature of which was not recognised, and which was supposed to be roseola. On August 9, 1880, two cases appeared in different wards of the New York Foundling Asylum, when it was remembered that two weeks previously these children had been exposed to a patient in the hospital attached to the institution who had what the physician in attendance supposed at the time to be roseola.

Commencing with these two cases, an epidemic occurred in the Asylum, mild in type, affecting only a few at a time, but extending over several months, until about sixty inmates, chiefly children, were attacked.

Toward the close of 1880, rötheln began to appear in the northern part of the city, in which the Asylum is located, and over which my practice extends. Its maximum prevalence was attained in the latter part of March and April 1881, when it particularly attracted the attention of physicians. A large proportion of the children attending certain public and private schools were attacked. It occurred in seventeen families in my practice, in which there were forty-two cases. Their ages are given in the following table:—

AGE.	CASES.
From 1 to 2 years.	3
„ 2 „ 5 „	8
„ 5 „ 10 „	18
„ 10 „ 15 „	11
There were 2 cases over 15 years, aged respectively 22 and 42 years	2
Total number of cases	42

Premonitory Stage.—Premonitory symptoms are, in most instances, absent, or so mild as to attract but little attention. It not infrequently happened in the New York epidemics that the parents or the teachers in the schools were first made aware of the illness of the children by observing the eruption. In some instances children were sent from school, not because they felt too ill to remain, but on account of the unusual appearance of the skin. Sometimes, however, in those old enough to express their sensations, a premonitory stage of some hours or a day, or even of longer duration, was present, consisting of such symptoms as usually occur when one has taken a severe cold, as languor, pain in the head, trunk, or limbs. The resident physician of the New York Foundling Asylum was so ill with rötheln that he was confined to his bed during the first day of the disease. Now and then patients experience nausea previous to the eruption, and in the first and second days of the eruptive stage. In only one instance did I observe grave prodromic symptoms. A boy aged eight years was suddenly seized with clonic convulsions; and while in a warm bath for the relief of these, the rash appeared upon those parts of his body which were immersed in the water.

Symptoms.—Tegumentary System.—(*a.*) *The Skin.*—The eruption commonly commences upon the forehead, around the ears, and along the neck, as in measles. Occasionally it may appear first upon the back or chest, as in the above-mentioned case, in which the hot water accelerated its appearance. Commencing above, the efflorescence travels downward, appearing after some hours

upon the lower part of the trunk and on the legs, resembling in this respect that of scarlet fever and measles. It occurs on all parts of the integument except the scalp and palmar and plantar surfaces. In the majority of the cases which I have seen it gradually faded away, disappearing by the fourth day; but on children who were kept warm in bed or in warm apartments it remained longer than on others. In many instances traces of the rash were still visible several days after recovery, when the patients were heated by exercise or excitement. It reappeared at times, though indistinctly, on a girl of thirteen years for three weeks. In most of the cases in the New York epidemics, the eruption commonly occurred in points and circular spots somewhat smaller than those of measles. These points and spots were numerous and thickly set, covering at times at least half, often more, of the surface, while between them the skin presented nearly or quite its normal appearance. The general aspect in most cases was more like that of measles than that of scarlatina; but in exceptional instances the skin between the points and spots had a redness similar to that of erythema, and the resemblance was very like the scarlatinous efflorescence. Thus, in a boy of three years, the eruption so closely resembled the scarlatinous over the trunk, that, were it not that the temperature was constantly below 100°, and all febrile movement had ceased within three or four days, I would probably have considered the malady a mild scarlatina. In certain patients the eruption, beginning in circumscribed spots like that of measles, becomes in two or three days confluent, so as to resemble that of scarlatina, while over other parts the spots remain discrete. This was the character of the eruption upon the third and fourth days on the extremities of a little boy in the Foundling Asylum. The rash is attended by considerable itching, from which, indeed, many patients suffer more than from all other symptoms. The eruption disappears on pressure, produces a slight roughness of the surface, as ascertained by passing the fingers gently over it, and usually fades away without desquamation. Exceptionally there is a slight branny exfoliation, and in one of my patients this was as considerable over the abdomen as in cases of scarlatina.

(*b.*) *The Mucous Membrane.*—In connection with the cutaneous eruption, a mild inflammation also occurs upon the mucous membrane covering the fauces, buccal cavity, and nostrils, and upon reflections of this membrane over the eyes and eyelids—*i.e.*, upon the conjunctivæ. In certain patients this inflammation is scarcely appreciable, but in the majority it arrests attention at once. It produces a suffused, reddish, or weak appearance of the eyes, with a moderately increased lachrymation. On everting the eyelids, the palpebral conjunctivæ are seen to be injected. In certain patients a moderate puriform secretion collects at the inner angle of the eyelids. In occasional cases the conjunctivitis causes œdema of the lids, usually slight, and likely to be overlooked by the physician; but in three instances, which I now recall to mind, the mothers of the children directed my attention to the swollen state of the lids. In one of these, an infant of twenty-three months, the tumefaction was so great, commencing about the time the eruption began to fade, that light was totally excluded from the eyes, and it was impossible to ascertain their condition. The skin over the eyelids retained nearly its normal appearance, and a puriform secretion appeared between the lids. In three or four days the œdema of the lids and the hyperæmia of the conjunctivæ rapidly declined. The coryza is, in most cases, sufficient to cause an unpleasant sensation in the nostrils and provoke sneezing; but the flow from the nostrils, though present, was, in no instance under my observation, as abundant as in ordinary cases of scarlatina, or even of measles. The fauces present an injected appearance, and in severe cases there is moderate swelling of the tonsils. The same catarrhal hyperæmia is also seen in spots or patches, more or less diffused upon the buccal surface. Both the faucial and buccal

catarrh are less in degree, however, than in cases of measles and scarlat having an equal intensity of cutaneous eruption; and this fact has aided m differential diagnosis.

The Respiratory System.—In both the epidemics which I have witnes the mucous membrane of the larynx, trachea, and bronchial tubes particip only slightly in the inflammation, which involved the nasal, buccal, and fau surfaces. Many of my patients had no cough, but others had a mild cough ing for a few days, but with normal respiration. It was, apparently, due very mild catarrh of the respiratory tract at the time when the nasal and conjunctival surfaces were the most affected. It subsided in a few days with treatment. In no case do I recollect that there was any hoarseness.

The Digestive System.—The tongue in rötheln is moist and of normal app ance, or covered by a slight fur. The appetite may be impaired, but is wanting in uncomplicated cases. The parents sometimes say that it is ne the same as in health. The thirst is slight and the bowels are regular. Naus not infrequent, and vomiting was, in several cases in my practice, one of initial symptoms. In certain patients it also occurred on the first or second of the eruption. In others there was no nausea, as far as I could learn, ei immediately before or during the prevalence of the disease. This system is frequent in rötheln than in scarlet fever, but is as common, apparently, a measles. I have never found albumen in the urine, though I have exam that passed by several patients. This secretion did not appear to be abno except that it contained urates, so common in febrile states.

The Pulse and Temperature.—The largest number of accurate daily ob vations relating to the temperature was, I think, that of Dr. Reid in the York Foundling Asylum during the month of March, 1874. He has kindly nished me with his statistics relating to this symptom as follows:—"I number of closely observed cases in which the temperature was taken twenty-four. In seventeen of the cases the temperature ranged from 97° to in six it reached 100°, 100¼°, and 100¾°; in one it reached 103¼° on the se day of the eruption, but remained so elevated only one day." In ce patients Dr. Reid observed what he designates "a tendency to the develop of an ephemeral fever." These observations correspond closely with t made by myself during the same epidemic. Thus, in sixteen cases, I found axillary temperature, taken each day, to be constantly between 98° and 1 with a pulse under 110°, except in one case, in which it numbered 124°. certain other patients a more decided febrile movement, lasting from one to or three days, occurred, usually in the commencement of the malady. Th girl, aged three and a half years, had a temperature of 101¾° and a puls 128. In another instance the pulse was 124 and the temperature 102. another, a girl of three and a half years, there was active febrile moven occurring without apparent cause, on Saturday night, but abating on the lowing day. She seemed well until the following Tuesday, when the fe movement returned and the eruption appeared. On Thursday the tempera from 102° to 103° fell to 99½°, and within a day or two she was convales In two other patients, from two to four days after the disappearance of the tion, an accession of fever occurred, lasting about one day, and attended by and distress in the epigastric region, but without vomiting or diarrhœa. one of these the temperature was 103¼°, and the pulse 130 per minute. I other case the temperature and pulse did not seem to be under these fig but were not accurately ascertained. Occasionally the febrile movement is more to complications than to the primary disease. Thus, in two of my pat the febrile movement was mainly attributable to diphtheritic inflamma which had attacked the fauces. But while the febrile movement in röthe

ordinarily of short duration, in certain patients temporary exacerbations may occur, in which the temperature is as high as in scarlet fever or measles.

Complications, Prognosis.—The only complication which occurred in cases in my practice has already been alluded to, namely, diphtheria, which, when prevalent, is apt to attack surfaces already inflamed. In the Foundling Asylum, varicella complicated one case, and pneumonia another; and in a third, pneumonia occurred about three days after the disappearance of the eruption. The prognosis in uncomplicated rötheln is always very favourable, and there is no more liability to sequelæ than in mild catarrhal inflammations of a non-specific character. The duration of rötheln is short, not ordinarily extending beyond three to five days.

Nature, Incubative Period, Contagiousness.—Is rötheln a distinct malady or one with which we are familiar, but the form and character of which are modified by unusual meteorological conditions? Is it roseola, assuming at certain periods an epidemic character, and appearing to be contagious? Or is it at all times infectious, possessing a specific principle, and, like other infectious diseases, self-propagating? Should it, in nosological classification, be placed among the non-contagious and local, or among the constitutional and infectious maladies? Let us consider the facts observed in the New York epidemics.

The first cases of rötheln in this city were often designated roseola by the physicians called to treat them, since they seemed to resemble more closely this disease than any other with which they were familiar. Rötheln, however, differs widely from the peculiar form of dermatitis known as roseola. The successive occurrence of the eruption over the upper and then the lower parts of the body, but covering the whole surface, and the definite duration of from three to five days, are points of difference. Moreover, roseola would not, without so great a change in its character as to become virtually a distinct disease, occur in the cool months without any appreciable dietetic cause as an epidemic over a certain area, and for a limited time, affecting whole households of children and sparing other households as well as individuals of a certain age. We therefore consider it distinct from roseola.

Most of the cases in the New York epidemics bore considerable resemblance to measles, both as regards the appearance and duration of the eruption and the catarrh of the mucous surfaces. Parents often diagnosticated measles before the arrival of the physician, and the physician himself, at first glance, sometimes made the same diagnosis. But in rötheln, the shortness and mildness of the stage of invasion, the absence of cough, or the presence of one trivial and scarcely noticed, appetite good or but slightly impaired; in fine, symptoms that are transient or slight, afford a striking contrast with the graver symptoms of measles. But the decisive proof that rötheln is not a modified measles is found in the fact that one does not prevent the other. Of the forty-eight cases observed by myself prior to May 1 in the epidemic of 1874, nineteen at least had had measles, and one who had rötheln took measles subsequently. I have already stated that in the New York Foundling Asylum rötheln, in 1873 and 1874, closely followed an epidemic of measles. A considerable number of the children attacked by the former disease had recently recovered from the latter. During the epidemic of 1880 and 1881 the same fact was observed, namely, that a previous attack of measles as well as scarlet fever afforded no protection from rötheln. Dr. Chadbourne, the resident physician, writes of the cases in the Foundling Asylum in 1880 and 1881:—"Eight children had rötheln who had had both scarlet fever and measles within six months under my observation, while certain others had had these diseases at some previous time." Of the cases observed by myself in family practice in the same epidemic, it is

stated in my notes that ten had had measles. These statistics are sufficient to show that rötheln is a distinct disease from measles, however close the kinship.

That rötheln is not a form of scarlet fever is evident from the fact that, as regards at least the New York epidemics, the rash was in most instances quite different from the scarlatinous efflorescence, occurring, as we have seen, in small more or less circular points and patches. Moreover, as we have remarked above, there is in rötheln a slight febrile movement and general mildness of symptoms which contrast with the high fever and other pronounced symptoms of scarlatina, or if there be considerable febrile movement its duration is brief. But the conclusive proof of an essential difference between these two diseases is found in the fact already stated in reference to measles, that the attack of the one malady does not prevent the occurrence of the other. There are, it is true, cases in which it is difficult at first to make the differential diagnosis between rötheln and mild measles or mild scarlet fever, but when the course of the malady has been closely observed for three or four days, it will rarely happen, I think, that we will be unable to make out its character.

Those cases of an epidemic which arise when the causes or conditions from which it is developed are most strongly operative, and which at this time are apt to be typical, obviously afford the best data for studying its nature. Such were the forty-eight cases which I saw in the epidemic of 1873 and 1874, and the forty-two in that of 1880 and 1881. As regards the former epidemic, in thirteen of the twenty-one families embraced in my statistics, the first cases were children who up to the time of the seizure were attending public or private schools, and in certain instances those who were nearly simultaneously attacked, living perhaps in streets widely separated, were attending the same school. During the epidemic of 1880 and 1881 the first patients in thirteen of the eighteen families in which rötheln occurred were school children between the ages of six and twelve years, and in most, if not all, the different schools which they attended, rötheln was at the time prevailing as an epidemic, as I ascertained on inquiry. It therefore seemed probable that these had contracted it from others.

In both the New York epidemics during the time that rötheln was at its maximum prevalence, in most of the families containing two or more children the cases were multiple, not occurring simultaneously, but in succession, as if the malady were contracted from those first affected. This is what we daily witness in the spread of exanthematic fevers. Thus in Mr. E.'s family, a girl attending one of the public schools took rötheln in the middle of December, 1873; the two remaining children sickened with it one week and two weeks later. A niece visiting in the family at the time when the first child was sick, but returning home to another street, also had the eruption on December 27th. Alice R., aged ten years, a frequent visitor at Mr. E.'s, living in the same street, and several times exposed to his children during their illness, also took rötheln about January 4th. West Seventy-first Street, where these cases occurred, is thinly settled and suburban, and I could learn of no other cases in the vicinity. A child of Mr. P., aged five and a half years, had been in the habit of playing with two children two doors away who became affected with rötheln in the beginning of April 1881. On April 14th he was supposed to have a mild coryza from taking cold, as he sneezed often, but in a few hours the efflorescence appeared. Four days subsequently, on the 18th, an infant was affected in the same way, and thirteen days later another child in the family, aged twelve years. In a similar manner rötheln occurred in the families of two brothers, living in adjoining houses in West Fifty-first Street. The first patient was a boy of twelve years. It appeared successively in the children of these two families until ten had been affected. In a family in West Forty-sixth Street,

the first case was a boy attending a school in which rötheln was prevalent. Within twenty days, namely, between March 31st and April 20th, four other children were attacked in succession.

These facts and cases seem to demonstrate the contagiousness of rötheln, at least during the time in which the conditions are most favourable for its development, or during the time in which the epidemic influence is most pronounced. In the declining period of both the New York epidemics, the cases which I observed occurred for the most part singly, although there was no attempt to isolate the patients; so the contagious character, if present, must have been very slight.

Rötheln is, in my opinion, an exanthematic fever, mildly contagious. It resembles varicella in general mildness of symptoms, in the absence of dangerous complications or sequelæ, and in the uniformly favourable prognosis, while its symptoms and history show a resemblance to measles and scarlet fever.

If the above view be correct, rötheln must possess an incubative period, which, in the cases observed in both epidemics, apparently varied between seven, or perhaps less than seven, and twenty-one days. Its incubation, therefore, resembles that of scarlet fever, which, as is well known, varies in different patients. In the cases which came under my notice the incubative period, when it could be accurately ascertained, was more frequently about two weeks than a longer or shorter period. The resident physician of the New York Foundling Asylum, when the epidemic was prevailing in that institution, returned to his home in the State of Maine, to a locality where rötheln was unknown. Fourteen days from the date of his departure he was himself affected with the disease in its typical form. No other case occurred at his home, where probably the atmospheric conditions were unfavourable. Minnie B., attending a school in which there were many cases, had the rash on April 5th. On the 23rd of the same month, eighteen days afterwards, it appeared upon the servant who was frequently in Minnie's room. Elizabeth C., attending a school in which rötheln was prevailing, had the eruption on April 17th. It commenced upon her sister thirteen days, and her mother fourteen days subsequently. Other cases might be cited of an apparently shorter as well as longer incubative period. The following note from Dr. Chadbourne, of the New York Foundling Asylum, bearing upon this subject, is interesting:—"I am led to believe, from my observations, that the period of incubation was in the majority of the cases from twelve to fifteen days. The disease has been very feebly contagious. In some cases one child would have rötheln, while the other, nursed by the same woman, would escape. In two instances nursing women had the disease, and though each suckled two infants, the latter escaped."

Rötheln requires no treatment.

The Real Position of the so-called Rubeola, Rötheln, or German Measles, and its Relation to Scarlatina and Measles, as Illustrated in the History of Thirty-one Observed Cases.

Dr. G. E. Shuttleworth, Lancaster.

Having, in my position as medical superintendent of a large institution for imbecile children, had the opportunity of closely observing over thirty cases of the so-called German measles, and of following the history of the patients,

both previous and subsequent to the attack, I venture to offer my experie as a slight contribution to the discussion of the subject.

My first series of twenty-seven cases occurred in the summer of 1874, was briefly described by me at the British Medical Association meeting in following year. My second series of four cases occurred during 1880. I tr I need not apologize for reverting to what, as regards the first series, may designated old material, inasmuch as the value of my observations in 1874, connection with the present discussion, is much enhanced by the knowledg the subsequent history of the patients.

To proceed then to a sketch of the leading features of the epidemic of 18 I may say, in the first place, that the Royal Albert Asylum (which then contai over two hundred children) had, from its opening in 1870, been up to that d entirely free from epidemic disease. The first case of rötheln which came un observation arose in the person of a nurse, who on the evening of June 15t very hot day) consulted me with regard to a rash which had just appeared the neck and face, consisting of a few brownish-red elevated papules isolated f each other by clear skin. The fauces were slightly reddened. The tempera at 9 P.M. was barely 99°, and the pulse but 84. There was no constitutio disturbance, but as a matter of precaution the girl was at once isolated. aperient was given. Next morning at 9 A.M., the temperature had risen to 101 and the pulse to 116; there was slight swelling of the tonsils, and a appearance of the tongue at tip and edges. The constitutional disturbance still trivial, and as the menstrual period was approaching, it was thought possibly the symptoms might have some connection therewith. The rash, of the same hue and character, had extended over the body and legs. On morning of the third day the temperature had fallen to 99°, the pulse to 84, the rash was fading from the face and neck. On the fourth day the pulse the temperature were normal and the rash had well-nigh disappeared. nurse was kept isolated for ten days and then allowed to visit her friends in country for a week, returning to her duties at the institution on July 3r having been previously ascertained that she was entirely free from desqua tion. On the 7th July, four days subsequent to the nurse's return, an imb girl of twelve (one of the nurse's charge) displayed a rash and other sympt in all respects resembling those described in the first case, except that the in pulse and temperature were higher (103° and 120° respectively), falling day to 100·5° and 112°, on the third day to 99·5° and 108°, and on the fo becoming normal. In this case there was more noticeable injection of fauces and swelling of the tonsils, and there was something of the "strawbe character of the tongue. The rash in colour and arrangement was more that of measles than of scarlatina; the throat symptoms and tongue more those of the latter. On the 8th and 9th July, the days succeeding tha which the girl's rash appeared, two fresh cases occurred, these patients belon also to the class of which the nurse was in charge. In both the rash was sl and in the second case there was scarcely any redness of fauces. Isolation all along resorted to, and from the 9th to the 18th July there were no cases. On the 18th a boy, from a different department, but attending sc with the girls, was attacked; and the next nine days furnished seven cases. These were all extremely mild, the temperature not rising above 1 and the rash not lasting for more than three days. Eight days elapsed three more children were affected; and after two more similar intervals the of twenty-seven was made up, the last case occurring on August 31st. The two cases were notable as displaying certain marked differences in the ch ters of the rash, which had indeed to a less degree been observed throug the series. Case 26 presented a diffuse and bright rash resembling tha

scarlatina, but there were no throat symptoms beyond redness of the velum palati, and the maximum temperature (that of the first day) was 100°. In Case 27, on the other hand, the rash, both in colour and arrangement, simulated measles, and there was considerable conjunctival injection, though but little lachrymation. In this case the initial temperature, which was the maximum, never exceeded 100°, and it fell to 99° on the third day.

I trust that the brief description just given, together with the table which I present, will suffice to give a general idea of the epidemic of 1874. I may add that the diagnosis of the cases caused me no little anxiety, the more so as medical men who saw only individual cases were inclined in some to diagnose measles, and in others scarlatina. Looking, however, at the whole series, I could not accept these diagnoses, and was greatly relieved to find that the descriptions given by Babington, Squire, Liveing, Tilbury Fox, Vogel, Von Ziemssen, and others of rötheln tallied with what I had observed. My diagnosis was confirmed by the absence in every case of any serious sequelæ; there was no albuminuria; and desquamation, when it occurred in two cases, was of the slightest furfuraceous character. I may perhaps be allowed to quote from the brief notice published in the *British Medical Journal*,* in August, 1875, my conclusions as to the pathology of the affection. "There was no doubt that the twenty-seven cases bore a homogeneous relation to each other; they were not mere summer roseola, for the weather was even cold at one period of the epidemic. They were not cases of *scarlatina*, for in no case were there the full characteristic signs or sequelæ of this disease; they were not measles simply, for there was an almost universal absence of catarrhal symptoms, and more than half the patients had previously had measles; and there was no reason to suspect that the rash depended upon any error of diet or hygiene. It was argued (both from negative and positive characteristics) that the cases were best described under the name of *rubeola notha;* and it was urged that the diagnosis of the affection, as entirely distinct on the one hand from scarlatina, and on the other from measles, might often be a matter of great moment to the practitioner."

It is noteworthy that of the twenty-seven patients in this series, fifteen were recorded to have had measles and four also scarlatina. The sequel enables me to supplement this statement by more recent experience with the same patients.

In 1877 the Asylum was visited by an epidemic of scarlatina, extending to fifty-six cases, and in the early part of 1880 by measles, extending to seventy-seven cases, the last occurring on March 23rd. In the summer of the same year occurred a series of six cases of scarlatina, the first occurring on the 25th May, the last in July.

Whilst these cases of scarlatina, of the nature of which there could be no doubt, were going on, four cases of anomalous rash occurred. On the 23rd June, a boy (D. B.), from an entirely different department from that in which the scarlatina had arisen, displayed a papular eruption on the face and chest, somewhat resembling measles, but the papules were discrete and not arranged in crescentic patches. In the morning (at 8 A.M.), when these symptoms were observed, the temperature and pulse-rate were normal; there was very slight conjunctival injection, and no coryza. In the course of the day the rash extended all over the body, and its resemblance to that of measles became greater. The temperature at 7 P.M. was 100°; at midnight 101·5°. There was little constitutional disturbance, and the appetite was as good as usual. On the second morning the temperature was 100°, the rash still out; on the third, the rash had entirely disappeared, and the temperature was nearly normal. This boy was reported to have had both scarlatina and measles in infancy. On the

* *British Medical Journal*, No. 764, p. 229.

CASES OF "RUBEOLA NOTHA," OBSERVED AT ROYAL ALBERT ASYLU

No.	Initials of Patient and Sex.	Age.	Date of Appearance and Duration of Rash.	CHARACTER OF RASH.	State of Fauces and Tongue.	Range of Temperature and Pulse. Days of Rash. I.	II.	III.	IV.	V.
	SERIES I.		1874.							
1	I. A. ... (F)	20	June 15 to 18	Brownish-red elevated discrete papules; first on neck and face, then over body generally	Fauces and Tonsils slightly red and swollen. Tongue raw at tip and edges	T 99 P 84	101·8 116	99 88	99·1 84	98 84
2	M. W. ... (F)	12	July 7 to 10	Resembling above	Similar to above. Tongue somewhat "strawberry-looking"	T 103 P 120	100·5 112	99·5 108	98·4 100	...
3	A. B. ... (F)	17	July 8 to 11	Very slight rash on face. Some injection of conjunctiva. Diffuse redness of buttocks: elsewhere rash discrete	One Tonsil enlarged. Tongue "strawberry-like"	T 99 P 84	98·5	98	...	...
4	A. F. ... (M)	10	July 9 to 10	Very slight rash on chest and face: more about buttocks	Scarcely any redness	Norm.	Norm.	Norm.	...	...
5	J. T. S. (M)	9	July 18 to 20	Slight intermittent papulated rash	Slight redness	T 99 P 96	98·4	...	...	...
6	W. J. ... (M)	10	July 21 to 24	Considerable redness of face, somewhat diffuse, but rash discrete on body. Slight conjunctival injection	Some redness and enlargement of Tonsils	T 99 P 96	99 96	98·4 84	...	...
7	J. T. ... (M)	8	July 22 to 24	Slight discrete rash	Slight redness	Norm.	Norm.	Norm.	...	...
8	J. D. ... (F)	16	July 23 to 27	Brownish-red papular discrete rash all over body	Some redness	T 99 P 96	99 96	98·6 96	98·4 84	...
9	H. G. ... (M)	13	July 23 to 26	Very slight rash. Injected conjunctiva	Scarcely any redness	T 100 P 96	99 96	99·5 96	99 90	99
10	J. B. F. (M)	11	July 23 to 25	Very slight rash on face and chest	Very slight redness	Norm.	Norm.	Norm.	...	...
11	H. W. ... (M)	10	July 23 to 25	Do. do.	Do.	T 99	Norm.	Norm.	...	...
12	F. R. ... (M)	15	July 23 to 25	Do. do.	Do.	T 99	Norm.	Norm.	...	...
13	J. T. ... (M)	6	July 24 to 26	Do. do.	Do.	T 98·9	Norm.	Norm.	...	...
14	J. F. ... (M)	15	July 24 to 26	Do. do.	Do.	Norm.	Norm.	Norm.	Norm.	Nor
15	G. B. ... (M)	8	July 25 to 27	Do. do.	Do.	Norm.	Norm.	Norm.	...	...
16	F. W. ... (M)	10	July 25 to 27	Do. do.	Do.	T 99	Norm.	Norm.	Norm.	...
17	C. B. ... (M)	11	July 26 to 29	Do. do.	Do.	T 99·5	Norm.	Norm.	Norm.	...
18	S. L. ... (F)	9	July 26 to 29	Slight rash: most on face ...	Red Uvula	T 98·6	Norm.	Norm.	...	...
19	J. P. ... (M)	12	July 27 to 29	Very slight rash on face and chest	Very slight redness	T 98·6	Norm.	Norm.	...	...
20	J. S. ... (M)	10	Aug. 5 to 7	Diffuse red rash on face. Papules on body	Slight redness	T 100·5 P 100	99·5 96	98·4 96	... 84	...
21	E. P. ... (M)	8	Aug. 9 to 12	Like No. 6, above described	Very slight redness	T 99·5 P 90	98·4	...	...	...
22	R. R. ... (F)	7	Aug. 9 to 12	Slight rash	No affection	Norm.	Norm.	Norm.	Norm.	Nor
23	L. D. ... (F)	6	Aug. 16 to 19	Slight rash	No affection	T 99	Norm.	Norm.	...	...
24	I. B. ... (M)	21	Aug. 16 to 19	Well-marked rash, like No. 1	Slight redness	T 99	Norm.	Norm.	...	...
25	C. B. ... (M)	30	Aug. 20 to 23	Slight: limited to chest and back	No redness	T 98·8	Norm.	Norm.	...	...
26	W. McA. (M)	14	Aug. 25 to 28	More diffuse and brighter rash than the others	Redness of Velum Palati. Tongue not "strawberry-like"	T 100· P 108	99·8 96	98·4	...	...
27	M. S. ... (F)	20	Aug. 31 to Sept. 4	Considerable conjunctival injection: somewhat crescentic arrangement of papules, resembling rash of measles	Considerable injection of Velum Palati: some redness of Tonsils	T 100 T 96	100 96	99 84	98·8 84	98 84
	SERIES II.		1880.							
	D. R. ... (M)	12	June 23 to 26	Papules discrete: brownish-red rash universal	Palate and Tonsils injected	T 98·8 100 101·5 P 100	100 96	98·8	Norm.	...
	M. H. (M)	14	June 27 to 30	Similar to preceding, but brighter	As above	T 101·4 P 100	T 100·5 87	99·4 86	Norm.	...
	H. ... (M)	13	June 28 to July 1	Similar to preceding, but less extensive	Slight injection of Fauces	T 100 P 96	99·8 90	98·4 84	...	...
	(F)	15	July 5 to 8	Brownish-red rash, first seen on face and neck, afterwards extending to limbs	... ...	T 101·2 P 100	100·6 98	98·6 90	Norm.	...

LANCASTER, IN JUNE, JULY, AND AUGUST, 1874, AND IN JUNE AND JULY, 1880.

Condition as to Desquamation.	Condition as to Sequelæ, Albuminuria, &c.	Duration of Isolation.	Possible Source of Infection.	Exanthems previously undergone.	OTHER PARTICULARS.	Sequel of History.	No.
None, though carefully searched for	None	June 15 to July 2	?	Measles	Was completely isolated from June 15 to June 24. Sent home into country from June 24 to July 2, and slept with younger sister: no spread of disease there	Left Institution 1875	1
None	None	July 7 to Aug. 31	No. 1	None	... ...	Died 1877	2
None	None	July 8 to Aug. 31	No. 1	Doubtful	... ...	Had { Scarlatina, 1877 / Measles, 1880	3
None	None	July 9 to Aug. 31	No. 1	Measles	(Some little difficulty in thermometry in this case)	Still in Institution	4
None	None	July 18 to Aug. 31	? No. 4 (Meets at School)	Measles	... ...	Had Scarlatina, 1877; Left Institution Jan. 1880	5
None	None	July 21 to Aug. 31	No. 4	Measles and Scarlatina	Medical man confirms friends' statement of his having had Scarlatina about three years before	Left Institution in 1879	6
None	None	July 22 to Aug. 31	No. 4	None	... ...	Died 1879	7
None	None	July 23 to Aug. 31	No. 4	Doubtful	... ...	Died 1880	8
None	Persistent high temperature (99·5)	July 23 to Aug. 31	No. 4	Measles	Persistent high temperature proved symptomatic of tuberculosis, of which he subsequently died	Died 1874	9
Very slight branny. August 4th.	None	July 23 to Aug. 4	No. 5	None	Went home to visit friends Aug. 4. No spread of disease there	Left Institution, Jan. 1880	10
None	None	July 23 to Aug. 31	No. 5	None	... ...	Had { Scarlatina, 1877 / Measles, 1880	11
None	None	July 23 to Aug. 4	No. 5	Measles	Visited home, Aug. 4. No spread of disease there	Left Institution, 1879	12
None	None	July 24 to Aug. 31	No. 6 or 9	Measles	... ...	(Left 1880, Oct.)	13
None	None	July 24 to Aug. 31	No. 5	Measles	... ...	Left Institution, 1878	14
None	None	July 25 to Aug. 31	No. 6 or 9	Measles	... ...	Still in Institution	15
None	None	July 25 to Aug. 31	No. 6 or 9	None	... ...	Had { Scarlatina, 1877 / Measles, 1880	16
None	None	July 26 to Aug. 31	No. 5	Measles	... ...	Died, 1875	17
None	None	July 26 to Aug. 31	No. 8	Measles and Scarlatina	This girl helped to make beds of several patients who had rash	Had Scarlatina, 1877	18
None	None	July 27 to Aug. 25	No. 6 or 9	None	Went to Blackpool, Aug. 25. No spread of disease there	Had Scarlatina, 1877, died, 1878.	19
None	None	July 26 to Aug. 31	No. 5?	Measles	Came from same department as No. 5, and had no communication with intervening cases	Had Measles, 1880 (severe)	20
None	None	Aug. 9 to 25	10, 11 or 12	None	Visited friends, Aug. 25. No extension of disease amongst younger children at home	Had { Scarlatina, 1877 / Measles, 1880	21
None	None	Aug. 9 to 31	No. 18	Scarlatina	"Has certainly had Scarlatina; not Measles"	Died, 1879	2
None	None	Aug. 16 to 31	No. 22	None	There was unusual irritability of temper for two days previous to manifestation of rash	Had Measles, 1880	
None	None	Aug. 16 to 31	No. 20 (probably)	Measles	... ...	Still in Institution	24
None	None	Aug. 20 to Sept. 14	No. 24 (certainly)	Doubtful	... ...	Had { Scarlatina, 1877 / Measles, 1880	25
Very slight branny.	None	Aug. 25 to Sept. 14	No. 24 or 25	Measles and Scarlatina	Symptoms resembled those of Scarlatina	Died, 1877	26
None	None	Aug. 31 to Sept. 22	No. 22 or 23 (certainly)	Measles and Scarlatina	This case resembled Measles more than did any of the others	Still in Institution	27
None	None	About a month	?	Measles and Scarlatina	Measles had prevailed in Institution four months earlier, and six cases of Scarlatina occurred in May, June, and July, 1880	Still in Institution.	28
None	None	Do.	No. 28 (?)	Measles and Scarlatina	Do. do. do.	Do.	29
None	None	Do.	No. 29	Measles	Do. do. do.	Do.	30
None	None	Do.	Preceding Cases.	Measles, Feb. 1880 Scarlatina, June 18 80	Was convalescent from Scarlatina when rash of Rubeola Notha appeared	Left Institution, July 1881	31

27th June, J. M. H., a boy from the same department as the preceding, can under observation. A rubeoloid rash, lasting three days, with a maximu temperature of 101·4° on the evening of the first day, and unattended with ar constitutional disturbance, was the principal feature in this case. There w slight reddening of fauces and conjunctival injection, but no catarrhal symptom This boy was also reported to have had both scarlatina and measles in infanc On June 28th, T. H., a lad from same department as preceding, had simil though less severe rash: he had had measles but not scarlatina in infanc These three boys were placed, for the sake of isolation, in the lower rooms of detached wing of the Asylum, the upper floor of which was occupied by cases scarlatina. One of these, a girl aged fourteen, who had sickened with scarlati on the 26th May, and (after a pretty severe attack) was making a satisfacto recovery, surprised her nurse by displaying on the evening of July 5th (t forty-second day) a brownish-red rash on face and neck, afterwards extendi to trunk and limbs, and accompanied by an elevation of the temperature 101·2° on the first evening. The rash remained visible during the second da and gradually faded on the third. There seems every reason to believe that this case infection had spread from the cases downstairs, which, it will remembered, occurred from June 23rd to June 28th. Special interest attacl to this case, inasmuch as the patient had had in the preceding February unquestionable attack of measles, and in the preceding month an unquestional attack of scarlatina.

A review of the history of the several epidemics to which reference has be made will, I trust, tend to throw some light upon the relations of rube notha with scarlatina and measles respectively. Of the twenty-seven patie who are recorded to have had rubeola notha in 1874, thirteen remained un my observation in the institution during the epidemics of scarlatina in 1877 a 1880, and during the epidemic of measles in 1880. "Of these thirteen patie no fewer than *five* had, in the most unmistakable way, both scarlatina a measles in the successive epidemics; whilst of the rest, *two* had measles on and one scarlatina, in addition to rötheln. Thus more than half of the patie remaining under observation from the rötheln epidemic were subsequen attacked with scarlatina or measles, the majority with *both* of these exanthe It is noteworthy that of the seven patients who had measles in 1880 only was said to have had that disease *previous* to the rötheln epidemic. The ot six patients, who had rötheln in 1874 and escaped the measles in 1880, reported to have had the latter in infancy." *

I have advisedly left out of consideration the fourteen cases making up twenty-seven tabulated, several having died, and others living at a distance w their friends. But I trust I have advanced facts enough to justify me urging upon the Section the following conclusion:—*That rubeola notha i disease sui generis, and is entitled to a distinct place in Pathology.*

My experience leads me to dissent from the view that the cases prope designated as rubeola notha are nothing more than mixtures or modifi tions either of scarlatina or measles. My cases show, so far as so smal number of cases can show anything, that the patients who have had rube notha are in no degree protected from subsequent attacks either of scarlat or measles; that cases of rubeola notha never communicate the infection undoubted scarlatina or undoubted measles; and that the characteristic sequ of these two latter diseases never follow cases of rubeola notha. In opp tion to the theory that rubeola notha is merely secondary measles, there the fact that six (if not seven) of my patients had measles, not *previou* but *subsequently* to a characteristic attack of rubeola notha; and the case of

* *British Medical Journal*, No. 1019, p. 49.

girl who had all three disorders within six months, under my own observation, furnishes perhaps the most striking evidence that could be advanced in favour of their distinct pathological character.

The confusion existing in the minds of the profession as well as of the public with regard to the "so-called rubeola, rötheln, or German measles," is much to be deplored. That an epidemic which is, in truth, a compound of scarlatina and of measles, and combines the dangerous elements of both disorders, should pass by the same name as the comparatively innocent affection which I have described, is an anomaly which cannot fail to be detrimental to the public safety. The term "German measles," applied to such a compound disease (I mean the concurrence of scarlatina and of measles), albeit it has sanction of high medical authority, gives a false sense of security, and leads to a neglect of precautions which the public are now learning to adopt with regard to the later stages of scarlatina. Quite recently it has fallen to my lot to visit a village decimated (as regards number of cases) by this compound disease, in which the scarlatina rash and the rash of measles appeared separately, and the characteristic symptoms of each disorder were distinguishable. Severe throat symptoms, nephritis, and free desquamation were observed in many of the cases; and in these there seems no reason to doubt that scarlatinal poison would be diffused, either separately or in conjunction with the infection of measles. The International Medical Congress will confer a benefit upon society at large if, by the discussion of this question, it fixes the legitimate application and obtains the authoritative definition of the "so-called rubeola, rötheln, or German measles," thus at last securing in the bonds of science this very Proteus of Pathology.

On Rubella; Rubeola sine Catarrho; Rötheln, or German Measles.

Dr. William Squire, London.

This exanthem, popularly known among us as "false measles," has been mentioned by various writers under the above names, or as rubeola notha, spuria vel incocta, and sometimes as epidemic roseola, for the last hundred years.

A century was required to complete the separation of measles from small-pox. Another century passed from Sydenham to Withering before scarlet fever was finally distinguished from measles. The disease in question has but a superficial resemblance to scarlet fever; its relations are clearly and closely to measles or rubeola as defined by Sauvages and Cullen. The century is fulfilled that should give autonomy to rubella.

Whether introduced by the Moors into Spain and Italy in the eighth century, or only first described by the Arabian writers, is uncertain. Their name of Hrasbah included both measles and scarlet fever, and gave origin to our comprehensive word "rash." It was included with scarlet fever by Sydenham, and consequently considered a slight ailment. We know that it was so included by Richard Morton (Πυρετολογιας, 1694), where scarlatina is spoken of as a mild form of scarlet fever, and a case of scarlet fever is given as "confluent measles." Hildebrand imagined it to be a true hybrid between scarlet fever and measles, in a way which those who now heedlessly adopt his view would hardly contend for. Bucholz mentions the long incubation. Heberden speaks of measles without fever or precedent catarrh. Willan named it *rubeola sine catarrho*, noticing that it did not protect from measles. Bateman confirms this (Synopsis, Lond., 1813), and makes it a non-febrile variety

of rubeola. The name *rubeola sine catarrho* is objected to by Mason Good, as the genuine measles themselves, capable of affording emancipation, have sometimes appeared with very slight catarrhal symptoms. He says it has been called, and especially by the German writers, spurious measles; but as it occurs more frequently when the genuine measles are epidemic, it is less properly a spurious than an imperfect or immatured rubeola. His prejudice for this being merely a variety of measles, and not itself an independent exanthem, prevented a right judgment being formed on the series of cases observed by Dr. Maton, already before him, which he considered to be varieties of scarlet fever, though distinguished from it by a long incubation period,—a character he notices to belong to certain cases of measles reported by Bucholz when the "efflorescence evinced a like procrastination, and appeared as late as the twenty-first day." *

To Dr. William George Maton, F.R.S., Physician-extraordinary to Queen Charlotte and a Fellow of our College of Physicians, is due the merit of discriminating the exanthem in question both from measles and from scarlet fever. In a contribution to the *Medical Transactions* of the College, vol. v. p. 149, Lond., 1815, Dr. Maton says, that having several times seen cases called either scarlatina or measles, in which the symptoms of illness were trivial and the external characters insufficient to decide their nature, he determined to keep a scrutinising eye on all similar cases. Then follow a series of eight cases. The first is a girl of thirteen, who on August 18th, 1813, had this rash; her face suffused with innumerable points, but she did not feel ill. A sister with her complained slightly, and had some fulness of the small cervical glands; next day she had the rash. In the room with these two sisters were four others of the family, aged from ten to seventeen years; two of these had the rash on September 4th and 5th, the other two on the 7th of September. Two other relatives, the eldest brother, aged twenty-four, and his infant son, a year and a half old, were taken on September 24th and 30th. Dr. Maton remarks: "There is only one other exanthem that I know to which these cases can be considered referable, that is, roseola; but tumours do not occur in roseola, nor is it infectious. The period intervening between the application of the infectious influence and the commencement of the disease was considerably longer than has been noticed in scarlatina. Hence it seems requisite to form a new designation, which, however, I do not venture to propose at present, being satisfied with calling the attention of my colleagues to the subject." It is needless to summarise all the notices of the disease from Dr. Babington to the present time (Trousseau, *Edin. Med. Jour.*) It has been long enough under general notice to deserve a name, and may well receive from the present International Congress a distinctive appellation, which Dr. Maton so wisely chose to defer. He has summed up all the characteristics of the disease in these few cases—the contagious property, the long incubation, the enlargement of the small cervical glands. All that subsequent observation has done beyond confirming these characters is to show that the disease is self-protective, and in fact possesses all those qualities of an independent and specific disease so well summed up by Sir Thomas Watson in the case of varicella, when he says, "This may be a very trivial disorder, but it is stamped with the characters peculiar to the group. It is contagious; it runs a definite course; it occurs but once in the same person." Of two allied diseases, the slighter form is more slowly recognised and distinguished. After the separation of measles, smallpox and varicella were long associated; but no one now doubts, however modified, their absolute distinction. Since the clear definition of scarlet fever, measles is found to have a near ally, as near as chicken pox is to smallpox.

* The Study of Medicine, Lond., 1825, vol. iii. p. 16.

Their gradual differentiation is brought about in the same way. The fever is observed to be less in the one than in the other; the severer disease affords no immunity from the slighter ailment, nor is that in any way protective against the other, while each is self-protective from recurrence. The difficulty of establishing these points may seem to be out of proportion to the practical good to follow; but definite ideas as to the course of the milder ailments, and of the effects produced by them, have important bearings, both as to the care of those attacked as well as on the safety of others, and all that can be collected concerning them is deserving of careful record.

Rubella, then, may be defined as a specific eruption, the rash appearing on the first day of the illness, beginning on the face in rose-red spots, extending next day to the body and limbs, subsiding with the fever on the third day, and rarely either preceded by catarrh or followed by desquamation; the former never for three days before, the latter never after three weeks. Propagated by contagion, it occurs in epidemics of limited extent with sporadic offshoots. It has a long period of incubation, mostly a fortnight, the extreme being twenty-one days; hence a difficulty in tracing the source of personal infection. This is increased by the slight and transient nature of the infection allowing patients to mix freely with others. Cases have been noted where the incubation may possibly have been as short as five or seven days.

One attack is preventive of a recurrence, but is not protective against either measles or scarlet fever, nor do attacks of either of these diseases in any way modify the liability to this one: it is as distinct from them as is chicken-pox from smallpox. During epidemics of measles or of scarlet fever, mild and irregular cases of both are not unfrequently mistaken for this exanthem; well-marked outbreaks of it are often attributed to measles, while slight attacks of scarlet fever are miscalled rötheln, or a hybrid disease imagined which has no existence. Young infants are said to escape; adults not unfrequently suffer, as many persons escape attacks in childhood; sex makes no difference. The disease is contagious even before the rash is thrown out, and it continues to be so for some days, or it may be for two or three weeks afterwards. Second attacks are rarer than for scarlet fever, but the rule against them may be less absolute than for measles. It is seldom fatal. When a mortality of attacks is reported as high as 3 per cent., measles is probably the epidemic present.

Slight fulness of head, heaviness, pain, or giddiness is felt, with a little aching of back and limbs, or tenderness of throat for a few hours on the evening before the rash appears. The cervical lymphatic glands are always perceptibly enlarged, and sometimes have been so for a few days before the rash appears. Mostly the rash is first seen with surprise, as the feeling of illness has already passed, or may have escaped notice. There is redness of the fauces or uvula; less mottled than in measles, not so intense as in scarlet fever; the tonsils are rather full and smooth; there is no ulceration. The eyes are suffused, but there is little or no coryza; the lids are somewhat swollen and irritable; the face is flushed, and the cheeks red or full even before the appearance of the spots; these are bright red, raised, rounded, with clear skin between them, but they soon coalesce; not grouped as in measles, the spots are more prominent than in scarlet fever, and there is not the finely diffused redness of the neck and chest as in that disease. Moreover, the rash is already fading from the face while extending to the limbs, so that it is generally subsiding by the third day. Sometimes an odour as of measles attends the rash. Some itching or a very fleeting yellowish tinge, but no discoloured mottling of the skin is left, and no desquamation, except of the branny kind, which occurs early, and is never delayed to the third week. However little illness is felt at the beginning, a continuous rise of temperature commences with or just before the rash; it may reach 102° to 103°, or

be only two degrees above the normal. With rest in bed this may fall one degree by the end of the second day, but is evenly maintained as the eruption proceeds, or subsides with it on the third day. During the following week it is readily disturbed, either raised by exertion or depressed by fatigue or chill. At this time recrudescence of the rash has been observed. Slight coryza may come on after the rash has faded, the eyelids become sticky, the nostrils stuffed, the throat sore, or some cough begins. Exposure or want of care at this time may determine serious disturbances of health, generally with pulmonary complications. The urine is often high coloured in the early part of the illness; the chlorides are increased, but there is no albuminuria, nor has this ever been known to follow. In some few cases transient complaint of the throat or of fatigue has been made a week before the rash, or epistaxis has occurred; fulness of the small cervical glands is often felt, but no constant intermediate symptoms are found, and any feeling of sickness is without fever.

The sudden onset of this form of rubeola without previous sneezing or cough distinguishes it from measles, to which it is much more nearly allied, as well by general characters as by the kind of rash, than to scarlet fever; but the spots are more evenly distributed, often at wider intervals, and not in groups. There is no gradual rise of temperature before the rash nor the sudden fall afterwards, both characteristic of measles. The small lymphatic glands are palpably enlarged in this ailment down the sides of the neck, and perhaps behind the ears, but not specially at the angle of the jaw, as in scarlet fever. The rash on the second day may look like scarlet fever, or the red flush of scarlet fever, at first sparsely distributed or with prominent red papillæ, may lead to mistake; but the sudden onset is much more marked in scarlet fever, when, should the rash appear as early, it will be more intense on the third day, especially on the neck and chest. Moreover, the fever persists till the fifth day in scarlet fever, even when not greatly elevated; there is also the state of the pulse and tongue or the prominence of throat symptoms. Sometimes it is not till the second or third week that the kind of desquamation, and possibly signs of renal irritation or the occurrence of other cases, complete the diagnosis. The length of interval between succeeding cases is also a distinction. Roseola is not contagious; it occurs in red points or spots not raised above the healthy skin between; there are no throat symptoms, no enlarged lymphatic glands, nor fever. Erythema affects parts of the skin only. Attention restricted to the character of its eruptions often leads to error.

Recovery is so much the rule, that were it not for the mischief any febrile disease may excite in weakly children, the risk of pulmonary disease from close confinement during the rash, or from premature exposure afterwards, all cases of rubella might be expected to do well. In all cases look to the throat and examine the chest. Two months may elapse before health is quite restored. It is not always the trivial disease so often met with. In any large number of cases some will develop unusually high temperatures, just as is seen in varicella; and in others, slow convalescence or marked deterioration of health is dated from this accident. This is due to the individual state or conditions, and not to change in the nature of the disease; it is seen with equal severity in those who have, as in those who have not, had measles previously. Where persons are said to have had measles twice, one of the attacks has been of this kind. A more serious mistake is often made in passing slight cases of scarlet fever under this category by the name, perhaps, of a rose rash or roseola, instead of rubeola or rötheln. Such terms as roseola or even rosalia, by which scarlet fever has been called, should be expunged from the nomenclature of the contagious exanthemata. Whether there be a distinct variety of scarlet fever concealed under the disease as now known is not under consideration. Scarlet

fever and measles, scarlatina and rubeola, belong to different types of disease; the one of short incubation, sudden ingress, and slow decline, with special tendency to throat and kidney mischief; the other of long incubation, gradual ingress, and sudden decline, with tendency to bronchial rather than to renal irritation. It is to this latter, the order rubeola, that the disease under consideration clearly and undoubtedly belongs. I would suggest that, as representing the allied but distinct form of rubeola, the diminutive of this word, "rubella," as already proposed in the United States by the American Dermatological Association, should receive the sanction of this International Medical Congress for general use and adoption.

DISCUSSION.

Dr. FERGUS, Marlborough: The distinction between measles and scarlatina and rötheln has been very much confused by giving the name rubeola to rötheln. Rubeola was the old name of measles. The College of Physicians gave the name *morbilli* to measles and left rubeola out, so that no certainty of distinction can be had. Rötheln is very distinct from scarlatina, and has occurred immediately after an attack of the latter disease. It is most difficult to describe the difference in books, but when the diseases are seen in company the distinction is easy and pronounced. Albuminuria and desquamation rarely occur in rötheln. It would help to avoid confusion if the word rubeola were given to measles by the College. Morbilli is not a fit name for a disease like measles, and will never be understood by the general public.

Dr. JOHN GLAISTER, Glasgow: The conclusion that rötheln is a distinct exanthem from measles or scarlatina has been forced upon me by the fact that I have just lately experienced an epidemic of this disease, in which I saw at least twenty cases. So far as my observations have gone, the incubation period is not a fixed one, varying from four or five days up to a longer period. The characters of the rash are these:—It appears on the third day of the illness, at first on the face and chest, and then the rest of the body. It sometimes distributes itself in crescentic patches mixed with scarlatinal-looking patches; and it begins to disappear on the fourth day. In one case the rash became purpuric. The temperature at the beginning of the eruptive period is about 102° or 103°, gradually declining till the fourth day, when it comes back to normal. I have not seen any desquamation except branny scales. The catarrhal symptoms were in some of the cases very severe; conjunctivitis considerable, with great intolerance of light; sneezing generally present at the beginning of the eruption and before its appearance, but soon passing off. Fauces and tonsils were in most cases injected, sometimes much so; the latter were sometimes so much swollen as to cause difficulty in swallowing; there is occasionally sufficient laryngeal injection to cause difficulty in breathing. What has forced the distinct entity of rötheln upon my mind is, that children have taken it whom I had previously attended with ordinary measles and scarlatina; and when once it made its appearance, generally more members in the family were affected. The treatment is simple and ordinary.

Dr. MARTIN OXLEY, Liverpool: It is a very difficult thing to assign a place to German measles, and I have been very much struck by the fact that experienced practitioners have great difficulty in coming to a conclusion as to the nature of a case accompanied by an eruption of the skin. The mistake seems to me to be in naming a disease from the appearance of the rash. I think we

should wait until we can be certain. This so-called German measles always follows, in my experience, an epidemic of scarlet fever or measles. If it follows the former, it is like scarlet fever; if the latter, it has the appearance of measles. In Liverpool some years ago at a public institution the medical officer was in this difficulty. He had 120 boys packed in an orphan asylum in the centre of the town. He had, besides, two other buildings, one filled with girls, another full of infants. These buildings were in a ring fence. His difficulty was that a few boys had a rash like that seen in scarlet fever. They had slight fever, temperature 100·5°; but they were not ill—they were able to eat. They had little headache; the rash was out over the chest and legs; the disease was spreading, and it was necessary to decide on its nature. He waited, and found that there was no strawberry tongue on the second or third day. He therefore decided to treat the disease as German measles, *i.e.*, keeping in bed while the rash lasted, giving no medicine, and raising no alarm. There was no peeling of the feet or hands, and no child out of the eighty or ninety who had the complaint died or had albuminuria. Speakers here as well as writers elsewhere vary much in their description. On one hand they describe it as like measles, on the other as resembling scarlet fever. I think we must have more evidence of there being a distinct disease before we treat what is known as German measles as a throat complaint.

Dr. J. A. Wood, Liverpool: The rash in German measles is always characteristic, and is generally sufficient to base a diagnosis on. It not only comes out in an irregular and sudden-like way in elevated patches, but the colour alone is, I think, pathognomonic. The peculiar tint of colour is difficult to convey exactly, but it is not livid like plain measles, nor has it the injected efflorescence of scarlet fever. I do not wish to say anything of many other symptoms which may be like, but are not identical with, the symptoms of other complaints, which is a distinction with a difference. It is strange, viewing the whole case, that any confusion should ever arise.

Dr. D'Espine, Geneva: made some observations on a tolerably widespread epidemic of true rötheln which last winter followed an epidemic of measles at Geneva, and which towards its close was accompanied by cases of true scarlatina. He had no doubt of the reality of this disease as a distinct entity, though in some cases the diagnosis was difficult.

Dr. A. Jacobi, New York: The difficulty in making a diagnosis must be great in many instances, for the description of cases and epidemics varies largely even with the best authors. The difficulty is greatest with those in large cities, when and where, as a rule, there are complications with other epidemics, mainly measles (generally preceding) or scarlatina (often following). During the prevalence of any infectious disease, everybody is, to a certain extent, under its influence. Thus during epidemics of diphtheria, scarlatina, erysipelas, cholera, everybody is liable to be taken on slight provocation, or, at all events, to exhibit some of the symptoms. Thus pharyngitis and cervical adenitis are very common amongst people who are considered well. Thus I explain the fact that symptoms not belonging at all to rubeola are mentioned amongst its symptoms. In large institutions, distant from large cities, distant from epidemics of measles and scarlatina, rubeola has been observed, as a distinct disease, to be very contagious, very little febrile, no or very few cases attended with throat symptoms or adenitis, the eruption resembling measles more than scarlatina; convalescence absolute, safe, and easy, and desquamation (farinaceous) very trifling.

J. P. G. Houstoun, Savannah, Ga.: referred to cases of German measles as observed in Savannah and in parts of Florida during the winter of 1880–81 and spring of 1881. The cases were very numerous, very mild, and almost universal, even in the more remote country places, and where there were no cases of measles or scarlatina. It occurred irrespective of previous attacks of measles or scarlet fever. Physicians of large practice in these localities do not hesitate to consider the disease distinct from rubeola and scarlet fever.

Mr. W. Stewart, Barnsley: The force of the discussion of this question seems to hinge principally upon the question of diagnosis. The previous speakers seem to lay the greatest, and, indeed, the only stress, upon the character of the eruption as being the mark by which we are to recognise it as a separate disease. But are we to erect into the importance of a separate disease an affection which only shows its different character by a modification of the measly rash? In the tables distributed by Dr. Shuttleworth, I think all the cases might come under the head of either measles or scarlet fever; and in the two cases No. 10 and No. 26, the symptoms are more those of scarlet fever, especially the fact of their being followed by desquamation. I have recently had a case of a like nature, which for a little time puzzled me with regard to its nature, the premonitory symptoms in which were like those of measles, but where the character of the rash was too papular and discrete to say that it was measles straight offhand. On the second day after, hot applications being applied, the rash became confluent, and the after-history of the case clearly showed it was measles. I believe the affections are the same.

Dr. Shuttleworth, Lancaster: The series of cases I have presented were of a very mild type, contrasting with those of Dr. Cheadle, which were very severe, but might, notwithstanding, be of the same essential character. For my own part, I should be very sorry to undertake to diagnose a case of rötheln with certainty at the first onset, but I think I could generally do so on the *second* day of the rash. There is often a *brownish* hue in rötheln contrasting with the bright redness of scarlatina, and an absence of the crescentic arrangement of papules observed in measles. Moreover, the temperature in rötheln (in mild cases, at any rate) usually attains its maximum on the morning of the second day. This very rapidly subsides, and there is far less febrile disturbance than in measles or scarlatina. But it is only by observation of a *series* of cases, with *their sequelæ and histories*, that one becomes convinced of the distinctive character of rötheln. As medical officer to an institution containing 500 children, the differential diagnosis of rötheln is for me always a matter of much anxiety, and in any dubious case of rash, of course, precautions from the first are indicated. There are, no doubt, epidemics in which scarlatina and measles may prevail simultaneously, and each disease communicates its like; but this is not "German measles" in the sense in which that name and its equivalents were used in my paper; and I think such use of the term constitutes a danger to public health by inducing people to be careless during convalescence.

Dr. Cheadle, London: The papers by Dr. Lewis Smith and Dr. Shuttleworth confirm the fact that previous attacks of measles and scarlatina afford no protection against rötheln. In Dr. Lewis Smith's series of forty-eight cases, twenty-seven had had measles before, eight had had scarlatina. Out of Dr. Shuttleworth's thirty-one cases, nineteen had previously had measles, eight had had scarlatina. Dr. Shuttleworth appears to hold that, while the mild cases described were really a distinct exanthem, viz., rötheln, those of more severe form were something more distinct—hybrids between measles and scarlet fever. With regard

to that point I would remark, that the severe cases I have recorded bred neit scarlatina nor measles; they bred the disease ordinarily recognised as röth The severe cases and the mild cases described in my paper were directly rela many of the latter springing from the former by immediate descent. W regard to the severity of the laryngeal symptoms, I would call attention t point noted in the paper, but which I have omitted in the reading of it for w of time, viz., that in an epidemic prevailing at Folkestone simultaneously v that of 1879 in London, laryngeal symptoms were equally prominent and sev I was informed by Mr. Tyson of that town that three cases of laryngeal obst tion had occurred, so extreme as to require tracheotomy, another in which operation was threatened, and that other cases of the kind had been met w The case mentioned by Dr. Oxley, in which a measles-like rash accompanied severe throat symptoms and exudation on the fauces occurred in a patient had had measles a short time previously, strongly confirms the conclusions forward in my paper.

Dr. WEST, President: A general consent exists—for there are only two sentients—as to the existence of a distinct disease, similar in some resp to, but not identical with, either measles or scarlatina. It is separated from former by the incubation period, which is longer, and approaches more ne to, though it usually exceeds, that of measles. It is further separated scarlatina not only by a difference in the character of the rash, which, ever, it is not always possible to appreciate within the first twenty-four h of its appearance, but also by the non-occurrence either of desquamation albuminuria; while its stage of convalescence is not attended with any of sequelæ which follow scarlatina or measles. A circumstance which marks distinction of rötheln from either is the great frequency of its occurrenc those who have had both measles and scarlatina. Of Dr. Cheadle's cases, th two in number, twenty-two had previously had measles, and ten of them u his own observation. Of Dr. Smith's forty-eight, nineteen had had mea and in another epidemic eight had had both measles and scarlatina withi months; and though it is a well-known fact that both diseases may occur than once, yet a frequency such as this is unexampled in the case of either dis Its affinity seems to be rather with measles; and the disease resembles, if it i identical with, Willan's *rubeola sine catarrho.* The rash often is closely sir in the two diseases; the general character of the symptoms approximates clo As a rule, it is very mild in type, running no danger, and calling fo treatment, though the case related by Dr. Cheadle shows that, notwithstan its distinctive peculiarities, it may present a very severe set of symptoms. observation, too, of Dr. Jacobi's is to be borne in mind, that occurring, often does, either intercurrent between two epidemics, one of measles, or scarlatina, or following or preceding one or other, its character may be s modified as to approximate in some respects to one or the other. A re this, to be borne in mind always, and well for our Epidemiological Soci to remember, as it was borne in mind by Sydenham, as his remarks on epidemic conditions of different years testify. One great practical poi to take care in doubtful cases of scarlatina, because they may be mild, thoughtlessly, and for the sake of giving present relief to the anxiety of par to say of a case, "It is German measles," and thus to be the unwitting of the spread of scarlatina in a family. Another is not to forget tha affinities are with measles, not with scarlatina, and that they resemble each somewhat as varicella and variola—alike but not the same.

> "Facies non una, nec diversa tamen
> Qualis stolet esse sororum:"

not twin-sisters, indeed, but half-sisters at any rate.

Le Rachitis et la Syphilis héréditaire.

Prof. PARROT, Paris.

Le rachitis reconnaît pour cause unique la syphilis héréditaire, et constitue l'altération la plus avancée parmi celles dont cette maladie frappe le système osseux.

Démontrer cette proposition, tel est le but que je me propose. Pour cela, je vais faire voir : qu'à partir des derniers mois de la vie intra-utérine, jusqu'à une époque qui n'atteint jamais le début de la seconde dentition, chez un certain nombre d'enfants, le squelette subit une altération polymorphe, qui évolue d'une manière lente et régulière, toujours la même, aux mêmes instants de la vie, et qui, sous sa forme dernière, est identique au rachitis. Or tous ceux qui en sont atteints sont la proie de la syphilis héréditaire.

Il faut donc avant tout que la syphilis ne puisse échapper ; qu'elle soit reconnue sous ses divers masques ; ce qui me paraît irréalisable à l'aide des moyens de diagnostic actuellement connus. Aussi pour établir la relation que je viens de formuler, dois-je faire connaître quelques signes, qui bien que très communs, n'ont pas encore attiré, du moins à ce point de vue, l'attention des cliniciens.

La syphilis héréditaire se révèle durant sa période d'activité, par des accidents actuels ; plus tard, quand elle est éteinte, on peut encore la reconnaître ; mais seulement aux empreintes qu'elle a laissées.

Les affections de la période active, bien connues, sont comme on le sait, à la surface : l'alopécie, les bulles, les taches, les plaques, les pustules, les ulcérations ; et profondément : les catarrhes, les abcès, les gommes ; et toute une série d'affections viscérales.

Je m'arrêterai quelques instants à une altération de la muqueuse linguale, encore peu connue, et qui présente ceci de particulier, qu'on l'observe avec les précédentes ; et beaucoup plus tard, alors qu'elles ont depuis longtemps disparu ; c'est-à-dire, depuis les premiers mois de la vie, jusqu'à la 7^{me} année, et peut-être même au delà.

Cette affection, circinée et desquamative, débute par des tâches blanches circulaires, où l'épithélium est plus épais que dans le voisinage, se desquamant à leur centre tandisqu'à la périphérie elles envahissent de nouvelles régions, d'où résultent des croissants ouverts en avant, qui s'étendent et se succédent sur la langue, comme des ondes sur une surface liquide ; apparaissant et cessant d'exister, pour se montrer de nouveau, sans cause appréciable et durant des périodes indéterminées.

Propre à la syphilis héréditaire, dans les conditions que je viens de faire connaître, elle n'est pas sans présenter quelque analogie avec d'autres desquamations de la langue. D'ailleurs, ce n'est pas seulement à la surface de l'organe que le mal est apparent ; le microscope montre dans le corps papillaire des amas de corpuscules lymphoïdes, comme au niveau des altérations cutanées et viscérales.

J'aborde maintenant les points véritablement nouveaux de cette étude, c'est-à-dire, la description des empreintes laissées par la syphilis héréditaire, et qui la rendent aussi incontestable, que si elle était là présente en pleine activité. Comme les médailles commémoratives d'événements passés, dont l'effigie et le millésime s'effacent à la longue, mais qui, dans cet état fruste, sont encore visibles pour un œil exercé, elles peuvent servir à reconstituer l'histoire des anciens troubles morbides, et même à fixer leur date.

Parmi ces stigmates, les cicatrices cutanées tiennent la première place ; et jouent le principal rôle dans ce diagnostic posthume.

On peut les rencontrer sur les différentes régions du tégument externe ; mais comme les syphilides dont elles sont la trace, elles se groupent en des points que j'appellerai volontiers *singuliers*, et tirent leur principale valeur de ces localisations.

Sur les lèvres, elles se présentent comme des sillons blancs, à droite et à gauche du lobule, aux commissures, ou sur un plus grand nombre de points. Mais c'est à la partie postérieure et inférieure du tronc, au niveau du sacrum et du coccyx, et sur les membres inférieurs en arrière, notamment sur les fesses et les cuisses, qu'elles sont le plus nombreuses et le plus nettes. Circulaires et de diamètre variable, diversement groupées ; rouges, violacées ou d'un blanc nacré, déprimées ou non, plissées et parfois même un peu gauffrées, elles sont souvent entourées d'une zone de pigment. Celles qui succèdent aux gommes, ont un aspect particulier et tout-à-fait caractéristique. Limitées aux membres inférieures, toujours rares et éloignées les unes des autres, elles consistent en une dépression infundibuliforme, violacée surtout au début, au fond de la quelle, il y a une petite cicatrice linéaire. Longtemps les tissus mous restent indurés à leur niveau.

Sur les dents, les empreintes de la syphilis héréditaire sont beaucoup moins communes que les précédentes ; mais infiniment plus solides et tenaces ; peuvant garder leur physionomie originelle, non seulement durant la vie de l'individu, mais après sa mort, pendant de longs siècles. Nous en avons de nombreux exemples ; et l'on peut dire qu'après les documents fournis par les os, il n'en est pas de plus incontestables, pour démontrer la très haute antiquité de la syphilis.

Tout ce qui a trait à ces empreintes, se présente à nous avec une rigueur mathématique, tant au point de vue des relations qui lient celles des différentes dents, que, dans leur chronologie ; si bien que celle d'une dent étant connue, toutes les autres s'en suivent.

Je rangerai dans cinq catégories ces modifications diverses, et les désignant sous la dénomination commune *d'atrophie*, je les qualifierai de :

Cupuliforme.
Sulciforme.
Cuspidienne.
en Hache.
d'Hutchinson.

La première, de beaucoup la plus fréquente, atteint surtout les incisives de la deuxième dentition. Elle est constituée par de petites dépressions superficielles, rarement isolées, presque toujours disposées sur la couronne en lignes horizontales.

La seconde apparaît sous la forme de sillons parallèles, dans la même région que la précédente.

L'atrophie cuspidienne frappe toutes les dents, mais surtout les premières molaires et les canines permanentes. Par elle, la couronne se trouve divisées en deux parties inégales et très distinctes l'une de l'autre. La plus éloignée de la gencive, amoindrie dans toutes ses dimensions, irrégulière, avec des cuspides acérées, semble enchassée dans l'autre.

Ces trois modalités atrophiques sont fréquemment réunies sur une même dent.

Elles résultent de modifications pathologiques de l'ivoire et de l'émail. Ce dernier toujours aminci ; parfois, fait complétement défaut.

La carie est la conséquence fréquente de cette absence de l'émail.

Les deux dernières variétés de l'atrophie ne sont pas primitives comme les précédentes et ne datent pas, du moins avec leur forme typique, de la période

intra-alvéolaire ; elles ne se montrent que consécutivement à l'éruption des dents, et sont le résultat de la carie ou de l'usure en des régions prédisposées à les subir par une altération congénitale de l'émail.

Celle en hache n'existe que sur les incisives supérieures. Durant une certaine période, la partie la plus voisine de la gencive est seule érodée, et comme le tranchant est intact, on a l'apparence d'un fer de hache.

Dans l'atrophie d'Hutchinson, c'est au contraire la partie centrale du bord libre des incisives, qui a été rapidement usée à cause de l'amincissement qu'y avaient déterminé de nombreuses cupules, au moment des dépôt de l'émail. Il en résulte une encoche de profondeur variable ; triangulaire ou en croissant.

L'atrophie peut atteindre toutes les dents, à l'exception des secondes et troisièmes molaires, et des prémolaires permanentes.

Son existence chez des sujets atteints de syphilis héréditaire, son évolution qui coïncide avec la période d'activité de cette maladie, m'engage à la considérer, suivant la remarque, déjà faite par M. J. Hutchinson pour la variété qu'il a décrite, comme étant sous sa dépendance, et m'autorise à repousser, si non d'une manière absolue, du moins dans l'immense majorité des cas, toute autre origine. En effet, les pyrexies de l'enfance, dont on a parlé, n'apparaissent en général qu'après la seconde année, alors que la formation de la dent est achevée.

Pour ce qui est des convulsions éclamptiques, aux quelles M. Magitot et ses élèves, font jouer un rôle exclusif dans cette étiologie, je les repousse d'une manière générale ; parce que chez la plupart des sujets, l'altération s'est produite ou a débuté durant la vie intra-utérine, dont les accidents nécropathiques sont encore inconnus ; parce que la hauteur et la profondeur des lésions, si bien expliquées par la durée et l'intensité d'action de la syphilis héréditaire, sont inconciliables avec le temps, relativement très court, des accès éclamptiques. Enfin, parce que les deux dernières molaires ne sont jamais atteintes. Immunité qui s'explique aisément par l'étiologie que je soutiens ; l'activité de la syphilis étant éteinte dans la période où ses dents se développent ; tandis qu'elle est en contradiction avec l'opinion de M. Magitot, car les convulsions sont loin d'être rares à cette époque de la vie infantile.

Ainsi la syphilis héréditaire se révèle d'une part par des éruptions cutanées, une affection desquamative de la langue et des altérations viscérales ; de l'autre, par des cicatrices cutanées et une atrophie particulière du système dentaire. Grâce à ces signes, elle n'échappera que dans un nombre de cas restreint.

Je vais maintenant examiner l'affection du squelette, qui par une série d'étapes conduit au rachitis, et qui coïncidant d'une manière à peu près constante avec les marques que je viens de faire connaître, doit être mise sous la dépendance de la syphilis héréditaire.

Cette affection est essentiellement systématique, et, si je puis ainsi dire, chronologique. Elle se présente sous des formes déterminées et distinctes aux diverses périodes de la vie infantile ; mais chez les différents sujets, sa physionomie reste invariable pour une même époque.

Elle atteint, suivant un ordre constant, les pièces du squelette et pour chaque os, les mêmes régions. Ses variétés sont très nombreuses, mais elles peuvent être rangées sous trois types principaux, qui correspondent à trois époques successives de l'enfance ; ce sont, par ordre de date :

Celui des ostéophytes durs.
Celui de l'atrophie gélatiniforme.
Celui du tissu spongoïde.

L'altération du premier type se présente dans toute sa pureté chez les fœtus mort-nés, les avortons, et dans les premières semaines de la vie extra-utérine. Elle consiste essentiellement en des couches ossiformes nouvelles, véritables

ostéophytes qui se développent à la périphérie des os longs et plats ; et qui affectent une disposition tout-à-fait caractéristique sur la moitié inférieure de l'humérus notamment en arrière, et à la face interne du tibia. Ces couches nouvelles, un peu moins dures que l'os normal, s'en distinguent surtout par leur teinte et par la direction de leurs trabécules, qui sont perpendiculaires à l'axe diaphysaire. En même temps, dans beaucoup de cas, il existe au voisinage de l'épiphyte, sur une épaisseur variable, mais qui dépasse rarement un ou deux millimètres, une substance crayeuse, plus friable que le reste de l'os, que je qualifie de *chondro-calcaire*, parcequ'elle n'est autre chose que le tissu cartilagineux infiltré de sels calcaires.

Dans l'altération du second type, on trouve les deux modifications précédentes de la diaphyse avec cette légère restriction, que les ostéophytes ont une dureté moins grande ; mais, en outre, et c'est là ce qui la caractérise, certaines portions, en général très circonscrites, de l'os sont remplacées par un tissu mou, aqueux, transparent, de nuances diverses, souvent jaune mais ou sucre d'orge, ayant l'apparence d'une gelée. C'est à elle que se rattachent les brisures juxta épiphysaires et la pseudo-paralysie syphilitique.

L'altération du troisième type est caractérisée par le tissu que M. Jules Guérin a qualifié de spongoïde ; élément nouveau et que l'on ne recontre en aucune autre circonstance. Il constitue les ostéophytes péridiaphysaires, et a pour siège de prédilection le voisinage de l'épiphyse, d'où il pénètre, sous forme de bourgeons, dans la couche chondroïde du cartilage, devenue exubérante. En même temps l'os tout entier a perdu sa dureté, et se laisse aisément couper à l'aide d'un scalpel, qui est dû à sa décalcification et au développement anormal de l'élément médullaire. Ces modifications histologiques ont pour conséquences : les tuméfactions des extrémités des os, qui sont comme noueuses, la déviation des surfaces articulaires, la déformation des diaphyses et leur fracture.

Sous des influences qu'il serait actuellement malaisé de déterminer, le mal débute non d'une manière nécessaire par la première forme, mais par l'une quelconque d'entr'elles. Il s'y arrête, ou bien, au contraire, il parcourt tous les étages ultérieurs. Et quand il a atteint sa période ultime, quelle que soit, à ce moment, sa violence, quels qu'aient été les désordres accomplis jusque là, il se manifeste spontanément une tendance vers la guérison, qui peut ne laisser après elle aucune trace des troubles antérieurs, mais qui, d'autres fois, fixe irrémédiablement les déformations qui s'étaient produites.

Toutes les parties du squelette peuvent être atteintes comme les os longs, en même temps, et suivant des modalités identiques. Je ne m'occuperai que du crâne. Il subit l'atrophie gélatiniforme qui, de la région sous périostique, arrive parfois jusque sous la dure-mère et amène la perforation complète de sa paroi. Plus tard il se couvre d'ostéophytes, qui ont pour siège de prédilection les angles péribregmatiques du frontal et des pariétaux, d'où ils s'étendent souvent aux deux tiers antéro-latéraux de sa surface. Il en résulte habituellement une déformation typique, qui m'a fait qualifier de *natiformes* les crânes atteints de la sorte.

Durant la période spongoïde et du ramollissement osseux, tandis que les régions antérieures s'épaississent comme il vient d'être dit ; en arrière, les circonvolutions cérébrales usent la paroi et même la perforent. C'est le *crâniotabes* d'Elsaëesser.

Après cette étude morphologique, si l'on aborde celle des détails de structure on voit, que ces lésions sont d'importance très différente, que l'atrophie gélatiniforme, la décalcification et la médullisation, n'occupent que la seconde place, puis qu'elles consistent : la première en une fonte des éléments osseux auquels se substitue un réseau fibrillaire ; la seconde en la résorption des sels calcaires ; et la troisième en la prédominance des parties molles sur les dures ;

tous phénomènes qui peuvent se produire dans d'autres circonstances pathologiques ; au contraire, que les ostéophytes, qui marquent les différentes périodes et les relient entr'elles, constituent un fait propre caractéristique et d'ordre vraiment spécifique. L'on ne trouve plus, dans leur tissu, ainsi que dans l'os normal, des canaux de Havers, courant dans une direction parallèle au grand axe de la diaphyse avec leurs systèmes de lamelles concentriques et leurs ostéoplastes aux canalicules multiples. Ils sont remplacés par de larges espaces, disposés perpendiculairement à l'axe de l'os, occupés par un réseau, conjonctif, des vaisseaux et de rares médullocelles, et limités par les trabécules ossiformes, constellées de corpuscules irréguliers, semblables à ceux du tissu conjonctif, avec des angles, d'où partent des prolongements fibrillaires, qui s'anastomosent et forment ainsi dans cette substance fondamentale morbide, un réseau dont la densité, va croissant avec les progrès du mal et atteint son maximum dans le tissu spongoïde.

Dans ces production nouvelles, l'élément anatomique et fondamental est donc toujours le même, aux différentes périodes de l'évolution morbide. Ce qui varie et donne aux ostéophytes des différents types, une physionomie particulière, c'est le nombre de ces éléments, c'est la dureté de la substance fondamentale où ils sont enchassés, c'est la largeur des espaces qui séparent les trabécules ; toutes particularités de valeur minime, et qui laissent subsister, entre les produits morbides, la parenté que nous montre le microscope. Cette parenté d'ailleurs, se révèle chez certains malades, d'une manière saisissante, par la disposition stratigraphique, sur le même os, de couches de types différents, rarement intactes, il est vrai, et dans l'état où elles s'étaient originairement déposées, chacune ayant ses caractères propres, mais retenant quelque chose des précédentes.

Ces altérations osseuses, ne diffèrent donc qu'en apparence. En réalité, elles font partie d'un même processus morbide, c'est-à-dire, que leur cause est la même ; leur physionomie diverse est une question d'âge, voilà tout. Si donc l'on peut découvrir l'origine de l'une d'elles, il faudra l'attribuer aux autres.

Eh bien ! j'affirme, pour l'avoir constaté plusieurs centaines de fois, que les deux premiers types se rencontrent exclusivement chez des sujets présentant, dans les viscéres ou sur la peau, quelque marque incontestable de la syphilis héréditaire. D'où il faut induire que le troisième et dernier type, est lui aussi sous la dépendance de cette maladie. Mais ce n'est pas tout ; chez un nombre considérable de sujets, enfants, adolescents ou adultes, actuellement atteints de l'affection chrondro-spongoïde, ou présentant les déformations qui en sont la conséquence ; on trouve soit des manifestations actuelles de la syphilis héréditaire, soit les stigmates que j'ai fait connaître.

Si maintenant l'on se reporte aux descriptions classiques du rachitis l'on voit que cette affection des os de l'enfance ne diffère en rien de la syphilis osseuse arrivée à sa troisième période. Dans l'un et l'autre cas, c'est le tissu spongoïde qui sert de caractéristique ; car, sans lui, il peut y avoir de l'ostéomalacie, mais il n'y a pas de rachitis.

Donc le rachitis est engendré par la syphilis héréditaire.

Est-il suprenant de voir un mal aussi particulier, aussi typique, reconnaître cette cause unique et puissante au lieu de celles que jusqu'ici on lui a attribuées ? Non certes ! surtout quand on jette un coup d'œil sur cette étiologie.

A la tête des causes prédisposantes, on met l'influence des générateurs. Si leurs enfants sont rachitiques, c'est qu'ils l'étaient eux-mêmes ; ou bien tuberculeux, scrofuleux, épuisés trop jeunes, ou trop vieux, ivrognes, sanguins ou bilieux ; et d'après Van Swieten mous, oisifs, adonnés aux plaisirs de la table, aimant les plats gras et sucrés, mangeant peu de pain, buvant des vins trop doux, se servant d'eau chaude, épuisés par des maladies chroniques ou par l'amour.

Puis viennent les affections mêmes de l'enfance : la toux, la diarrhée, toutes les fièvres. On fait jouer un rôle considérable aux climats froids et humides, aux habitations basses, imparfaitement aérées et éclairées.

Les causes occasionnelles ne sont pas moins nombreuses. On a surtout accusé l'insuffisance et la mauvaise qualité des aliments. Guersent s'en prenait au lait et aux farineux. M. Jules Guérin, après avoir incriminé l'allaitement trop prolongé, voit au contraire aujourd'hui l'origine du mal, dans une alimentation trop animalisée. Trousseau a soutenu cette manière de voir. L'insuffisance des sels calcaires a joué un rôle non moins important que l'alimentation dans cette étiologie ; et M. Seemann pense que, chez les rachitiques, il existe un trouble digestif essentiellement constitué par la production insuffisante d'acide chlorhydrique, d'où l'absorption insuffisante de sels de chaux par le tube digestif. Pour Broca et M. Gamba, le rachitis n'est pas une maladie spéciale, mais bien la conséquence de toutes les causes qui troublent la nutrition pendant la période de croissance du jeune âge. Comme on le voit, tout est invoqué mêlé, confondu, dans cette étiologie si compréhensive, si disparate, si contradictoire.

Et en vérité rien n'est moins fait pour convaincre de l'efficacité de l'une de ces causes, que leur liste aussi longue que banale.

On comprend à la rigeur, que la plupart d'entr'elles, par leur action débilitante, aient rendu moins active la nutrition des divers tissus, et notamment celle du squelette ; mais de là à faire du tissu spongoïde et des ostéophytes, il y a loin. D'ailleurs ne sait-on pas qu'il y a des rachitiques gros et gras ; et par contre que beaucoup d'enfants malingres, chétifs et en proie à des diathèses comsomptives, ne sont jamais atteints de rachitis.

Il y a donc un vide considérable dans l'étiologie classique du rachitis. Ce vide je crois l'avoir comblé en faisant intervenir la syphilis héréditaire ; et en lui attribuant d'une manière exclusive la genèse de cette altération des os.

Il y a déjà longtemps que cela avait été dit ; tout ne l'a-t-il pas été ; mais à ma connaissance, cela n'avait pas été démontré à l'aide de preuves empruntées à la clinique et à l'anatomie pathologique, comme celles que je viens de présenter au Congrès.

De la Syphilis comme Cause de Rachitisme et de Malformation dentaire.

Dr. Bouchut, Paris.

Par suite de l'état cachectique qui l'accompagne si habituellement, la syphilis infantile, héréditaire, peut être une cause de rachitisme et de malformation dentaire. C'est l'opinion d'un grand nombre de médecins. Mais le rachitisme reconnaît bien d'autres causes. Leur nature mérite d'être indiquée avec soin ; et, puisque la question est mise à l'ordre du jour du Congrès International de Londres, je vais dire l'opinion à laquelle je me suis arrêtée.

Je parlerai—

1° *Des malformations dentaires cachectiques* attribuées à la syphilis.

2° *Du rachitisme* considéré comme la manifestation constante d'une diathèse syphilitique.

1° Les malformations dentaires d'un certain ordre sont depuis les travaux d'Hutchinson assez souvent attribuées à la syphilis. Mais s'il a donné la description de ces altérations, il n'a peut-être pas suffisamment établi le diagnostic différentiel entre cette malformation et les autres, de sorte que les

lecteurs ont pu supposer que sa description avait un caractère général s'appliquant à toutes les malformations de la seconde dentition. Des observations analogues ont été publiées par quelques autres médecins. On a depuis lors considéré presque tous les sujets dont les incisives permanentes étaient piquées, rougies ou échancrées comme atteints de syphilis héréditaire.

C'est là une affirmation de séméiologie et de diagnostic qui est trop absolue, et sur laquelle il importe de faire des réserves.

Magitot a déjà fait remarquer que les dents incisives permanentes, offrant l'altération décrite par Hutchinson, n'appartenaient pas à des sujets syphilitiques. W. Nicati en a dit autant, et, dans un travail trés-intéressant, a montré que certaines malformations des incisives permanentes étaient dues au rachitisme plutôt qu'à la syphilis et qu'il fallait établir un diagnostic différentiel entre les unes et les autres. Cela est à mes yeux un pas vers la vérité, mais ce n'est pas encore la vérité tout entière.

J'ai observé très-souvent les altérations et les malformations des dents de première et de seconde dentition chez les enfants. Mes conclusions différent totalement de celles d'Hutchinson et se rapprochent beaucoup au contraire de celles de W. Nicati. Je vais m'expliquer à ce sujet, lorsque j'aurai dit quelques mots des altérations des premières dents, dites *dents de lait.*

Ces dents s'altèrent avant ou après leur sortie de la gencive.

Avant leur sortie, elles peuvent s'altérer dans leur germe ou dans leur évolution, si l'enfant a des convulsions, ce qu'a signalé Magitot, s'il a eu du rachitisme congénital, enfin s'il a eu de *l'ostéoporose ou rachitisme acquis* par suite d'une dyspepsie prolongée, d'une entérite grave ou d'une autre maladie. Dans ces différents cas les premières dents sortent tard, sortent mal, sortent irrégulièrement, sont jaunes, altérées dans leur émail et tombent quelquefois prématurement.

Après leur sortie des gencives les dents de lait s'altèrent, si les enfants sont gravement malades, s'ils ont une entérite prolongée avec acescence buccale. Elles noircissent, se corrodent, au niveau du collet, se creusent, et se cassent au bord de la gencive.

Si les enfants ont une stomatite ulcéreuse qui ronge la gencive et la détruit sur son bord libre dans une plus ou moins grande hauteur, les dents s'allongent et on en voit la racine dans une assez grande étendue.

Mais les malformations les plus curieuses sont celles de la seconde dentition. Elles portent principalement sur les incisives permanentes. Elles résultent d'une maladie de la première enfance qui a modifié la nutrition des germes dentaires.

Quelle est cette maladie ?

Cette maladie est-elle diathésique et donne-t-elle une forme particulière à la malformation des dents ?

D'après ce que j'ai vu, il n'y a pas de maladie spéciale diathésique qui produise la malformation spécifique des incisives permanentes. Toute maladie grave et prolongée de la première enfance produit ce résultat. La syphilis pas plus que le rachitisme ou l'entérite chronique n'a d'action particulière. Oui, la syphilis héréditaire, oui, le rachitisme déterminent la malformation des incisives permanentes, mais ce n'est pas comme syphilis ou comme rachitisme ; c'est comme état cachectique. J'ai donné pour preuve les mêmes malformations observées par moi à la suite de l'entérite chronique et des suppurations prolongées du premier âge. Ces altérations sont des *malformations marastiques ou cachectiques,* mais pas autre chose.

C'est donc comme état cachectique modifiant la nutrition des germes dentaires que la syphilis héréditaire et le rachitisme déterminent l'altération des dents permanentes, et non comme maladies diathésiques ; à cet égard elles se confondent avec toutes les autres maladies cachectiques du premier âge.

Il se passe dans les germes dentaires ce qui se passe dans les ongles, dans les cheveux et dans les os. La nutrition de ces éléments s'altére dans toutes les maladies. Les ongles, après s'être amincis un moment, s'épaississent lorsque l'enfant va mieux et présentent ces sillons que Beau a fait connaître dans la dyspepsie. Les cheveux sont secs, pâlissent, deviennent langoureux, étiques, et, comme on le dit, les enfants ont mauvais poil. Les os se gonflent au niveau des épiphyses, se raréfient et offrent de *l'ostéoporose*. Ils sont plus mous, se courbent facilement, ne peuvent plus momentanément porter les enfants, qui veulent rester au lit après avoir commencé déjà de marcher. C'est le rachitisme.

Toutes ces altérations analogues sont de même nature et dépendent de *l'état marastique, cachectique, cacochymique et cacothrepsique*, comme on dit à présent.

Les germes dentaires de la seconde dentition subissent la loi de généralité et ils deviennent cachectiques avec le reste de l'économie, quelle que soit la cause du trouble survenu dans la nutrition générale des sujets. Ils souffrent quand tout souffre, et les dents qui en sortent sont malades, altérées dans leur émail et dans leur ivoire, quand la première enfance a été très longtemps et très gravement maladive.

D'après ce que j'ai observé en dehors de la syphilis et du rachitisme proprement dit, on trouve les malformations dentaires signalées par Hutchinson et par Nicati comme appartenant à ces deux maladies. Ainsi pour ne citer que quelques faits je raconterai l'observation suivante relative à un jeune garçon dont je connais la famille et que j'ai suivi depuis dix ans. La malformation de ses dents incisives ressemble à la malformation décrite comme syphilitique par Hutchinson, et cependant elle n'est que cachectique.

X., fils de M. X., rue des jeûneurs, a failli mourir au moment de sa naissance, il y a 10 ans, d'une hémorrhagie nasale pour laquelle j'ai été appelé en consultation. Il est resté exsangu, et le perchlorure de fer employé comme hémostatique lui a laissé un coryza chronique qui dure encore, et qui gêne la respiration. Cet enfant a été malade toute sa première enfance, et sans rachitisme ou syphilis ; il est pâle, très maigre et a fort mauvaise mine malgré un assez bon état de santé. Les dents sont blanches ; il a quatre incisives écartées, et les deux moyennes échancrées au bord tranchant sans altération de l'émail. Il en est de même à la machoire inférieure. Il a ses premières molaires, mais bien qu'il ait 10 ans, il n'a pas de canines. Sur une fille de 12 ans, nommée Kertzel, au 24 de Ste. Catherine, sortie le 7 février, guérie d'une scarlatine, qui n'a pas eu de syphilis ni de rachitisme, et qui a été maladive dans le premier âge, on trouve les deux incisives médianes supérieures, intactes dans les cinq sixièmes supérieurs, mais près du bord libre un sillon transversal profond, anfractueux qui amincit tout à coup la dent et forme comme un pas d'escalier. Les autres dents sont saines.

Dans un autre cas, encore à l'hôpital en ce moment, il s'agit d'une jeune fille, L., de 11 ans, couchée au No. 27 de Ste. Catherine, entrée le 30 Decembre 1879, pour un vertige épileptique. Elle est petite, maigre, peu développée pour son âge, n'a jamais eu de signe de syphilis héréditaire après la naissance ni postérieurement. Elle a toujours été maladive, et dans la première enfance a eu de fréquentes diarrhées. Ses incisives supérieures présentent des érosions pointillées, c'est-à-dire, des piqûres noirâtres au niveau desquelles l'émail est a peu près détruit, en même temps que l'ivoire est altéré. Ces ponctuations sont au nombre de quatre sur chaque incisive, et elles sont placées sur une ligne horizontale près du bord tranchant qui est fortement ébréché par de fines dentelures. Au dessus de cette ligne de piqûres de l'émail se trouve un relief transversal formant comme une marche d'escalier.

Ces lésions ressemblent à celles que W. Nicati attribue au rachitisme, et cependant l'enfant n'est pas rachitique, n'a pas les côtes déformées ni la tête

volumineuse. Elle ne l'a pas été. Elle n'a été que maladive et cachectique dans la première enfance.

Sur une autre fille (V.) de 14 ans, non formée, couchée au No. 5 de Ste. Catherine et entrée le 30 janvier 1879, pour des crises nerveuses hystériformes, il y a aussi une malformation considérable des dents. Cette fille a été maladive dans toute la première enfance, a eu des convulsions pendant longtemps jusqu'à 10 ans et puis sa santé est devenue plus robuste. Aujourd'hui ses quatre incisives sont étroites en haut et plus larges en bas, échancrées, concaves et mousses à leur bord libre. L'émail est au dessus rugueux, piqué de points noirâtres et offre un sillon transversal déprimé. Les incisives inférieures sont également échancrées et concaves au bord libre qui est mousse. L'émail est altéré, et, au dessous du bord, offre un sillon très-déprimé, noirâtre. Les canines coniques et petites présentent également une altération de l'émail près de la pointe.

L'autre enfant, qui est un garçon, présenté à l'hôpital (30 janvier 1879), a 19 mois. Celui-là offre une malformation des dents de lait qui sont au nombre de huit. La première est sortie à 7 mois, âge réglementaire. Cet enfant ne marche pas, ses membres inférieures sont grêles, il a les symphises chondro-costales tuméfiées de façon à offrir le chapelet rachitique. Il n'est pas sujet à la diarrhée mais il tousse habituellement; il a la poitrine *grasse*, et remplie de râles muqueux. Sa mère n'est pas syphilitique, on ne sait rien du père, et lui-même n'a pas offert de trace de syphilis héréditaire. Chez lui les deux dents incisives médianes supérieures sont écartées, jaunâtres, courtes, largement échancrées et mousses à leur bord libre. Les incisives supérieures latérales sont collées aux autres, coniques et jaunâtres. Les incisives médianes inférieures sont également échancrées; mais ici elles se touchent et les incisives latérales sont plus courtes, coniques et jaunâtres. L'émail n'est pas piqué mais il est un peu altéré dans sa couleur.

Ici, la malformation a les caractères de ce que l'on a dit être la syphilis, mais elle est de nature cachectique par rachitisme.

Une autre petite fille de 4 ans, au No. 31 de ma salle Ste. Catherine, hémiplégique, à la suite de convulsions répétées, survenues à l'âge de 16 mois, peut cependant marcher. Elle a quinze dents, très mal formées et mal placées. Elle n'a ni rachitisme, ni syphilis; elle est cachectique. A la mâchoire supérieure, elle a deux incisives droites obliquement placées; la médiane assez large et jaune, et celle qui est à côté conique, petite et serrée contre la première. Les deux incisives de gauche sont également obliques, et forment avec celles du coté opposé un V ouvert par en bas. Elles sont également de mauvaise nature, mais elles ne sont ni échancrées, ni piquées, ni dentelées. Les dents incisives inférieures sont également mal rangées, jaunes et très-petites.

Ici, pas de syphilis ni de rachitisme, mais bien un état maladif, prolongé, suite de convulsions et de paralysie. C'est un de ces cas que l'on pourrait citer à l'appui de l'opinion de Magitot, qui a dit que des troubles trophiques de la nutrition des germes dentaires pouvaient résulter de l'état convulsif.

Dans un autre cas, que je pourrais citer, et qui est relatif à l'un de mes élèves, interne autrefois à l'Hôpital Ste. Eugénie, et aujourd'hui confrère parisien, les dents incisives supérieures sont piquées, pointillées et transversalement sillonnées par une dépression de l'ivoire. Il n'y a eu chez lui ni syphilis héréditaire, ni rachitisme, mais seulement un état cachectique prolongé dû à une première enfance maladive.

A l'hôpital des enfants, dans mon service, et à la consultation publique, ces faits de malformation dentaire sont très-nombreux. Ils sont plus rares dans la première dentition que dans la seconde et c'est principalement vers 10 à 15 ans que je les ai observés. J'en ai recueilli 114 exemples.

Tout étant bien considéré, après une analyse minutieuse des causes et d phénomènes de ces 114 exemples de malformations, il m'a semblé que si on l observait après la syphilis héréditaire c'était moins comme état syphilitique q comme conséquence d'un état cachectique ou marastique. En effet, sur ces 11 cas, je n'en ai trouvé que huit qui fussent syphilitiques. J'en dirai autant d malformations exclusivement attribuées au rachitisme.

D'ailleurs la recherche de ces causes est extrémement difficile et délicate. Sa quelques cas où les parents avouent leur syphilis et celle de leurs enfants, et ce dans lesquels le médecin a suivi l'enfant pendant plusieurs années, depuis naissance, on en est réduit aux conjectures et aux hypothèses les moins acce tables.

Que de fois n'ai-je pas vu considérer comme dents syphilitiques des den appartenant à des enfants sur le passé des quels on n'avait aucun renseigneme précis ; à des enfants non syphilitiques mais dont le père avait eu autrefois syphilis ; ou, enfin, à des enfants dont on ne connaissait pas les antécédents, m qu'on jugeait pouvoir être syphilitiques, parce que la plupart des pères peuve avoir eu la syphilis. Il en est de la recherche des influences syphilitiques s les dents, comme de la recherche de *l'arthritis* dans la production de certai affections cutanées ou viscérales dites arthritiques. Avec une forte idée p conçue on trouve tout ce qu'on désire.

Ce n'est pas ainsi que doit progresser la science. Il faut en médeci bannir toutes les hypothèses, et n'y introduire que des faits certains et bi observés. Tout ce qui est vague doit être mis de côté, ou du moins ne d être accepté qu'avec réserve.

Ainsi en est-il des dents réputées syphilitiques et rachitiques. Cependant qu'on a dit des dernières est bien plus acceptable que ce qui a été dit des p mières. Ces altérations sont un effet de l'état cachectique et pas autre cho Cachexie syphilitique, cachexie rachitique, cachexie scrofuleuse, cachexie int tinale ou pulmonaire, toutes déterminent *l'ostéoporose*, ralentissent le mouvem de nutrition du tissu osseux et ainsi engendrent les malformations dentaires et déformations du squelette en général.

2° *Ramollissement des os par état cachectique.*—Tout ce que je viens de d de ces malformations dentaires s'applique au ramollissement des os longs, gonflement de leurs extrémités, à leur déformation, et à tout ce que les mé cins appellent *rachitisme*. En effet, chez les jeunes enfants une maladie ai prolongée, une fièvre éruptive, suivie de complications sérieuses, une enté chronique, arrêtant l'évolution du tissu osseux, ramollissent les os dont extrémités se gonflent, et dont la diaphyse se raréfie, enfin, amènent les dif mités du squelette avec ou sans fracture consécutive. On produit ces défor tions chez les oiseaux en supprimant tout phosphate de chaux de leur nourritu et chez de jeunes chiens d'une même portée on en fait autant par l'alimentat prématurée et par la claustration dans un endroit obscur. La maladie et l'é cachectique sont les causes de cette altération du tissu osseux.

Sans doute la syphilis héréditaire congénitale, ou développée peu après naissance, peut amener l'ostéoporose ou rachitisme, mais au même titre que t autre état de souffrance grave, que toute maladie chronique de l'intestin, l'estomac, des poumons et des articulations, survenue dans les deux premi années de la vie. C'est l'état cachectique, quelle qu'en soit la cause, q dépende d'une entérite chronique, d'une suppuration prolongée, de la scrof de la syphilis, qui en est la cause, en troublant la régularité du mouvement d nutrition osseuse. La spécificité de la cause, ou, si l'on veut, la diathèse, n'y pour rien ; c'est l'affaiblissement des jeunes sujets qui est le point de départ toutes ces altérations.

En résumé, les malformations dentaires, l'ostéomalacie, le rachitisme, peuv

être parfois le résultat de la syphilis infantile, mais, dans le plus grand nombre des cas, ces lésions n'ont rien de syphilitique, et elles dépendent de l'état cachectique ou marastique des jeunes enfants.

Die Syphilis als eine Ursache der Rachitis.

Dr. M. KASSOWITZ, Vienna.

Ich habe bereits in meiner im Jahre 1876 erschienenen Monographie über die Vererbung der Syphilis (S. 135) in Kürze diese Frage erörtert. Seitdem habe ich aber gerade die Rachitis und die krankhaften Veränderungen des Knochensystems bei der hereditären Syphilis, sowie überhaupt die normale und pathologische Knochenbildung einem sehr eingehenden Studium unterzogen, und ich bin eben im Begriffe, die Resultate dieser Unternehmungen der Oeffentlichkeit zu übergeben.*

Da aber die Section für Kinderkrankheiten des internationalen medizinischen Congresses diese Frage aufgeworfen hat, so stehe ich nicht an, auch jetzt schon meinen Standpunkt in derselben in möglichster Kürze zu präcisiren.

Nach meinen Untersuchungen liegt *das Wesen des rachitirten Processes* in einem chronischen entzündlichen Vorgange, welcher an den Appositionsstellen der wachsenden Foetalen und kindlichen Knochen seinen Ausgang nimmt, und sich hauptsächlich in einer krankhaft gesteigerten Neubildung von Blutgefässen und in einer bedeutenden Hyperämie sämmtlicher Gefässe in jenen Teilen des Knochensystems äussert, in welchen aber die Auflagerung neuer Knochenteile erfolgt, also hauptsächlich in der Knorpelwucherungsschichte der Chondoepiphysen, im Perichondrium und Periost, und endlich in der Nahtsubstanz der Schädelknochen.

Diese übermässige Vascularisation und Hyperämie des knochenbildenden Gewebe hat nun notwendiger Weise eine krankhafte Modification der Knochenbildung in ihrem Gefolge. Es haben nämlich meine Untersuchungen schon für die *normale Ossification* ergeben, dass die Erscheinungen der Knochenbildung und Verkalkung, sowie auch die Knocheneinschmelzung und die Resorption der Kalksalze in hohem Grade abhängig sind von den Blutgefässen des Knochens und der knochenbildenden Gewebe, und ich will diese Abhängigkeitsverhältnisse für unsere Zwecke in folgenden Punkten zusammenfassen :—

1.—Die Bildung von Knochengewebe und die Ablagerung von Kalksalzen in das letztere findet niemals in der unmittelbarsten Nachbarschaft der Gefässwände statt, weil die von dem Blutstrome ausgesendete Saftströmung in einem gewissen Umkreise um das Blutgefäss die Ablagerung der Kalksalze verhindert. Eine fortschreitende Ossification ist nur möglich bei nachlassender Saftströmung, wie sie entweder durch allmälige Obliteration der Blutgefässe oder durch die zunehmende Entfernung derselben von dem ossificirenden Gewebe bedingt ist.

2.—Eine experimentell hervorgerufene Verstärkung der Blut- und Saftströmung in einem bestimmten Teile des wachsenden Knochensystems verhindert in deutlich nachweisbarem Maasse die Ablagerung der Kalksalze in den neugebildeten Knochenteilen.

3.—Fertiges und bereits verkalktes Knochengewebe verliert seine Kalksalze

* Ein Teil dieser Arbeit ist bereits unter dem Titel : "*Die normale Ossification und die Erkrankungen des Knochensystems bei Rachitis und hereditärer Syphilis. I. Teil. Normale Ossification,*" bei Braumüller 1881 erschienen. Den zweiten Teil, welcher die Rachitis behandeln wird, bin ich eben im Begriffe zu bearbeiten.

und wird wieder in weiches Markgewebe umgewandelt, wenn es in die lebhafte Saftströmung eines neugebildeten Blutgefässes geräth, oder wenn ihm ein bere vorhandenes Blutgefäss nähergerückt wird, oder wenn ein solches aus irge einem Grunde eine Steigerung seiner Blutfülle und der von ihm ausgehend Saftströmung erfährt.

Wenn nun die knochenbildenden Gewebe und in weiterer Folge der ga Knochen von überaus zahlreichen und blutüberfüllten Gefässen durchzog sind, so wird nicht nur das neugebildete Knochengewebe eine abnorme Besch fenheit aufweisen und speciell die Ablagerung von Kalksalzen in diesem n gebildeten Gewebe verzögert oder gänzlich verhindert werden, sondern es werd auch vielfache Einschmelzungen des bereits verkalkten und erhärteten Gewe erfolgen, an die Stelle dieses harten Knochengewebes wird weiches Bildun gewebe und später wieder unverkalktes und daher wenig resistentes ne Knochengewebe treten, und der ganze Knochen wird nach und nach in bekannten Weise seiner Starrheit verlustig werden. Auch die für die Rach so charakteristische, übermässige Knorpelwucherung lässt sich ganz einfach die abnorm vermehrte Säftezufuhr zu dem wachsenden Knorpel zurückführe

Es liegt auf der Hand, dass diese Auffassung des rachitischen Proces wenn sie auf richtiger Basis beruht — was ich in meiner ausführlichen Ar beweisen zu können glaube — viel besser die gesammten Erscheinungen Rachitis zu erklären im Stande ist, als die bisherigen Theorien, welche fast a nahmslos eine mangelhafte Zufuhr oder eine mangelhafte Aufnahme von K salzen in den Organismus zum Ausgangspunkt nehmen. Denn abgesehen von, dass eine solche mangelhafte Zufuhr nur für eine sehr geringe Anzahl Fällen wirklich angenommen werden kann, könnte eine solche höchstens mangelhafte Verkalkung der neugebildeten Knochenteile erklären, dage würden die Gründe für alle übrigen so auffälligen Veränderungen im gesa ten Knochensystem, für die vermehrte und abnorm beschaffene Knorpelwuc rung, für die so bedeutend gesteigerten Einschmelzungsprocesse im Kno und Knochen, für die krankhaft *gesteigerte* Auflagerung von periostalem Knoch gewebe u. s. w. vollkommen im Dunkeln bleiben.

Wenn nun wirklich ein entzündlicher Process die Grundlage sämmtlic rachitischer Erscheinungen im Knochensysteme ist, so drängt sich uns sod die Frage auf: Wodurch wird jener Entzündungsprocess zunächst in knochenbildenden Geweben und dann im gesammten Knochensystem her gerufen? Warum entwickelt sich derselbe gerade nur in den letzten Foe monaten und in den ersten Lebensjahren? Und schliesslich, warum ist gerade das Knochensystem, welches in dieser Lebensperiode in so enor Häufigkeit einem solchen Entzündungsprocesse anheimfällt?

Knüpfen wir gleich an die letzte Frage an, so müssen wir vor Allem Erinnerung bringen, dass *das Knochengewebe sich in einem wichtigen Pu fundamental von allen anderen Geweben des thierischen Körpers unterscheidet,* gewissermassen ein Unicum unter den Geweben bildet. Alle übrigen Gew wachsen nämlich *expansio*, und gleichzeitig in allen ihren Teilen, sie vergröss ihr Volum dadurch, dass ihre sämmtlichen Gewebebestandteile wachsen sich vermehren, dass also zwischen den alten immer wieder neue Gewebet sich bilden. Eine solche Art des Wachstums ist aber für den starren unausdehnbaren Knochen durch die physikalische Unmöglichkeit ausgesch sen, und es ist daher hier eine Volumsvermehrung überhaupt nur möglich du *Auflagerung neuer Knochenteile auf der Oberfläche des bereits erhärteten K chens.* Man denke nur, welche ganz enorme Steigerung das Wachstum jenen Appositionsteilen des Knochens im Vergleiche mit irgend einem Br stücke eines in allen seinen Teilen gleichmässig expansio wachsenden Gewe von gleicher Ausdehnung erleiden muss, namentlich wenn man die Apposit

an einem intensiv wachsenden Knochenende eines langen Extremitätenknochens oder einer Rippe in's Auge fasst. Was bei dem expansiven Wachstum in der ganzen Ausdehnung des Organes geschieht, muss hier in der räumlich so beschränkten Wucherungsschichte des Knorpels geleistet werden.

Daraus resultirt aber auch notwendiger Weise *eine gänzlich abweichende Verteilung in dem Zuflusse der Ernährungssäfte* für die wachsenden Teile des Knochens einerseits und für die übrigen Organe andrerseits. Während also z. B. die foetale oder kindliche Leber in allen ihren Teilen nahezu gleichmässig wächst, und demnach die Ernährungssäfte, welche das Material für das Wachstum liefern, auf dem Wege der Blutgefässe zu allen Teilen des Organs ganz gleichmässig zugeführt werden; muss dieses Ernährungsmaterial, soweit es zum Wachstum verwendet werden soll, fast ausschliesslich zu den Knorpelwucherungsschichten und in etwas geringerem Maasse zu dem Periost hingeleitet werden, während dem ganzen übrigen voluminösen Skeletteil nur jene verhältnissmässig wenig bedeutende Menge des Ernährungsmaterials zukommt, welches zu dem Lebensunterhalte des, eines weiteren Wachstums unfähigen erhärteten Teile des Knochens hinreicht.

Setzen wir nun den Fall, dass in der Säftemasse des foetalen oder kindlichen Organismus irgend ein abnormer Bestandteil circulire, welcher im Stande wäre, *einen krankhaften Reiz auf die Gefässwände* und auf die übrigen von der Saftströmung passirten Gewebe auszuüben, so wird es wol begreiflich sein, dass ein solcher krankhafter Reiz sich an jenen Punkten in ganz unvergleichlich gesteigertem Maasse geltend machen muss, wo eben aus physiologischen Gründen eine so ausserordentlich gesteigerte Menge des Säftematerials fortwährend die Gefässe und die Gewebe passirt; dass also, mit anderen Worten, ein jeder krankhafte Reiz, welcher den Gesammtorganismus des Foetus in den letzten Foetalmonaten und den des Kindes in den ersten Lebensjahren trifft, gerade an den Knochen, und gerade an jenen Knochenenden, welche eine besonders lebhafte Apposition aufweisen, sich ganz besonders geltend machen, und Anlass zu der Entstehung und Erhaltung jenes entzündlichen Zustandes der Gefässe und Gewebe geben muss, welchen wir an jenen Stellen bei der Rachitis *de facto* beobachten.

Wenn wir nun weiter fragen, wodurch jene abnormen Bestandteile und krankhaften Reize innerhalb der Blut- und Säftemasse der Rachitischen entstehen, so müssen wir zunächst die Umstände erwägen, unter denen die Rachitis überhaupt zu Stande kommt, und diese Umstände sind, soweit uns die klinische Beobachtung darüber belehren kann, die folgenden :—

1.—Die Rachitis ist vorwiegend eine Krankheit der Armut. Bei einer sehr sorgfältigen Untersuchung sämmtlicher Kinder *unter* 3 *Jahren*, welche mein Ambulatorium besuchten, ergab sich regelmässig und immer wieder ein Verhältniss von *mehr als* 90 *Rachitischen unter* 100 *Kindern* überhaupt.

2.—Die vorzüglichste Schädlichkeit, welche Rachitis hervorruft, liegt nach meinen Erfahrungen in den *schlechten Wohnungsverhältnissen*. Die Wohnräume der Arbeiter und ärmeren Handwerker, deren Kinder das Contingent für die öffentlichen Ambulatorien abgeben, sind fast immer eng, übervölkert, schlecht oder gar nicht ventilirt, und die Kinder verleben ihre ersten Jahre, insbesondere in der schlechteren Jahreszeit, fast ununterbrochen in einer verdorbenen und durch die menschlichen Ausdünstungen, die Effluvien der Küche, des Waschtroges, der Handwerkstube u. s. w. verunreinigten Athmosphäre.

3.—Die *schlechte Ernährung des Kindes* kommt bei meinem Materiale entschieden erst in zweiter Linie, wenn auch die grosse Bedeutung derselben für die Entstehung der Rachitis keineswegs unterschätzt werden soll. Die Mehrzahl der Kinder wird nämlich hier an der Mutterbrust ernährt, und dieselben zeigen oft trotz ihrer rachitischen Affection einen nicht gerade schlechten Ernäh-

rungszustand. Dagegen ist es zweifellos, dass die meisten Kinder bei nachlässiger künstlicher Ernährung, oder solche, die von kümmerlich genährten Müttern gesäugt werden, ganz besonders häufig den schweren Formen der Rachitis verfallen.

4.—*Eine jede den allgemeinen Ernährungszustand des Kindes in den ersten beiden Lebensjahren beeinträchtigende Erkrankung*, insbesondere eine jede chronische Affection der Respirations- und Verdauungsorgane begünstigt in hohem Grade die Entstehung der Rachitis, und bewirkt in einem noch viel höheren Grade die Steigerung einer bereits vorhandenen rachitischen Affektion.

5.—Die Rachitis beginnt viel häufiger, als man bisher angenommen hat, *bereits in den letzten Monaten der intrauterinen Entwicklung.* Die Totgeborenen und Frühverstorbenen der Gebärhäuser und der Findelanstalt ergaben mir bei der makroskopischen und insbesondere bei der histologischen Untersuchung der rasch wachsenden Knochenenden in einem sehr bedeutenden Percentverhältnisse bereits sehr deutlich nachweisbare rachitische Veränderungen. Ebenso konnte ich auch bei den lebenden Kindern ausserordentlich häufig schon in den ersten Wochen so greifbare Veränderungen im Knochensysteme nachweisen, dass man deren Entstehung unbedingt in die intrauterine Periode verlegen muss. In allen diesen Fällen wird man also notwendiger Weise zu der Annahme gedrängt, dass hier die krankhaften Reize aus der Säftemasse der Mutter in jene des Foetus übergegangen sind.

Es ist also nicht etwa eine einzige bestimmte Schädlichkeit, welche die Rachitis hervorruft, sondern offenbar ist *eine jede, durch welche Ursache immer hervorgerufene krankhafte Beschaffenheit der Säftemasse des Foetus und des Kindes in der Periode des energischesten Längenwachstums* im Stande, an jenen Stellen, wodurch die energisch gesteigerte Knochenapposition ein vermehrter Affluxus und demnach eine Potenzirung jener Schädlichkeiten erfolgt, jenen entzündlichen Vorgang herbeizuführen, welcher die rachitischen Veränderungen des Knorpel- und Knochengewebes und in weiterer Folge die rachitische Erweichung des Skelettes zur Folge hat.

Nun begreift es sich auch ganz leicht, dass eine *Heilung des Rachitis* nicht möglich ist durch eine künstliche Zufuhr von Kalksalzen, weil diese, in noch so grosser Menge zugeführt, sich niemals auf jenem hyperämischen und entzündlich afficirten Boden ablagern können; wol aber durch die Entfernung und Bekämpfung jener Schädlichkeiten, welche die entferntere Ursache des entzündlichen Reizungszustandes an den Appositionsstellen sind; ferner durch alle jene hygienischen Maassregeln und Heilbehelfe, welche die Lebensverhältnisse und den allgemeinen Ernährungs- und Gesundheitszustand des Kindes verbessern; und endlich auf dem Wege der Naturselbstheilung, wenn nämlich nach Ablauf der ersten Lebensjahre die Energie des Längewachstums bedeutend herabsinkt, und dadurch auch die Verteilung der Ernährungssäfte in den einzelnen Skeletteilen eine gleichmässigere und wenigstens annähernd jenem Verhältnisse ähnlich wird, welches allen übrigen Organen des menschlichen Körpers *während der ganzen Wachstumsperiode* zugeschrieben werden muss.

Wenn diese meine Theorie der Rachitis die richtige ist, dann ergiebt sich *das Verhältniss der Syphilis hereditaria zu der Rachitis* ganz von selbst. Hier tritt uns eben eine von jenen supponirten schädlichen Materien, welche in der Blut- und Säftemasse des Foetus und des rachitischen Kindes circuliren sollen, in einer concreten Form entgegen. Wie immer man sich auch das Syphilitische Virus vorstellen mag, so kann es doch kaum zweifelhaft sein, dass dasselbe in der Blut- und Säftemasse verteilt ist, und dass es im Stande ist, in den verbindensten Organen und Geweben durch seinen Reiz Entzündungsprocesse hervorzurufen. Dass dieses Gift auch in dem Knochensysteme Entzündungs- und

Zerstörungsprocesse hervorruft, und dass dieselben beim Foetus und beim Neugeborenen speciell an jenen Knochenenden hervortreten, welche ein lebhaftes Längenwachstum aufweisen, ist durch die Untersuchungen der letzten Jahre hinlänglich bekannt geworden, und man wird sich nach dem Vorhergegangenen darüber um so weniger wundern, als eben notwendiger Weise auch in diesem Falle eine Sammlung des Giftes an jenen Stellen des lebhaften Appositionswachstums stattfinden muss.

Aber auch abgesehen von diesen intensiven Processen, welche zu einer weitgehenden Zerstörung des Knorpel- und Knochengewebes und zur vollständigen Ablösung der Chondroepiphysen von den Diaphysen führen können, vermisst man bei dem hereditär syphilitischen Foetus und Kindern der ersten Lebensjahre *niemals* die deutlichsten Zeichen einer entzündlichen Reizung, also die vermehrte Vascularisation und Blutüberfüllung der knochenbildenden Gewebe mit allen ihren Consequenzen, wie sie eben der Rachitis zukommen, und auch die augenfälligen Erscheinungen der Rachitis, die Auftreibung der Knochenenden, die Craniotabes, die charakteristische Veränderung der Schädelform, die Anomalien der Zahnbildung u. s. w. vermisst man nur sehr selten bei den hereditär Syphilisirten, ja man findet dieselben auch sehr häufig dann, wenn diese Kinder unter den günstigsten Verhältnissen gehalten werden. In diesen Fällen ist es also höchst wahrscheinlich das syphilitische Gift selber, welches den entzündlichen Process an den Knochenappositionsstellen hervorruft und unterhält.

Darin scheinen mir indessen einige Autoren der neueren Zeit zu weit gegangen zu sein, dass sie, wie Parrot in Frankreich und Lees und Barlow in England, der Syphilis die Hauptrolle bei der Entstehung der Craniotabes zuschreiben wollen. Nach meinen Beobachtungen ist die Craniotabes eines der häufigsten Symptome der allgemeinen Rachitis im ersten Lebensjahre. Ich habe aufs Geratewol die Notizen über die letzten 100 *Kinder unter einem Jahre*, welche in den letzten Wochen in mein Ambulatorium gebracht worden sind, in Hinsicht auf die Rachitis revidirt, und habe gefunden, dass von diesen 100 Kindern 84 zweifellos rachitisch waren, und nur 16 sich als ganz frei, oder noch nicht mit zweifellosen Erscheinungen der Rachitis behaftet erwiesen. Von diesen 84 Rachitischen waren 47 mit Craniotabes behaftet. Von diesen 100 Kindern waren aber nur 3 hereditär syphilitisch. Diese waren allerdings sämmtlich schon rachitisch und 2 zeigten die Erscheinungen der rachitischen Schädelerweichung. Alle übrigen 45 Kinder mit Craniotabes waren nicht hereditär syphilitisch.

Wenn ich also auch damit nicht übereinstimmen kann, dass der ererbten Syphilis ein numerisch so bedeutender Anteil an der Entstehung der Rachitis oder eines speciellen Symptomes derselben, nämlich der Craniotabes, zugestanden werden soll, so stimmt doch sowol meine Theorie der Rachitis, als auch die tatsächliche Beobachtung darin überein, *dass auch das syphilitische Gift im Stande ist, den rachitischen Process in den Knochen hervorzurufen und zu unterhalten.*

Beiträge zur Pathologie der Rachitis.

Dr. F. H. Rehn, Frankfort-on-the-Maine.

1.—Vergleichende Messungen des Schädelumfangs gesunder und rachitischer Kinder des ersten und der ersten Hälfte des zweiten Lebensjahrs ergaben *im Durchschnitt* grössere Werthe bei letzteren, besonders vom 8ten Lebensmonat ab.

2.—Die in diesem Lebensabschnitt noch vorhandene—durch pathologische Verhältnisse oft gesteigerte Dünnheit des Schädeldachs, lässt im gegebenen

Fall den Schluss auf ein grösseres Hirn-Volumen rachitischer Kinder zu. (Messungen aus späterer Zeit sind nicht maassgebend.)

3.—Der chronische Ventrikel-Hydrops, in seiner wesentlich extrauterinen Entwicklung, stellt eine sehr seltene Complication *angeborenen* Rachitis dar. Die Diverse ist prognostisch wichtig, in der der Vortragende einen hochgradigen Fall der Art nach nochmaliger Punction bei entsprechender diätetischer und medicamentischer Behandlung heilen sah.

4.—In einer Anzahl von Fällen hochgradiger Rachitis constatirte Vortragender eine geringe Vermehrung der weissen Blutkörperchen;—unabhängig von einer Milz-Vergrösserung, Vermehrung bis zum vierten Jahre der normalen Zahlung, (mit dem Malassiz-Hagen'schen Apparat, modificirt von Thoma-Zirpi).

5.—Den bei Rachitis vorkommenden Krampf-Formen ist, nach der Beobachtung des Vortragenden, auch der *Spasmus nutans* in seinen prognostisch, günstigen Formen—und zwar nicht immer mit dem Mittelglied erschwerter Dentition—zuzurechnen.

6.—Es kommen bei hochgradiger Rachitis, abgesehen von der Malacie der Schädelknochen, ausgeprägte Malacien der grossen Extremitäten-Knochen, speciell des Vorder-Arms und Vorderschenkel-Knochen vor, welche die Möglichkeit nahetragen, dass unter besonders ungünstigen Ernährungs-Bedingungen eine mehr oder weniger allgemeine Erweichung des Skeletts bei Rachitis eintreten können.

7.—In einem dieser Fälle wurde eine erhebliche Ausscheidung von Erdphosphaten—in überwiegender Menge phosphorsaurer Kalk—im Harn bei dreimaligen Proben nachgewiesen. Dieser Nachweis ist von Wichtigkeit, insofern er die Untersuchungs-Resultate neuerer Autoren (Neubauer, Seemann u. A.), welche keine Verwesung in Erdphosphate nachweisen konnten und die älterer Untersucher, welche erhebliche Mengen fanden (Marchand, Lehmann) zu ermitteln geeignet ist.

Zur Pathologie der Rachitis.

Dr. Adolf Baginsky, Berlin.

1.—Die klinische Erfahrung, dass Rachitis congenital vorkommt, dass die Mehrzahl mit congenitaler Syphilis geborener Kinder bei bester Pflege rachitisch wird, dass die Rachitis bei Kindern entsteht nach schweren fieberhaften Krankheiten, nach schweren chronischen Dyspepsien, bei schlechter Ernährung, mangelhafter Zufuhr frischer Luft, die weitere Erfahrung, dass Rachitis mit erheblichen Alterationen des Nervensystems verläuft (so Laryngismus Stridulus, Hydrocephalus, Hirnhypertrophie), alles dies schliesst schon klinisch die Möglichkeit aus, die Krankheit auf einen einfachen Defect der Kalksalze zu reduciren.

2.—Die Annahme, dass die Rachitis auf einem blossen Defect der Kalksalze beruhe, wird auch dadurch ausgeschlossen, dass das Verhältniss des Kalkgehalts des Knochens zu der organischen (fettfreien) Substanz desselben so erheblich verändert ist, cir. 160·100 gegenüber 563·100 in normalen Knochen, dass wenn man voraussetzt, man habe einem Kinde den Kalk ein ganzes Jahr hindurch gänzlich entzogen, dieses Missverhältniss nicht resultiren würde, sondern nach annähernder Berechnung auf 513·100 käme.

3.—In einem gewissen Gegensatze zu diesen klinischen Erfahrungen stehen die experimentellen Studien einiger Autoren (obenan Roloff, Berlin) welche den Nachweis geführt haben, dass man durch Entziehung des Kalks im Futter, Rachitis bei Thieren erzeugen könne.

(*a*) Reine Entziehung des Kalks im Futter erzeugt allerdings eine erhebliche Veränderung im Knochen.

(*b*) Dieselbe stellt sich makroskopisch und mikroskopisch dem Auge nahezu so dar, wie eine leichte rachitische Veränderung des Knochens.

(*c*) Der erzeugte Grad der Knochenveränderung ist aber, wie die anatomische Untersuchung, ganz besonders aber auch die chemische Analyse ergiebt, ein milder.

4.—Wenn man nun neben der Entziehung des Kalks, Milchsäure dem Futter zusetzt, so sind die erzeugten Veränderungen wesentlich beträchtlicher. Das ist beweisbar:

(*a*) Sowohl durch die makroskopische und mikroskopische Untersuchung des Knochens.

(*b*) Ganz besonders durch die chemische Untersuchung.

5.—Die beträchtliche Beeinflussung des Knochens durch die Milchsäure, neben der Kalkentziehung beweist, dass in der Kalkentziehung das Massgebende der Einwirkung nicht liegen könne.

6.—Vielmehr ist bei den Factoren, der Kalkentziehung und der Milchsäurefütterung, das Gemeinschaftliche, dass sie eine Alteration der Gesammternährung bewirken; das eine Mal geschieht dies durch Entziehung eines integrirenden Bestandtheils des Organismus, das andre Mal durch Einführung einer die Verdauung störende Substanz.

7.—Die Läsion der Knochen tritt nur deshalb in den Vordergrund, weil die Störung der Gesammternährung in eine Zeit fällt, wo das Knochenwachsthum der jungen Thiere besonders lebhaft ist.

8.—Man kann weiterhin den Nachweis suchen, dass neutrale Peptonlösungen Kalk zu lösen im Stande sind.

9.—Diese Eigenschaft der Peptone zeigt den Weg, wie bei Kindern, deren Blut durch fehlerhafte Ernährung alterirt ist, Kalksalze aus dem im Wachsthum und in der Ossification begriffenen Knochen gelöst und weggeführt werden.

10.—Die Ausscheidung der so weggeführten Kalksalze geschieht, wie man durch Untersuchung der Stuhlgänge rachitischer Kinder erweisen kann, durch den Darm.

11.—Demnach ist also die Rachitis eine Dyscrasie, hervorgegangen aus einer Alteration der Gesammternährung, welche wiederum durch die mannigfachsten, auf den jungen Organismus einwirkende Schädlichkeiten, bedingt wird.

12.—Damit stimmen die seit längst her gegenüber der Rachitis errungenen therapeutischen Erfolge durch solche Mittel, welche die gesammte Ernährung zu heben geeignet sind, insbesondre gute hygienische Verhältnisse, normale Nahrung, der Gebrauch von Bädern und tonisirenden Arzneien.

DISCUSSION.

Dr. JULES GUÉRIN, Paris: If the doctrine which purports to attribute rickets to congenital syphilis had not been put forward gravely by a man whose name and whose work lay claim to consideration, it would have been out of place to dwell on it at all. This doctrine is indeed not only an empty hypothesis, but manifestly false, and is in clear contradiction to all the facts of observation and experience. A few words will be enough to demonstrate this statement. As far as *observation* goes, the doctrine of syphilitic rickets is based alone on a material change, arbitrarily and quite exceptionally detached from the malady in its entirety, by the suppression of all the other symptoms and lesions which mark its course. Rickets, as we see it in its development under our obser-

vation, begins by a general disturbance of the economy—an alteration of the digestive functions. On this there follows a perversion of nutrition. The osseous system, to which, before my researches, the whole malady was exclusively referred, begins to undergo change only in a second stage of the disease. It suffers at first an arrest and then a perversion of its process of organisation. To keep to the most well-marked characteristics of this disturbance, I must call attention to the fact of the concentric layers of the long bones being the seat of a nearly colourless fluid effusion which separates them from one another and renders them isolated. This interlamellar effusion of liquid undergoes a progressive change of consistence and appearance. It becomes gelatiniform, and soon acquires the consistence of a reticulated tissue of a pink colour, to which I have given the name of "tissu spongoïde." At a more advanced period, if the malady tends to get well, this spongoid tissue becomes the seat of calcareous deposits, the islets and lamellæ of which can be seen by the naked eye. Finally this newly formed bony tissue acquires all the consistence of normal bone. During this succession of changes of the newly formed tissue the old osseous tissue itself presents a series of modifications no less interesting and significant. Arrested at first in its development, it loses its consistence, its lamellæ waste and are partly absorbed. That which remains unites with the newly formed tissue and becomes inseparable from it. When the disease does not tend towards cure all the old bony tissue disappears, and gives way to a mass of spongoid tissue reddish in colour, areolar and elastic, which yields to the weakest muscular contractions, whence result the angular bendings and fractures of rickets. In a lesser degree of the disease, some of the lamellæ of the old bone still remain, but they undergo changes in form, owing to their defective resistance, the vertical action of the weight of the body, and the slow but repeated actions of the muscular contractions. In this way come about the more regular curvatures of the legs and thighs, the deformities of the pelvis and of the spinal column. What place and what meaning among this assemblage of facts, which are strictly successive and harmonious, and are united in one and the same morbid process, can be claimed by certain temporary changes of bone, accidental in character (the nature of which I refrain, indeed, from considering here), and which even the author of the theory I am combating recognises and allows as being often wanting? They can only be *epiphenomena*, the minute description of which testifies to the patience of the observer, but comment on which is only calculated to throw into confusion what is certainly and clearly demonstrated. Thus much for the testimony of observation. Now for that of direct experiment. Having been led by the observation of more than two thousand cases of rickets in children, who came before me at the Hôpital des Enfants, to ascertain that all of them, belonging to the working classes, had been prematurely fed on substances fit only for adults, either alone, or in combination with either a natural or artificial milk diet, I drew the conclusion that this malady might be the product of this premature alimentation. Since that time, at several intervals, I have submitted dogs of three or four weeks old to a course of premature feeding—bread, meat (and especially raw meat), and no milk. Not only have these animals exhibited in succession the phenomena of rickets as it develops in children—large belly, swelling of the epiphyses, curvature of the shafts of the bones and difficulty in walking—but I have proved, by an interruption of the prescribed diet, a kind of suspension of the morbid process, and then the return and aggravation of the process when recourse was again had to the experimental regimen. Two of these dogs, killed at the most advanced period of the malady, showed all the alterations of the osseous tissue that are seen in rachitic children, and, in particular, the separation of the concentric layers of the long bones, and the production and deposit in their interstices of the characteristic spongoid

tissue. I have showed several specimens from these animals at the Academy of Medicine. I have had drawings made of them at the most advanced period of their deformity, and I have now in my collection, for all who wish to see them, two of the skeletons of these artificially rachitic dogs. To these unyielding facts, to those decisive experiments, M. Parrot has brought in opposition certain experiments which have not succeeded, and denials or erroneous interpretations. Among other things, our colleague says he has produced osteomalacia by premature feeding, but not the "spongoid" tissue. This result depends on the animals having been killed at too early a stage of the disease, when the bony tissue had not yet begun to be re-formed. As to osteomalacia, it is never found in dogs submitted to premature feeding any more than in children, for the good reason that osteomalacia, an essentially different disease from rickets, and belonging to a more advanced age, is quite apart from rickets in its causation. Finally, I have shown that in putting forward "spongoid" tissue as a specific mark of syphilitic rickets, M. Parrot has supported himself entirely on a merely transitory stage of the malady, on an isolated fact, throwing on one side all the other phases and phenomena which constitute rickets in its entirety.

Dr. Byers, Belfast: My object in taking part in the present discussion is to show how clinical experience gained at a provincial children's hospital points in reference to the *quæstio vexata* of the relationship of syphilis to rickets. At the out-patient department of the Hospital for Sick Children in Belfast, with which I am connected, cases of rickets are of frequent occurrence, and when I say cases of rickets, I must be understood to include in that category not merely those exaggerated forms in which almost every bone in the child's body is affected, and which more frequently come under the care of the surgeon than the physician, but also those milder varieties, too often overlooked, in which the morbid process is only just beginning, or has been arrested at an early stage of its development. On the other hand, cases of congenital syphilis are much less frequently met with. I have observed carefully a large number of these cases of rickets, keeping specially in view the question of syphilitic causation, and the result has been, that so far as these observations have gone, they are opposed to the view that syphilis *per se*, as has been insisted on very strongly by some, is the cause of rickets. The evidence afforded by these cases as opposed to the syphilitic causation of rickets is twofold:—(1.) In families in which there were rickety children I could get no evidence, in the great majority of cases, of the occurrence of syphilis in them, in other members of the same family, or in their parents. (2.) In the case of families in which some of the children presented unmistakable evidence of congenital syphilis, or in which I knew that the parents had been the subjects of this disease, I was unable to find in them, or in their brothers or sisters, those signs which we in England are accustomed to regard as diagnostic of rickets, with the exception of craniotabes, about which I shall have to say a few words. With reference to the occurrence of this symptom craniotabes, which some believe to be of such paramount importance as an early diagnostic sign of rickets, my observations would confirm the conclusions of Dr. Barlow and Dr. Lees, who have done such good work in elucidating the clinical value of this symptom—viz., that it is much more frequently present in the skulls of those who are the subjects of congenital syphilis than in the rickety. Indeed, I would be inclined to say—at least such is the result of my own observation—that this sign is by no means of such frequent occurrence in rickets as one would infer from a perusal of the current literature on the diseases of children. I have not as yet met with examples of children who at one time showed signs of hereditary syphilis afterwards becoming rickety; and even if cases of this kind did occur, they would

by no means prove that the syphilis was the sole cause of the rickets, unless we could exclude altogether every other existing cause, such as improper food, &c.; for I hold that it is just as illogical to argue that in such cases the syphilis and rickets stand in the relationship of cause and effect, as it would be to say that in the case of a child who at present is the subject of rickets, and who had previously suffered from measles, or any of the other exanthemata, that the measles was the sole cause of the rickets, unless in all such cases we were absolutely sure that there was no other element, such as unsuitable diet, bad hygienic condition, &c., which might have acted as a cause. I remember how Mr. Jonathan Hutchinson in his clinical lectures at the London Hospital used to impress on us that syphilis is the great simulator of other diseases, illustrating this specially by reference to skin diseases; and he has shown that in congenital syphilis a state of general periostitis is very common, in which the joint ends are swollen and tender and the child is in pain all over. Such a condition would be very liable to be mistaken for rickets, with which, however, it has no real connection. I am compelled, then, to agree with those who hold that syphilis is not the cause of rickets. My own observations lead me to conclude that errors in food, either in quantity or in quality, are the principal cause of rickets.

Professor STEPHENSON, Aberdeen: A question so important and general, cannot be decided by the changes in the structure of bone, nor yet on the general pathology alone. Physiological experiment is valuable, but not in itself sufficient. With these must be combined the clinical history. The researches must agree, not contradict each other. I have made the clinical history a subject of special study for twenty years in Edinburgh and in Aberdeen, where I have seen much of rickets. The result of my observations emphatically contradicts the statement of M. Parrot, that "hereditary syphilis is the constant cause of rickets." The clinical course of the two diseases, as a speaker has already observed, is wholly different, and in some respects opposite. M. Parrot has observed this, and tried to get out of the difficulty by saying, "The syphilis is spent. It no longer exists, but has substituted for itself a new affection. This is an incontestable example of transformation of disease." The pathological facts demonstrated by M. Parrot are extremely valuable, but his deductions therefrom are wholly erroneous. Syphilis and rickets are quite distinct conditions. They are like the red and yellow rays in a beam of light; they may combine to form an orange band, but to regard the one as caused by the other is as false as to say that the yellow light is transformed into the red. The keynote to the pathology is the recognition that rickets is dependent upon a diathesis, or, to avoid the differences of opinion attached to that term, I would say a *constitutional habit*. Bad hygienic conditions are not in themselves sufficient; they are common exciting causes of disease, in one child producing rickets, in second tuberculosis, in a third scrofulosis. The determining element is the constitutional habit, but this opens up the general pathology, which is not here the meeting.

Dr. GIBERT, Havre: Although I hold but a modest position as a man of science, it is my duty to state the results which a long clinical experience has given me. I am a convert of M. Parrot's, but a convert in spite of myself; it [illegible] facts that I have been converted. There are two important points to touch [illegible]. (1.) Can chronic enteritis produce the bony changes seen in rickets? [illegible] from that it cannot. Situated as I am at Havre, in a country where enteritis [illegible] considerable ravages every year, I am able to say that bad or unsuitable [illegible] produces those disorders which have been described by Chossart, and

by M. Parrot in his excellent work on "Athrepsie." (2.) Rickets treated by antisyphilitic remedies gets well much sooner than by any other method of treatment. This assertion rests on a large number of cures. M. Guérin has made use of this argument in his appeal to the saying, "Naturam morborum ostendit curatio." In conclusion, M. Gibert asked that at the next Congress anatomical specimens should be shown, demonstrating that rickets is produced by bad feeding alone.

Professor RANKE, Munich: I would express the opinion that large centres like Paris, where syphilis frequently occurs, may not be suitable places for the study of the etiology of rickets. I have myself a large field of observation at Munich, 4000 to 5000 children a year, and I see amongst that number a great many cases of rickets, and only comparatively few cases, perhaps twenty a year, of hereditary syphilis,—the latter disease being rare among the Munich population. A great many cases of rickets are also continually brought to my clinique from the country, children of well-to-do peasant proprietors, amongst whom syphilis may be said to be scarcely known. I have, on this field of observation, come to the conclusion that there is simply etiologically *no* connection between syphilis and rickets. Of course, with this statement, I do not mean to deny that hereditary syphilis may not, in some rare cases, show a certain resemblance to rickets, as Dr. Wegner has shown some years ago, and as some of the specimens exhibited by Professor Parrot seem to point out. But the greater part of Professor Parrot's specimens appear to me to demonstrate merely a co-existence of syphilis and rickets in the same subject. This co-existence of the two diseases may be frequent enough in very large cities, where syphilis abounds, but it remains, nevertheless, a mere coincidence. In conclusion, I would say that M. Gibert's method of coming to a conclusion regarding the etiology of rickets by way of therapeutical observation seems to me not a scientific one. If M. Gibert has frequently seen a cure of rickets after the administration of a few spoonfuls of his syrup, I can only say that he has seen a good number of cases of rickets getting well without any medicinal treatment at all.

Dr. SANSOM, London: The result of my observation at the North-Eastern Hospital for Children is opposed to the idea that syphilis is a constant cause of rickets. It is quite true that in a certain proportion of cases these are associated, but it appears much more probable that the true relation is, that syphilis is a cause of a general dyscrasia upon which rickets is engrafted. I consider that rickets is the attendant of a dyscrasia which is dependent on a great variety of morbid causes. I have found a prime phenomenon of rickets to be pyrexia; subdue the pyrexia (as you can by cold ablution), and treat the cases hygienically and medically with judgment, and the rickets becomes cured; but antisyphilitic treatment is by no means a *sine quâ non*. Whilst differing in my conclusion, I have to express my cordial thanks to M. Parrot for his admirable researches on bone-disease in syphilis.

Dr. ROBERT LEE, London: From an analysis of about 10,000 (10,833) cases of diseases of children which have come under my care during the last three years, I find 364 of rickets and 60 of syphilis. If I added to these the cases during the previous six years, we should have a total of about 1000 of rickets and 180 of syphilis. In addition to this I have the histories of 20 cases of family syphilis; in these only two cases are recorded of rickets. From this experience I am of opinion that when syphilis does affect the bones, which it does but rarely, then it produces the condition described by M. Parrot. These are syphilitic and special to syphilis, but these are not what we usually term rickets;

so that it seems to me as true to say that syphilis never produces rickets as to say it alone produces rickets. If we take the analysis of M. Parrot's views, we should agree with him in the first six paragraphs. As we go on we find him introducing a theory, and attributing to osteomalacia those conditions which we universally call rickets. We therefore have a difference of meaning in our terms, and thus our discussion must be unsatisfactory. In regard to the twenty cases of family syphilis, let me observe that the difficulty which some meet with in obtaining histories of cases has really no excuse, and that from my experience there is no difficulty in obtaining perfect histories if we only take the trouble to do so.

Dr. EDDISON, Leeds: I am glad to express my sense of the value of the discussion that was begun by the paper of M. Parrot. I object strongly to what I must describe as a vicious argument based on therapeutic results, and would point out that it is possible that the action of mercury may be most useful in cases of true rickets, in which there is no specific taint whatever. In all large towns in England there are ample opportunities of watching the course of cases over long periods of time, cases which certainly are improved by improving the food and the hygienic condition of the patients. Moreover, these cases do not present the various other symptoms of hereditary syphilis.

Dr. NORMAN MOORE, London: Every one who begins the study of rickets soon comes, I suppose, to some one hypothesis with respect to its cause. I must confess that I did so, and that my first conclusion was that rickets had but one cause, and that that was improper feeding during the period of suckling. Subsequent observation convinced me that there are cases in which one must seek some other etiology, and no doubt cases of syphilis are amongst these. It has seemed to me that there is one point which has been overlooked—that of the geographical distribution of the disease. It occurs everywhere; but when I read with great respect and admiration the observations of M. Parrot, and found him describe as common what is certainly rare, at any rate, in some regions of observation in this country, it seemed to me that the most probable ground of the difference is the fact that this disease, occurring at a period when the individual is so subject to the habits of the population in which he lives, is one of the most likely to be affected by local conditions and conditions of race. With regard to the question of syphilis, I think too little exactness is often used in the determination of the existence of syphilis in any given case. Applying the most exact tests which I could frame to the numerous cases of rickets which I have observed, I must say that I have been able to regard syphilis as a cause of rickets in but a small proportion of cases.

Dr. GOODHART, London: Without detaining the members of the Section more than two minutes, I can, and I think ought to, add something to the discussion with reference to craniotabes. And as a preliminary let me say, that while thinking, as every one must do, most highly of M. Parrot's work and preparations, I, in common with most English observers, have been, and am still, unable to accept the doctrine of the unity of rickets and syphilis. And when my friends Drs. Barlow and Lees published their observations upon craniotabes, a disease heretofore regarded as rachitic, but by them considered to be syphilitic, I was quite opposed to the belief in its syphilitic origin. However, the accuracy of all their work made me go into the question of craniotabes again more minutely; and of the cases that I have seen since then, some ten or twelve, they have all, with I think one doubtful exception, presented more or less evidence of congenital syphilis. Thus, so far, I am

coming round to the opinion that craniotabes is an affection associated with congenital syphilis; at the same time, I am sure, if such a statement is worth anything, that rickets to its most pronounced degree is often unassociated with any evidence whatever of syphilis, and occurs in families—of which each of us could, I feel sure, give examples—where our intimate knowledge of all their surroundings enables us to say with much assurance, that they are free from any taint of such a disease, spent or otherwise.

Dr. A. JACOBI, New York: Craniotabes is by no means a symptom of syphilis; the immense majority of cases have nothing to do with the latter. Mercury is used freely in cases of dyspepsia, as an antifermentative, as an antiphlogistic, and its alleged efficacy in rachitis does not prove the syphilitic nature of the latter. The counting of blood-cells after the use of mercury in small doses given a long time appears to show that their number is increased. Syphilis may be *one* of the causes of rachitis, but there are many others. Still Professor Parrot asserts that syphilis is the only one. I might just as well say that mothers' milk is the only cause of rachitis, because some babies get rachitical when fed exclusively on caseinous and fat milk.

M. PARROT, Paris: The arguments which have been brought against my views rest chiefly on two points, etiology and treatment. With regard to etiology, M. Gibert has already given a good answer to objections. It is well known that there is a large number of children weakened by chronic maladies, enteritis, pulmonary tuberculosis, bronchitis, Pott's disease, &c., who have no sign of rickets, while there are rickety children who are both large, fat, and of healthy appearance. These causes of debility produce osteomalacia—*i.e.*, the disappearance of a certain quantity of lime salts, the enlargement of the Haversian canals, and the substitution of soft tissue for the hard material. But they do not ever produce rachitis—*i.e.*, a lesion essentially marked by the existence of spongoid tissue; for without this spongoid tissue there is no rachitis. It has been said that rickets has been produced in animals (especially in dogs) by M. Jules Guérin; and as they do not get syphilis, the conclusion has been drawn that rickets is not always produced by hereditary syphilis. But M. Tripier of Lyons, who has made numerous experiments on the production of rickets after Guérin's method, has been unable to produce it. I too have tried to produce rickets according to this method on two litters of puppies. I have produced a slight degree of softening of the bones, but never rickets. With regard to treatment, it has been said that if rickets arose from syphilis we ought to be able to cure it by antisyphilitic treatment. I, for my part, say that we *can* cure the osseous lesions of hereditary syphilis in their two first stages—those, namely, which precede the rachitic period. But the syphilis has spent itself in producing rachitis, and is no longer syphilis. It is a new disorder, and I consider that here we have an example of the transformation of disease. Have we not similar instances in dermatology? I give iodine in rachitis, but I give it as we do in many other morbid states where syphilis is not concerned. I may add, however, that I have often doubtlessly referred its efficacy to the source from whence the rachitis springs.

M. BOUCHUT, Paris: Syphilis, considered from the point of view of a specific poison, goes for absolutely nothing in the causation of rickets. It only contributes to such a state by the cachexia which it involves. In this light it takes its place with scrofula, anæmia, and all chronic maladies which arrest the regular progress of growth and which produce a malnutrition of the body generally and the bony system. Among these cachexias, chronic enteritis is the

true cause. Whatever it is due to, prolonged diarrhœa arrests development an produces all the changes seen in rickets. To such an extent is this true, tha one may see a child of a year old, unaffected by syphilis and able to wal alone, suddenly fall ill, and be prostrated for five or six weeks; and when it convalescent, it is found that it can no longer hold itself up when the endeavou is made to induce it to walk. The bones are soft and painful, and it perha will be three months before the child can walk again. We see rickets produce under these conditions, and under these conditions we can bring it into exi tence. Again, rickets can be produced at will in dogs. If half of a litter puppies be shut up, and stuffed with food, causing diarrhœa, while the oth half are given their freedom and suitable nourishment, the former becon rickety, the latter undergo normal development. Where do we see here a pla for the syphilitic element? Syphilis, then, as such, is no factor in the produ tion of rickets, and I see nothing in such an affirmation but an hypothesis bad supported.

Dr. H. Rehn, Frankfort: I must maintain the non-identity of rickets a hereditary syphilis. It is most remarkable that a dispute should have taken pla as to the existence of rickets, on the soil where the definition of the disease Glisson originated. With regard to the most creditable researches of Parrot, must refer to Wegner's investigations on hereditary syphilis in young childre published some years ago. In Parrot's description I miss the termination necrotic caries and detachment of the epiphysis. According to Wegner, it is o in the first stage (*i.e.*, the stage of proliferation in the intermediary cartilage) th a similarity exists between the syphilitic and rickety processes at the epiphysi later the two processes are widely separated. Clinically they differ, in opinion, in the marked local signs of inflammation accompanied by fever whi are confined to syphilis, in which disease also characteristic lesions of skin a mucous membrane are rarely absent. Moreover, the earlier period of life is the proper time to decide this question. Those cases are especially confir tory in which the well-known signs of rickets present themselves at the end the first or second year, at the time of weaning, from sudden alteration nutrition, improper food, or after a gastro-intestinal disorder following a gr acute disease. With regard to the accurate decision of these points, let me po out the favourable position of the family doctor. Professor Parrot's opin regarding the cessation of syphilis at the moment it produces rickets, his s position of a *transformation*, and his statement regarding the non-contagi nature of this rickets, is the making use of unintelligible and inadmissible notio Professor Parrot's additional three propositions regarding the relation of ost malacia to rickets are also highly confusing, since they place us on entirely ground. I would ask whether, if (as Professor Parrot states) the causes wh have been considered those of rickets can induce osteomalacia only, the c clusion does not logically follow that the usual definition of rickets resolves it into syphilis and osteomalacia? With regard to the third proposition of fessor Parrot, "Osteomalacia may be produced artificially, rickets never," I m refer to the recent experiments of Dr. E. Voit of Munich, with microscop examinations by Professor Buhl.* My views lead me to the conclusion rickets and syphilis are processes of slight similarity only with regard to t localisation in the skeleton, but in their etiological, clinical, and therapeut aspects differing totally.

Dr. West, the President: You will, I trust, not think that I arrogate a r to myself in consequence of the honour you have done me in appointing

* *Zeitschrift für Biologie*, 1880.

your President, if I ask permission not so much to give a *résumé* of what has been said by previous speakers, as to express my own opinion with reference to some of the questions which have come before us, and which have been discussed with so much ability and in a tone which eminently becomes a scientific congress. It has struck me that some light might be thrown on the question of the relation of syphilis to rickets, and something might be gained, by an appeal to history; for if syphilis were its invariable cause, rickets ought not to have been known before the siege of Naples. Now, although we owe the first formal description of the disease to our countryman Glisson, I think one finds traces of a disease affecting the young and producing deformities like those of rickets long anterior to that memorable event. Another point which seems to me worth notice is the important one raised by Dr. Norman Moore, and concerning which I had hoped he would have given us further information—namely, *the influence of climate and geographical distribution.* My impression is that rickets in Paris presents a less severe character in general than that which it commonly presents in London. A Brazilian physician whom I met in Paris told me that while syphilis is extremely common in the Brazils, rickets is an unknown disease; and it would throw much light on the question if it were ascertained that in the islands of the Pacific, the inhabitants of which are known to have been decimated by syphilis, rickets was rare or unknown. With reference to the indications of past syphilis on which M. Parrot relies, some of them, as the condition of the tongue, which he pointed out to me as syphilitic, did not to me appear conclusive, as I think I have seen this in isolated members of a family which was otherwise healthy, in connection with symptoms of indigestion, and wholly independent of any symptom of syphilis past or present. With reference to the one special condition, craniotabes, I do feel a certain hesitation as to its possible connection with bygone syphilis, but my feeling is one of hesitation and nothing more. As to the general manifestations of rickets, they do not in general show themselves with distinctness before the second year, often even later, and in the great majority of instances are ushered in by a general febrile disturbance, and by other symptoms indicative not of a special affection of the osseous system alone, but of a general constitutional disease. Moreover, there is one peculiarity already referred to by M. Guérin which is characteristic of all the deformities produced by rickets—namely, that they are attended by an arrest of development. This, however, is not the case with children who may yet be most markedly syphilitic, and who have grown up to the age of six or seven years, or older, with evident tertiary syphilitic symptoms untreated, or certainly uncured. The arrest of development, too, does not affect the whole skeleton alike; the vertebral column, though twisted and deformed, is not shorter than natural were it but stretched out, while the pelvis is dwarfed, and the long bones present a length far below what is natural. Besides, the possibility of producing rickets in animals by feeding them on unsuitable and consequently unnutritious food, and their recovery when properly dieted, points in the same direction; and the experiments of M. Guérin and M. Wegner, and the later ones of Rollo of Berlin and of Voit of Munich, all lead to the same conclusion. For all these reasons, I can come to no other conclusion with reference to the opinions of our distinguished colleague M. Parrot than that, in the words of the Scotch verdict, they are *not proven.* Once more I apologise for having spoken; but I thought that having grown grey in the study of children's diseases, you would expect me to have an opinion on this important question, and to express it.

Dr. REHN, Frankfort-on-the-Maine: Demonstrated the skeleton of a child of probably 1½ or 2 years of age, the only hitherto known case of osteomalacia in

childhood. Microscopically the absence of the common rickety changes at the epiphysial line is striking. Professor von Recklinghausen of Strassburgh made the microscopical examination, and considered it to be real osteomalacia. The skeleton was demonstrated for the first time at the Congress in the Section of Diseases of Children, and the case is described and discussed by Dr. Rehn in the chapter "Rachitis" of Professor Gerhardt's "Encyclopædia of Children's Diseases."

Conditions Governing the Occurrence of Paralysis and Albuminuria in Diphtheria.

Dr. West, the President, opened the subject by suggesting the following questions:—What governs the *albuminuria?* Does it bear any invariable proportion to the severity of a case? It always exists to a high degree in malignant fever, and appears slightly in all where the difficulty of respiration becomes extreme; but otherwise—What rule is there about it? Does it appear in some epidemics more than in others? Does it, and if so, in what circumstances lay the foundation of abiding albuminuria, as that of scarlatina does? Is there a connection between it and subsequent occurrence or degree of paralysis?

As to paralysis, my own impression is that it is more frequent in France. It differs decidedly in different epidemics. It occurs sometimes with formidable risks in early stages of not apparently very severe diphtheria. When it comes on as sequela, it usually, but not invariably, begins at and extends from the seat of original deposit. But why does it become generalised? Does it leave post mortem appearances in the nervous system? Why does it come on at a longish interval of time? What is its pathology?

Paralysis and Albuminuria in Diphtheria.

Dr. A. Jacobi, New York.

I.—*Paralysis.*

Local paralysis of the soft palate, and sometimes the muscles of deglutition and epiglottis, often attends the local deposit on these parts, with œdematous swelling of the same.

The diphtheritic paralysis, properly so called, is an affection of apparent convalescence. The majority of cases occur after mild attacks, sometimes after those with but little fever and a slow general course. It does not usually occur in those complicated with albuminuria and nephritis. There is no symptom indicating its occurrence in the future.

II.—*Albuminuria.*

Albuminuria is often found in diphtheria, but is mostly no grave symptom. It may be the result of high temperature or of suffocation, as in other diseases. It often appears in the first few days of the disease, not always with high fever, and appears to be the result of rapid elimination of the poison. Acute diffuse nephritis makes its appearance at an earlier period than it does in scarlatina.

Albuminuria in Diphtheria.

Dr. JOHN ABERCROMBIE, London.

e recently had under observation ninety-one cases of diphtheria, in all the urine was examined.

minuria was found in twenty-four. Of the ten patients in whom the came albuminous after they came under observation: in one, albumen within twenty-four hours of the onset; in three, not until the tenth day e; in the others, it appeared between the fifth and eighth days. Judging cases where albuminuria was present when the patient first came under ion, it is evident that it is usually developed early in the disease, and rance so late as the tenth day must be looked upon as rather exceptional. uria does not, as a rule, last for long; in one case it only lasted two d in no case more than a fortnight. In no case was any anasarca , nor was the urine ever smoky, though a few blood corpuscles with l and blood casts could be seen under the microscope.

re never seen any symptoms of uræmia. Examination of the kidneys th revealed the ordinary changes of acute parenchymatous nephritis. In that I examined with my friend Dr. Samuel West, we found the vessels in the cortex crowded with minute shining bodies, which we be micrococci. I may add that the autopsy in this case was made seven ter death, and that the kidney was only examined after hardening in acid and spirit. It was the most malignant case I have ever seen. Of ty-four patients in whom albuminuria was discovered, no less than died, whilst of the sixty-seven in whom there was no albuminuria eight that it is evident that the danger to life is much greater when there is uria than when there is not, but it is also evident that freedom from uria is no guarantee that the patient will recover.

On Diphtheritic Paralysis in Children.

Dr. JOHN ABERCROMBIE, London.

unusual opportunities for studying diphtheritic paralysis that I have the Hospital for Sick Children, Great Ormond Street, will, I hope, ne in bringing the results of my experience before the members of this

ave to express my thanks to the members of the medical staff of the l for their kind permission to make use of the cases.

total number of cases included in this paper is eighteen, of which nine proved fatal. Nine patients were boys. Half of them were the ages of two and four years; with the exception of one boy who was rs and three months old, the others were under seven years of age.

y one patient was under observation during the diphtheria; it is necessary, e, to analyse the histories of the other patients, as they all came under ion after the development of the paralytic symptoms. Four patients n treated at home by a medical man for diphtheria. In two cases the ad been under treatment at home for diphtheria. In four cases there istory of sore throat, nasal discharge, and lumps in the neck. In three history of sore throat, lumps in the neck, and fever with insanitary

conditions in house or neighbourhood. As regards the remaining four patients, in two there was a history of sore throat and swelling in the neck, in one of languor and sore throat, and in one of lumps in neck only. Most of the patients had not had any treatment for the primary illness, and several of them had not even kept their beds.

The *onset* of the disease is always gradual and insidious, and it is difficult, therefore, to fix with any degree of accuracy the earliest date at which the paralytic symptoms were noted. Excluding cases where thickness of speech was said to have persisted from the commencement of illness, I find that in two cases the paralytic phenomena appeared in two weeks from the onset of illness and that in four cases they were not observed until five weeks from the onset the other cases occurred between these limits.

In fourteen of the cases the *earliest symptom* was either nasal voice, return of fluids through the nose, or some difficulty in swallowing; in the remaining four, weakness in the legs was the first thing noticed.

In the majority of cases, the order of symptoms is much as follows:—Thick and nasal speech, cough when drinking, return of fluids through nose, weakness in legs, back, and arms. Occasionally, however, this order is almost exactly reversed. Thus in one of the very few cases in which I had the opportunity of watching the symptoms develop from the commencement, there was first weakness in the legs, then weakness of the arms, cough, alteration of voice, impairment of vision, difficulty in drinking, and finally inability to sit up or hold head up.

The only constant symptom was paralysis of the soft palate, which was present in all the cases. In twelve the soft palate was ascertained to be anæsthetic; in some of the other cases the point could not be determined.

As regards the relative frequency of other symptoms:—

Irregularity of the heart's action was noted in six patients. In one it was so marked that the case was at first thought to be one of organic heart disease. It is an early symptom, and often subsides after the patient has been kept in bed for a few days.

Albuminuria was found in four patients, one of whom recovered. In several of the fatal cases the urine could not be obtained; its absence was noted in seven cases, two of which proved fatal. It is not very easy to see why it should occur in this disease; possibly the patients in whom it was noted had already had nephritis with their diphtheria, and it may be due to a relapse of this condition, or the nephritis may never have cleared up. In one case hyaline granular and epithelial casts were found microscopically, but I regret that I have omitted to examine under the microscope the kidneys from any of the fatal cases. must always be a symptom of grave import.

Incontinence of urine or fæces, as a rule, only occurs in the last stages of the disease.

The *pupils* are always *dilated and sluggish*; in two cases there was marked inequality. I have never been able to recognise any changes in the fundi oculi with the ophthalmoscope.

Convergent strabismus was present in seven cases; no other form of squint was seen. It has been pointed out to me that partial paralysis of accommodation in a hypermetropic person would produce this squint, and in one patient with squint it was ascertained that he was hypermetropic. It is not very easy in children to determine whether there is paralysis of accommodation or not, but in five patients this symptom was ascertained to be present, and three of them also had convergent strabismus.

Facial paralysis was noted in three cases; it was always slight and transitory.

Paralysis of the muscles of the chest-wall was present in ten cases; in one of the patients, who had had for some days paralysis of his intercostals, the diaphragm was paralysed for twenty-four hours, the patient breathing by the aid of his neck muscles alone. This patient was the only one of the ten who survived, and in my experience this symptom is the most formidable one that can arise.

The *knee phenomenon*, or patellar tendon reflex, as it has been called in this country, was found to be absent in nine cases; it was not sought for in the others. It is often, however, so difficult to obtain this knee jerk in healthy children, that we must be careful not to set too high a value on its absence.

The *cutaneous reflexes* were noticed to be natural in three cases—slight in one, absent in two.

Cutaneous sensibility was but little if at all affected in any of the cases.

The response of the leg muscles to *faradism* was found to be good in four cases; in one case where the constant current was employed the excitability was diminished.

Duration of fatal cases.—Two patients died about the ninth day after the onset of the paralytic symptoms; one between four and five weeks from their first appearance; in the other cases death occurred between these limits. In all the cases death resulted from pulmonary catarrh, in seven cases supervening upon paralysis of the diaphragm, in the remaining two upon paralysis of the intercostal muscles.

Duration of the non-fatal cases.—In the majority of cases the symptoms lasted from five to six weeks; in one patient they had disappeared at the end of three weeks, in another they persisted for fifteen weeks; this was by far the most severe of the cases that recovered.

As regards *post-mortem appearances*, in all the cases there was marked pulmonary congestion, and in two the heart was empty. There were no lesions of the nervous centres discoverable with the naked eye. In seven cases I have examined the medulla oblongata and spinal cord methodically after hardening in Müller's fluid; the sections were stained with hæmatoxylin.

The only pathological changes I have been able to detect are in the grey matter of the anterior cornua, and they consist in a swollen condition of the large motor cells. The margins of these are very ill defined, and the processes have in most instances entirely disappeared. The contents of the cell have a granular aspect and the nuclei have disappeared, or, where still visible, are highly granular. These changes only occur in very limited areas, and I have not found them constantly in any one region of the cord, but more commonly in the upper and middle dorsal regions than elsewhere. Where one cell of a group is affected, all the cells of that group show some change. In some places the cells appeared shrunken rather than swollen. Perhaps this is a later stage of the same process. In some specimens the neuroglia corpuscles were perhaps unduly numerous, and the central canal was sometimes found blocked with a homogeneous substance; but I am not prepared to lay any stress upon these points. No lesions of the white matter of the cord were recognised. The blood-vessels were in most cases filled with blood, but no extravasations were seen. The examination of the medulla oblongata yielded negative results with one single exception, where some of the outermost cells of the vagus nucleus were found to be rounded and more or less completely deprived of their processes.

Of the other two fatal cases, I examined the cord, in conjunction with Dr. Cheadle, in one; but our examination was not so exhaustive as it might have been, and we did not find any morbid condition. The other patient died during my absence from town, and I had no opportunity of examining the nerve centres microscopically. I have not examined the nerve roots or muscles in any of the cases.

The occurrence of hemiplegia in connection with diphtheria is so rare that I would briefly mention the following case:—A delicate boy, seven years old, came under treatment on the third day of a severe attack of diphtheria, the urine being albuminous when he was first seen. At the end of a fortnight all membrane had disappeared and the urine was free from albumen. On the sixteenth day he was convulsed, after which he complained of right frontal headache, and there was left facial paralysis. On the following morning he was again convulsed, after which there was complete left hemiplegia. Ultimately naso-pharyngeal paralysis appeared, and he died from paralysis of his intercostal muscles on the twenty-eighth day from commencement of illness. The right middle cerebral artery was found occluded at its bifurcation by a firm, pale, adherent thrombus.

I would briefly call attention to an account of an examination of three fatal cases by M. Déjérine in which he describes changes similar to those which I have found, but more advanced. He has further investigated the state of the anterior and posterior roots, and found the latter invariably healthy, from which he infers that there cannot be an ascending neuritis in this disease; but I am not aware of any facts which tend to prove that inflammation cannot travel up a motor nerve. A much stronger (indeed in my opinion conclusive) argument against the theory of ascending neuritis is to be found in the cases (which are not so very uncommon), where the paralysis does not commence in the palate, but in some remote part, such as the legs, for instance.

It has been said that paralysis may follow a simple angina, but when we recollect how common it is to find well-marked diphtheria in one child of a family, while its brothers or sisters have only a slight tonsillitis evidently due to the same poisoning, I think it is clear that in many cases an apparently simple angina may in reality be due to diphtheria. Every one will admit that a child may have scarlatina without the rash being noticed. Why may not a child then have diphtheria without the throat condition attracting notice?

As regards prognosis, my experience leads me to look upon this affection as one of the most formidable ones a child can have. As to whether paralysis is a common sequel of diphtheria, I do not find myself in a position to give a positive opinion. From the extreme rarity of its occurrence in children who come under observation for diphtheria, one would think not. But if it is so very rare, what an extraordinary number of cases must go unrecognised! I think the probable explanation must be that the good feeding and care taken of the children in hospital prevents it. Of course one must bear in mind that about 28 per cent. of the children die from their diphtheria before sufficient time has elapsed for the paralytic symptoms to appear.

With regard to treatment I have but little to say. Belladonna in one case, given in large and oft-repeated doses (half a drachm of the tincture every two hours), certainly seemed to act beneficially. At any rate, the child recovered after having had paralysis both of diaphragm and intercostal muscles. But in other cases it has failed, nor has the hypodermic injection of atropine proved of much use. Strychnia, iron, iodide of potassium, and arsenic have all been tried, but I doubt if they exercise much influence. The patients should be kept in bed, and especially protected from draughts; they should be given a liberal diet and stimulants.

I would submit that the foregoing observations justify me in drawing the following conclusions:—

1.—That diphtheria leaves behind it a special tendency to subacute myelitis, which has certain definite clinical characters, and that this tendency is at least as great after a mild attack as after a severe one.

2.—That the prognosis should be guarded.

3.—That the same precautions should be exercised in the after-treatment of diphtheria, with a view to prevent paralysis; as are observed to prevent the occurrence of nephritis after scarlatina.

On Paralysis after Diphtheria.

Dr. William Squire, London.

Diphtheria is not the only acute specific or febrile disease followed by paralysis of the kind so well known as its sequela; but it offers the type of this kind of paralysis, and that with such frequency as to make us look for it, in some degree, as an almost constant consequence of diphtheria. We find it also as a rare exception after rheumatic and enteric fever, erysipelas, uncomplicated scarlet fever, quinsy, and croup.

With all these diseases diphtheria has this much in common: sudden onset of high temperature, the increased waste of general fever, tissue change at the seat of local mischief, high vascular tension, and renal congestion, often with albuminuria, and always with impaired elimination.

To the general considerations I have to offer, time has added great results from the advance of nerve pathology, and so brought into clearness much that was most obscure. Accumulated clinical observations have also largely co-operated towards a fuller understanding of diphtheritic paralysis.

We shall best begin by taking some of the simpler instances of disease followed by paralysis. After deep-seated inflammation of any part, an alteration of sensation and motion will persist long after apparent recovery; nor is this simply the physical result of some exudation still left; altered sensation in one finger has persisted so long after erysipelas of the hand and arm, as to make a limited injury to nerve or nerve-trunk almost certain.

In acute rheumatism with any cardiac inflammation, remarkable slowness of pulse is very likely to follow, with ready after disturbance. The ends of the vagus at the seat of inflammation first suffer, and then the ganglionic innervation. After or during inflamed throat from any cause, there is dysphagia from effusion in the muscular structure, from imperfect muscular consensus, and fluids will return by the nares; the sensibility of the parts invaded by exudation is lessened, and the proper reflex is not readily excited. Sometimes, as in the case of erysipelas given, the nerve filament of the part is injured; and, in diphtheria at least, nutrition changes may reach the nerve origin and extend there, causing further mischief in the centres themselves. Diphtheritic paralysis may from its very commencement depend upon centric changes, presenting so far an analogy with infantile or essential paralysis, but in diphtheria the symptoms do not come on early, and will completely disappear. The centric disturbance in diphtheria is more often functional from temporarily impaired nutrition than from active irremediable organic degeneration, and is attended by loss of reflex and not by exaggeration of it, or by rigidity. An exact counterpart of this form of paralysis, as seen in diphtheria, has lately occurred to me in a young man twenty-five years of age—after rheumatic fever complicated by albuminuria. The part played in this case by renal congestion and consequently diminished elimination in delaying recovery and determining the paralytic symptoms is incontestable. Three weeks after the attack some numbness of fingers and then of the feet occurred; this quickly passed off, and then loss of power in varying muscles was noticed; the hands and abdominal muscles soon recovered, but the legs were altogether powerless; patella-reflex was lost; the extensors of

legs and feet, though acting tardily to the interrupted current, remained almost useless for three months. As in most such cases of diphtheritic paralysis, recovery was complete. Paraplegia after or during enteric fever beginning with more acute spinal symptoms, has disappeared less completely in longer time, and not without recurrence of some symptoms in later years. In rheumatic fever this more acute paraplegia of spinal origin is shown to be not very rare by a proneness to sacral bed sores in certain cases, with increased pain on turning the patient in bed, and a persistence of fever.

There are three ways in which these paralyses occur :—

1.—By the general effect of waste matters in the blood, where fever has been suddenly high, and the means of elimination insufficient.

2.—By the effect of local inflammation on the innervation of the part, with or without changes in the nutrition of the nerve-tissues.

3.—By nutrition changes in the nerve-centres as a consequence of altered blood supply during or after the illness. This may be one of the consequences of the first state, but it may also result from embolism, or from a local change in the blood-vessels, or from changes in the corpuscular elements in the blood, and not from any poison in the blood. Some of these changes may be attributable to special matters in the blood, as in the case of embolism from detached fibrin, but this generally leads to hemiplegia, and though frequent after rheumatic fever and not unknown in scarlet fever, is rare in diphtheria, and is not here under consideration. Where a special poison is imagined, the facts are less certain. It is the question of special poison I wish here to speak of. It is not the special poison of syphilis that paralyses; any irritant, or the accidental bleeding of a vessel, may determine the proliferation of cells in a syphilitic subject that will encroach upon the nerve structure and destroy it, if not removable by treatment. Why imagine paralysis the result of a special ferment in rheumatism, or of a special poison in diphtheria, when in both cases the waste products of those fevers remaining in the blood or tissues suffice to set up the changes on which those paralyses depend? No one imagines a special poison for infantile paralysis. There is an observation of Dr. Buzzard of excess of uric acid accompanying this kind of seizure, of some value as an illustration here.

The mere relaxation of small vessels during fever with excited heart leads to their dilatation; afterwards, a slower current may cause congestion, and either favour effusion, or prevent the removal of waste products which remain in the tissue to excite irritation; or, the blood itself may contain such waste, and so exclude, contract, or dilate the arterioles. This view harmonises well with what is seen in diphtheric paralysis; but there is another possibility to be contrasted with this, and also with the special poison theory, and that is the influence of a special germ or organism directly concerned in producing this as well as other phenomena of the disease. Given the dilated glomeruli in the febrile stages of diphtheria and scarlet fever, albuminuria with intertubular congestion and clouded epithelium with desquamation might be expected without the intervention of special microzymes to permeate the tissues and cells, block the vessels, and disintegrate the structure; though such particles are seen in the epithelium cell or in the nerve fibril, they may mark the degeneration without being the cause of it. To those who look for either germs or a special poison in all the manifestations of diphtheria, certain paralyses are as pathognomonic of the disease as fibrinous exudation; on the other hand, paralysis *per se* is no more pathognomonic of diphtheria than fibrinous exudation is. Diphtheritic paralysis is of two kinds: the one local, appearing early, directly produced, and of a gravity *proportioned* to the intensity of the local or general disease present; the other general, or secondary, coming on later, not before the second or third week, and *not in proportion* to the severity either

of the local lesion or of the general disease. Sometimes, as in the paralysis of the ciliary muscles, the loss of power of accommodation, and consequent impairment of sight, this accident, like the desquamation of scarlet fever, is the only sign by which we know that a patient has had diphtheria. This second form is much the same whatever the original seat of mischief.

The leading characteristics of post-diptheritic paralysis are: first, the passing and shifting character of these paralyses; they are generally functional, and of good prognosis, pointing to a temporary disturbance of nutrition, such as a passing congestion of nerve centres, or a limited vasomotor disturbance. Second, the time of their occurrence; this is at the end of two or three weeks, or even as late as six weeks, when the balance of healthy function throughout the body is being restored. It is to be noted that when this period is retarded, and the patient very weak, these functional paralyses are more likely to be prolonged, and may be associated with impairment of nerve structure, when co-ordination, or patella-reflex has been long in abeyance, and the reaction of certain muscles to faradisation almost lost. In many cases of diphtheria, where paralysis has come on late in the course of the disease, albuminuria has existed to a greater or less extent at some part of the illness; this may either persist at the time of the paralysis, or have already disappeared. Diphtheria may also be followed by paralysis when no albuminuria has existed at any time; some of these cases have occurred early, associated with a certain intensity of the local process. Terminal filaments of a nerve may be destroyed by ulceration, or their structure and that of a mixed nerve fibre invaded by the degenerative changes set up by inflammation; but short of this, the function of the nerve may be in abeyance as a consequence of over-stimulation, and here also the nerve centre is concerned. Hence the frequency with which mere return of fluids by the nose is followed by altered voice and impaired deglutition rather than by the more serious affection of the vagus, and loss of power in the muscles of the neck, or in those of respiration. In the earlier paralyses mixed nerve-trunks are implicated, and not the nerve centre to the same degree, for there are alterations of sensation, and the muscles readily respond to the stimulus of the interrupted current; in the severer cases this response is weakened; the centre involved is in the medulla or near the root of the phrenic; death may occur before any marked degeneration of the grey matter is established. Actual disintegration of both grey and white nerve structure and of the nerve axis has been shown by Sir W. Gull, Professor Charcot, and more recently and completely by M. Déjérine. Whether in these cases the degeneration has been propagated upwards by the destroyed nerve, or the nerve has suffered from injury at its ganglionic centre, is not so clear to me; probably both conditions occur, the latter most frequently; the former being more likely in the early incidence of paralysis after severe local lesion. There is a form of early paralysis, with dangers of its own, not connected with direct organic nerve lesion. Signs of cardiac paralysis, as early as the first week, arise from degeneration and infiltration of tissue, implicating the ends of nerves and not their centres. During paralysis of the respiratory muscles congestion of lung may set in, and not clear off when movement of the chest is restored. The ganglionic centres, either of the heart or the great sympathetic, may be affected; the former is shown by fatal cardiac asthenia apart from thrombosis, the latter by a case of Dr. Heyl (*American Journal of Medical Science*, April, 1880), of unilateral exophthalmos and chemosis.

There are thus clinically three great classes of post-diphtheritic paralyses:—

1.—Those occurring soon after the appearance of the local manifestation of the disease, even when there has been no serious local lesion, and with or without albuminuria.

2.—Those occurring somewhat later with grave local and general symptoms, likely to be associated with degeneration or disintegration of nervous structure.

3.—Those occurring after the second or third week of the disease, often as late as the sixth week, where there has mostly been albuminuria in the course of the disease, but not always a severe, and sometimes even a trivial initial lesion. These cases are mostly readily restored by the use of tonics, as iron and especially strychnine, and by the use of the continuous galvanic current if not so aided, nerve degeneration may result, generally a descending paralysis with wasting of muscles. Cases of this kind form the bulk of those to which our attention is called after the first dangers of the disease are over.

These three clinical and pathological classes, if not exactly thus counterparts of each other, show a parallelism sufficiently close to be reasonable in theory and useful in practice. Diphtheritic paralysis does not always depend upon one and the same cause.

DISCUSSION.

Dr. G. B. Barron, Southport: My experience is that diphtheria is not so frequent in England as it used to be, and that there is now no place in the country where it may be considered to be an epidemic disease. Many cases are called diphtheria which are not true diphtheria at all, with small patches of leathery ulcer, and even with slight albuminuria, but with no subsequent paralysis. My opinion is decidedly opposed to the prevalent idea that croup and diphtheria are identical. No facts that I know of convince me of this, and the result of a prolonged observation is negative. I have not seen a case of true diphtheria for fifteen years, but many that partook of some features of it. It is also much less frequent in the vicinity of Manchester than formerly, nor are such cases when seen of so malignant a character. As regards treatment, my experience leads me to rely upon great precautions against movement, properly administered nourishment, cleanliness of the fauces by washing with a weak solution of nitrate of silver, but not the application of the solid caustic, internally iron and chlorate of potash, strychnine in the later stages. The treatment has been of a very varied kind, some men using one thing, some another. Indeed the treatment of this disease is the odium of medicine, and wants some more defined principles. Tracheotomy is rarely necessary. I saw a case bearing upon this question, where a cast of the whole length of the trachea with a short cast of the upper part of the bronchi in a perfectly cylindrical form was coughed up, in a woman forty years of age. It was well marked with the ring indentations of the trachea; microscopical examination failed to show ciliated epithelium. The case was of course fatal. It was a case of acute fibrinous tracheo-bronchitis.

Dr. C. A. Leale, New York: I wish that I could agree with the last speaker in regard to the diminution of diphtheria in New York City, for nearly every month in the year I see cases of the disease, and occasionally the entire number of children in a family are lost on account of its intense malignancy. I was recently called in consultation by Dr. Fruitnight where the last of three children was in the comatose condition of the intensely poisonous stage of the disease, and where, from the bad hygienic surroundings, recovery was almost impossible. In regard to the occurrence of albuminuria, I have found it present in a very large proportion of the worst cases; the albumen being often 50 per cent. Of these cases, some may expect to recover. In regard to casts, the degeneration of the kidneys may be watched by their increase, number, and character. Some authorities state that fatal results follow if a great abundance of casts are present. In regard to the occurrence of paralysis, I have seen it commence in the vocal chords most frequently. I have observed a number of cases of paralysis

following diphtheria, most of which have recovered. In regard to treatment, I have found strychnine and iron most useful, combined with other tonics to improve the general condition. I have observed large accumulations of serum in the different serous cavities in the following order of frequency—peritoneum, pleura, pericardium, and arachnoid. I have seen instances of each recover.

Dr. A. JACOBI, New York: In regard to Dr. Barron's remarks, and the frequency of fibrinous tracheo-bronchitis: The literature is small, the first large monograph on the subject being by Professor Biermer of Breslau. In the spring of 1879 I have seen three cases in children, probably half-a-dozen altogether. About as many more I have seen in the post-mortem examinations or in fresh specimens. In regard to diphtheritic paralysis: The main diagnostic symptom, though the soft palate or the muscles of accommodation commence to be affected first, is the intact condition of the sphincters, and the irregularity in the order in which the several localities are taken. If there were nine fatal cases in Dr. Abercrombie's eighteen, it is probably because his cases were carried to the hospital when in very bad condition. I have seen a few cases of paralysis of the respiratory muscles, and all of them died, I believe, when the affection was fully established. The average cases of diphtheritic paralysis are mild, will get well in from five to ten weeks or more, and require tonic stimulant and nervine treatment,—the faradaic and galvanic currents, iron, and strychnia (either internally or *subcutaneously*). The latter is contra-indicated, it appears, in the usual forms of acute or subacute myelitis, but it is *certainly* the very best nervine in diphtheritic paralysis. The case of hemiplegia will, I think, not be claimed as diphtheritic by Dr. Abercrombie. There were two attacks of convulsions, which are the frequent cause of partial or complete paralysis, or death.

Dr. S. SPRATLY, Birkenhead: said that out of an experience of between 120 and 130 cases of diphtheria, and thirteen deaths, he had only seen two cases of paralysis, one severe and one mild.

Dr. C. WEST, President: There is no doubt of the contagious nature of diphtheria, and the possibility of infection direct from one person to another; also that this stands in some connection with damp, locality, and season of the year. The essential nature of the contagion is unknown, but while everything turns our attention in the direction of Pasteur's discoveries, it must be remembered that the presence of bacterian or of any other microscopic growth in the membrane, does not prove that to be the cause. A question to be considered also is, how far does the fact that one attack does not preserve from another tell against the bacterian theory? In the case of charbon, in which the contagium is of a bacterian nature, inoculation preserves the subject from an attack.

Observations on the Spread of Diphtheria.

Dr. HUBERT AIRY, London.

§ 1.—The remarks which, with your permission, I venture to offer upon the circumstances under which, in the rural parts of England and Wales, diphtheria is usually found to take an epidemic character, are directed more especially to the question whether or not diphtheria is spread by the agency of the wind; and from the facts adduced, an attempt will be made to form some tentative inferences as to the nature of the diphtheritic contagium.

The subject is one for which I must claim no small degree of international importance, for nation has given to nation this fell disease, and nations have been made fellow-mourners by the ravages it has caused. Let it be an object of international zeal and rivalry to track the paths and arrest the progress of this common enemy of mankind!

§ 2.—Already, in specifying the "rural" parts of the country, I have reminded you of one circumstance which curiously distinguishes diphtheria from other infections, namely, its greater frequency among a population that is scattered in small villages and isolated homesteads than among the inhabitants of our crowded towns.

It is almost an axiom of epidemiology that the prevalence of infectious disease in a community bears some direct proportion to the density of the population in the district. This holds good, on the evidence of the Registrar-General's returns of mortality, with regard to epidemic, endemic, and contagious diseases generally. From these diseases the mortality in large towns is double that in the country. In the very first report of the Registrar-General, and much more elaborately in the fifth report, this contrast is exhibited by Dr. W. Farr.

Diphtheria, on the other hand, by the same evidence, affects our rural population more than our towns. For example, during the ten years 1861–1870, the average annual death-rate from diphtheria in London was 1·75 in every 10,000 of the population. In the south-eastern group of districts, in the same period, the rate was 2·00. In the eastern counties, which contain fewer large towns, the rate was 2·95.*

We have yet to learn the cause of this paradoxical inequality of incidence of diphtheria on town and country. Nothing is more certain than that the diphtheritic infection, like others, is favoured by personal intercourse, and inasmuch as personal intercourse is more frequent in towns than in the country, the natural consequence should be a greater prevalence of the disease in the former than in the latter. The actual reversal of this forecast proves that there are other factors at work in diphtheria besides personal communication—factors which operate to the disadvantage of rural as compared with urban communities.

§ 3.—The contrast is well set forth by Dr. Charles Kelly, Medical Officer of Health for the West Sussex Combined District, who draws a marked distinction between two classes of cases known by the same name:—

"1.—Those which seem to depend upon inhaling sewer gas, and where the patient suffers from a form of blood-poisoning. Such cases are generally confined to the house where the local defect in the drainage exists, and there is no extension of the disease. Other inmates may be attacked, because they are all exposed to the common cause. *To this class belong those cases which break out in towns.*

"2.—Those which seem to occur independently of any defective drains or water-supply, and which are most common in bleak and exposed situations. Such cases are met with in clean, well-built cottages, as well as in poorer tenements, and very often the disease breaks out with greater severity among the well-to-do than among those who are badly housed and clad. . . . *This form of disease, unlike the former class, appears in an epidemic form.*" †

§ 4.—It sometimes happens that diphtheria will spread from the country into the immediate neighbourhood of a town, and yet will seem unable to take root or extend itself epidemically in the town itself. A singular case of this kind occurred within my own experience. Diphtheria broke out among

* See "Supplement to the Thirty-fifth Report of the Registrar-General," pp. 4–11.

† Report for 1879, p. 90.

scattered homesteads within a mile or two of a small market-town (Builth) in Brecknockshire. The children affected were attending the village school. Other children from the immediate outskirts of the town attended the same school, and were mixed indiscriminately in school with those who came from the hill-side farms and cottages. Yet not one of the town children caught so much as a sore throat.*

I need hardly say that instances to the opposite effect are by no means infrequent, but in the long-run the general result tells in favour of the towns.

§ 5.—While the rural districts as a whole suffer more severely from diphtheria than the towns, there are found certain tracts of country which are almost wholly free from this disease, and others in which, with brief intermissions, it is almost constantly present, year after year. Among the former I may cite, from the Registrar-General's returns, the district of Fordingbridge in Hampshire, which, with a population of 6409, had only one death from diphtheria in the course of ten years, 1861–1870. Alresford district in the same period lost only two out of 7226. Andover district lost only seven out of 17,361. St. Alban's district, only four out of 20,003.

Compare with these such regions as the country around Chelmsford, where, out of a population of 33,269, diphtheria claimed 170 victims in the same ten years, a death-rate of 5·11 per annum in 10,000. Certain parts of Kent and Sussex, and many districts in Wales, are similarly noted for epidemic diphtheria.

The localities thus affected are for the most part described as cold wet clay lands; but there is evidently great variety in the soils on which diphtheria can prevail, for it is found in full force on the chalk downs of Kent, on the loamy sands and clays of the Sussex Weald, on the alluvium and boulder clay of Essex, on the marls of the new red sandstone, and on the slopes of the slate rocks of Wales.

§ 6.—Within these larger local preferences, it is noticed that certain villages or hamlets appear to be favourite haunts of diphtheria, which reappears from time to time, now in one and now in another. Certain houses in like manner are found to be revisited again and again by this disease. Whether these reappearances are due to a revival of the contagium derived from previous outbreaks in the same place, or to some favouring conditions which these places offer for the development of infection derived from some other quarter, must at present remain a doubtful point.

§ 7.—Much interest attaches to an observation which has often been made by writers on diphtheria who have had opportunities of studying its habits in the country. They have been struck by its frequent occurrence in high exposed situations, in a little village on a hill, or in an isolated cottage or farmstead on the crest of a bare ridge, where the wonder is that any disease at all should arise except such as might be caused by exposure to the violence of wind and weather.

I have already quoted a passage in which Dr. Charles Kelly incidentally alludes to the "bleak and exposed situations" in which rural diphtheria so commonly occurs.†

Among the valuable records of the great outbreak in 1859, published in the Second Report of the Medical Officer of the Privy Council, there is evidence to this effect. Dr. Sanderson there says: "In the district around Launceston the localities which have been most severely visited by diphtheria have certain topographical characters in common, which, although not striking at first sight,

* See also G. H. Fosbroke, S.Sc. Cert. Camb., in *Sanitary Record*, Feb. 15, 1881, p. 296.

† Report for 1879, p. 90.

are well worthy of attention. Their universality is suggestive to the minds of a real relation between diphtheria and the conditions they express. *All the villages invaded by diphtheria, with scarcely an exception, are in elevated situations. Many of them are on the ridges or slopes of the hill.*"

In Kent the same observer noticed that diphtheria prevailed with the greatest fatality in an elevated and exposed tract of country of considerable extent lying on either side of a line drawn from Dover Castle to Canterbury. Dr. Sanderson names certain villages situated in valleys in which diphtheria occurred, but of one of them he noticed that the cases occurred at a farmstead "on the crown of a neighbouring hill." At the same time "in the town of Sandwich, which is completely surrounded by marshes, only a few isolated cases of diphtheria occurred, and in the villages on the marsh . . . not a single case had been met with."

In the same report from which I have quoted these passages there is published a communication from Dr. (now Sir William) Gull on "Diphtheria in Metropolitan Districts." Dr. Gull says: "The conclusion I have formed from the cases I have seen is, that an exposed locality is as liable to diphtheria, and perhaps more liable, than more sheltered spots."

Dr. Slade King and Mr. Wynter Blyth in Devonshire have met with facts telling the same tale even more distinctly. Dr. Slade King found that nine-tenths of the cases (in an outbreak at Ilfracombe in 1873–74) were confined to the higher parts of the town, the lower escaping almost entirely. Mr. Wynter Blyth, studying the same epidemic, which extended into his district from Ilfracombe, says: "The fact of its preference for high, open, and airy situations was extremely marked; the places of selection being isolated houses on lofty hills, supplied with polluted water and surrounded with nuisances." *

§ 8.—In the outbreaks which have come under my own observation, the disease appeared to originate in a high situation in thirteen instances, in a low situation in seven, and midway in four.

The thirteen high sites were the following:—

1.—Killingworth village, on a well-marked ridge of sandstone in a generally level part of Northumberland a few miles north-east of Newcastle.
2.—Staple Cross, a hamlet on a conspicuous hill-top in the east of Sussex, overlooking the valley of the Rother.
3.—Felsted, a village on a hill-top near Dunmow, in Essex.
4.—Henley, a village on high ground a few miles north of Ipswich, in Suffolk.
5.—Chipping, near Clitheroe in Lancashire. At a farmhouse known as Abbot's Barn, on an eminence near the village.
6.—Llanllyfin, near Carnarvon. At a cottage high up on the hill-side.
7.—Manorbier in Pembrokeshire. At a lonely cottage on the crest of a ridge of old red sandstone known as the Ridgeway.
8.—Manorbier in Pembrokeshire. At the village of Jamestown, on a range of high clayey ground.
9.—Mathry, near St. David's in Pembrokeshire. At an isolated cottage on a bleak ridge.
10.—Woolfardisworthy, near Bideford in Devonshire. At an isolated farmhouse on a commanding hill.
11.—Forest Row, near East Grinstead in Sussex. At an isolated farmhouse high up on the north side of Ashdown Forest.
12.—West Hoathly, near East Grinstead, a village on the top of a commanding hill.

* See "Blyth's Dictionary of Hygiène and Public Health," art. "Diphtheria." London, 1876.

13.—Church Gresley, near Burton-on-Trent, a village on the top of a commanding hill on the border of Leicestershire.

The eight low sites were as follows:—

1.—Blanchland in Northumberland, a village at the bottom of a deep thickly wooded valley.
2.—Killingworth Colliery, in Northumberland, in flat open country.
3.—Castle Gresley near Burton-on-Trent, at the foot of the Church Gresley hill above mentioned.
4.—Wrexham in Denbighshire. At a house in the lower part of the town.
5.—Glanaber, near Beddgelert in Carnarvonshire, a hamlet at the foot of a thickly wooded hill.
6.—Llandewi'rcwn, near Builth in Brecknockshire. At an isolated cottage in an upland valley, at the foot of high hills.
7.—Chipping in Lancashire. At a low-lying spot called the Bottoms, on the bank of a stream.
8.—Penkridge, near Stafford. At a cottage at foot of hill.

The three sites which could neither be called high nor low were:—

1.—Onibury, near Ludlow. At a cottage on the slope of a hill.
2.—Lurgashall in Sussex. On the higher and more exposed part of a wide clayey common.
3.—Raunds, near Thrapstone in Northamptonshire. On the slope of a valley side.

It will be noticed that this list contains instances drawn from many different parts of England and Wales, and so far may be supposed to be free from undue influence of any one set of topographical peculiarities.

On the whole, I think it must be admitted that diphtheria outbreaks in their commencement show a decided, though by no means exclusive, preference for elevated exposed situations.

§ 9.—Intimately related to the question of situation is the question of conveyance of infection by the wind; and to this point I hope to return shortly. But first I wish to ask your attention to another preference which is manifested by diphtheria. I mean the preference that it shows for a certain season of the year as the time of commencement of its epidemic activity.

From various sources, but chiefly from the data gathered by Drs. Greenhow and Sanderson, and the medical inspectors of the Local Government Board, I have collected records of 147 outbreaks or isolated occurrences of diphtheria, in which it was possible to fix pretty closely the date of the first attack. The result is curious. The number of outbreaks occurring in

January	is 4		July	is 16	
February	„ 6	= 16	August	„ 11	= 38
March	„ 6		September	„ 11	
April	„ 8		October	„ 40	
May	„ 6	= 25	November	„ 16	= 68
June	„ 11		December	„ 12	
Total in first six months,		41	Total in last six months,		106

These numbers may best be exhibited by the aid of a diagram (fig. 1), in which the remarkable excess of outbreak frequency in October stands conspicuous.

If this curve of outbreak frequency is compared with the curve of mortality from diphtheria, it will be seen that there is a remarkable difference between

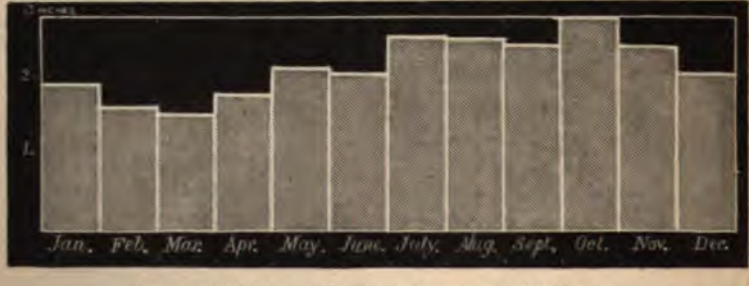
Fig. 1.

Monthly Frequency of Diphtheria Outbreaks.

Fig. 2.

Quarterly Mortality from Diphtheria in England and Wales, 1870–79.

Fig. 3.



Monthly Rainfall at Greenwich, average of 65 years.

them. Fig. 2 shows the average diphtheria mortality in each quarter during the ten years 1870–79 for England and Wales, as obtained from the quarterly returns of the Registrar-General. This gives only the main variations of the mortality as between different quarters, and necessarily conceals all minor features. The two curves agree in having their maximum in the fourth quarter, but while in point of frequency the outbreaks sink to a minimum in the first quarter, and thence rise through the second and third to the fourth, the mortality on the other hand is scarcely less in the first quarter than in the fourth, declines further in the second, and does not reach its minimum till the third; so that, while the number of outbreaks that take origin in the first half of the year is less than two-fifths of that in the second, the resulting mortality is just about the same in the two halves of the year.

This difference can be simply and naturally explained by the consideration that an epidemic of diphtheria breaking out in the autumn will probably increase in the next month or two, continue raging through the winter, and perhaps not subside till the height of the following summer.

§ 10.—The minor features of the curve of diphtheria mortality for London alone are exhibited in one of a valuable series of diagrams which illustrate a recent paper by Mr. Alexander Buchan on "The Weather and Health of London." * This curve, which is constructed from the weekly returns of deaths in London extending over a great number of years, is in some respects at variance with the curve (fig. 2) for the whole of England and Wales. The London curve shows a more rapid decline from the maximum in the fourth quarter, and apparently reaches its minimum not in the third quarter, but in the second; and the mortality in the second half of the year is evidently much greater than that in the first. I venture to suggest that this may be due to the purely urban character of the data on which this curve is based. We have already seen reason to believe that diphtheria in towns is less prone to take an epidemic form than in the country. The natural consequence would be (*cæteris paribus*) an earlier diminution in the towns of the mortality due to the autumnal outbreaks.

§ 11.—The sudden increase in the number of diphtheria outbreaks in October is, at first sight, highly suggestive of the presence of some fungoid organism depending on the season of the year for its development and reproductive activity.

It is true that after Dr. Burdon Sanderson's experiments with the fluid of a simple peritonitis,† we cannot regard as utterly inconceivable the idea that the human body out of its own elements might develop an infectious disease; and the advocates of the theory of spontaneous development of diphtheria in the human body might point with some degree of assurance to the fact that October is the wettest month of the twelve, and that there is a certain similarity between the curve of monthly rainfall and the curve of monthly frequency of diphtheria outbreaks (see figs. 1 and 3), both having their minimum in the first quarter of the year, both rising in the summer, both falling again before October, and both in October rising to a conspicuous maximum, from which both fall to the end of the year. The only marked disagreement between the two curves is in January; and we must remember that in that month a great deal of the "rainfall" is in the solid form, from which we should not be justified in expecting the same results as from the same element in the liquid form. But the data on which the curve of outbreak frequency has been formed are perhaps too scanty to admit of criticism of the more minute features of the curve.

§ 12.—In view of the general agreement between the two curves in question, it is difficult to avoid the impression that the one is in some way dependent upon the

* See *Nature*, June 16 and 23, 1881, pp. 143 and 173.

† "Infective Processes of Disease," by J. Burdon Sanderson, M.D., F.R.S.

other. It might be suggested that the general preference of diphtheria for a wet clayey district seems to harmonise with the idea that the disease in its origin owns some dependence upon rainfall. The spontaneists would probably urge that exposure to wet caused simple colds and sore throats; that these spontaneously developed, by rapid steps of the kind artificially reproduced by Dr. Sanderson, into a virulent infectious disease. The contagionist, if he regarded diphtheria as caused by a morbific organism capable of multiplication only in the human body, would say that probably the damp favours the vitality of the germs thrown off by a diphtheria patient, and so favours the transmission of the infection to others, and that it also probably acts upon the systems and tissues of those who are exposed to it in a way that makes them more receptive of the diphtheritic infection. He would also with great force urge analogy with those diseases of the zymotic class in which the presence of specific self-multiplying organisms (*bacilli*) in the blood and tissues has been proved, and would express strong confidence that before long the observations of Oertel and Hüter in this direction, in respect of diphtheria, would be confirmed by experimental researches of cultivation and successive inoculation, which alone could set the question at rest.

The spontaneist would perhaps question the specific nature of these bacilli, and speak of them as adventitious and not essential to the morbid process. And he would proceed to ask how, except on the supposition of spontaneous origin, could the fact be accounted for that the great majority of diphtheria outbreaks are perfectly inscrutable and untraceable in their commencement, while in several marked instances they have been preceded by a prevalence of apparently simple sore throat in the surrounding district, suggestive to the skilled observer of a "progressive development of infectiveness."*

§ 13.—At this point a third theory, which may be called the "exogenist," is advanced, which accounts for the apparently spontaneous character of diphtheria outbreaks by supposing that they are caused by an organism which has a life external to the human body as well as within it,—"now fructifying" (to use the expressive words of Mr. Simon †) "in soil or water of appropriate quality, and now the self-multiplying contagium of a bodily disease." The advocate of this theory would claim that it fully accounts for the untraceable origin of diphtheria epidemics: for their excessive frequency in October, and the general correspondence between their monthly frequency and the average monthly rainfall (and at the same time for particular exceptions to this general rule); for the preference this disease exhibits for certain soils and certain localities, and for its infrequency in towns as compared with rural districts; while at the same time the theory maintains solidarity with the advance of microzymic discovery. In this relation the researches of Klebs and Tommasi Crudeli‡ proving (as predicted by Mr. Simon in the paper to which I have already referred) that ague is caused by the inception of a specific microzyme (*Bacillus malariæ*) which has an external habitat in the soil and the air close to the surface of the ground in certain marshy tracts,—researches which have been recently supplemented by successful experiments in the transfer of ague by inoculation,—have especial importance.

§ 14.—But which of these three theories can account for the preference that diphtheria shows, in the commencement of an outbreak, for a dwelling on the top of a hill?

Can the spontaneist plead that children who live in exposed situations are

* "Remarks on the Origin of Infection," by Dr. Thorne Thorne. *Transactions of the Epidemiological Society*, vol. iv. part ii. p. 244.

† "Essay on Contagion," *Brit. Med. Jour.*, Dec. 20, 1879, p. 974.

‡ *Archiv. für Experimentelle Pathologie*, July and Oct. 15, 1879 (referred to by Mr. Simon in his "Essay on Contagion").

more likely to suffer from the simple throat inflammation whence he supposes the infectious disease to be derived?

This plea is met at once by the objection that in the majority of such cases there has been no antecedent series of sore throats, but the very first attack is of undoubtedly specific nature.

Can the exogenist suppose that hill-tops and bleak ridges are the favourite native haunts of the diphtheria organism before it enters the human body? So unlikely an hypothesis cannot be listened to without a particle of evidence to support it.

What says the contagionist? Can he tell us of any mode by which the diphtheria contagium can with any probability be conveyed undestroyed across the country, and strike by preference the dwellings that are perched on heights that seem securest from any infectious disease?

He is not at fault for an answer, but boldly declares his belief that the contagium is carried by the wind. Mr. Wynter Blyth, now Health Officer for Marylebone, in his valuable paper on "The Prevention and Propagation of Diphtheria," in considering the question as to what is the most probable cause of the outbreak of the so-called spontaneous or sporadic cases, says, "I believe we have fairly strong evidence to accuse the atmosphere, and that such cases may be ascribed with some certainty to the wafting of the specific poison by currents of air." * Mr. Blyth mentions that this theory was held by Dr. C. Budd of North Tawton in Devonshire, from whom he quotes an instructive case, and he cites two cases from his own experience in support of this view.

Another observer, Dr. Edwyn Slade King, Health Officer for Ilfracombe, in his "Notes on the Spread of Diphtheria" (1880), says, "The only reasonable explanation (of the untraceable spread of diphtheria) seems to be that the atmosphere becomes contaminated by contagia from the bodies of the sick, and that currents of air carry the poison with them in their course."

The idea of infection being carried in the air is, I need hardly say, a very old and very popular one; and for very short distances—for example, within the four walls of a room—the idea is accepted and acted upon by every medical practitioner who warns a family to isolate their fever-stricken child. But for distances greater than this, it is perhaps a little too much the habit to neglect the question of aërial infection, probably because of the difficulty of putting it to the proof. Certainly the idea is not necessarily absurd, and I am not acquainted with any set of careful experiments or observations that conclusively disprove it. Yet it is a point of the very highest importance to prove or to disprove, and I trust I may be pardoned for dwelling upon it awhile.

§ 15.—What do we imagine to be the act whereby a healthy person, present in a room which contains a diphtheria patient, himself contracts the disease? Besides direct bodily contact and common use of drinking-vessels and the like, do we not suppose the infection to be caught by inhaling air which contains infectious particles derived from the body of the patient? Doubtless the risk of infection is greatest in the immediate neighbourhood of the sick, where the air must be most thickly charged with infective particles, but there seems no sufficient reason for supposing that one single particle inhaled by a susceptible person may not be enough to give rise to the disease.

Now no one will deny the possibility of the mechanical transportation by the wind, and the chance inhalation by some person at a distance, of a single particle from an infected house. The air is full of particles,—pollen grains, mould spores, soot, globules of smoke tar, and a multitude of nondescript fragments of matter, organic and inorganic (as any one may see by fitting a funnel on a wind-vane so as to direct the wind-stream against a spot of glycerine

* "Prevention and Propagation of Diphtheria," by A. Wynter Blyth, M.R.C.S., &c., *Sanitary Record*, May 15, 1880, p. 407.

on a glass slide, and then examining under the microscope), and at every breath we inhale a cloud of solid matters. Why should not one or two of these particles have come from a distant sick-room, where the windows have been thrown wide open for ventilation,—that is, for the committal of infection germs to the stream of the wind? A cloud of infection germs thus launched upon the air will not necessarily be dispersed, according to any fixed mathematical law, like the radiation of light from a centre, but a good deal of it may go in a body down the wind, somewhat as the smoke of an ocean-steamer may be seen hanging in a streak above the horizon long after the vessel herself has passed out of sight.

But it may be objected that the vitality of the contagium would be destroyed by such exposure in the open air. I very much doubt it. A poison which can retain its virulence for weeks and months exposed to the air in an empty room, would not so readily lose it in floating quietly down the wind for an hour, in which time it might easily traverse a distance of ten or twelve miles,—the average velocity of the wind at Greenwich being from 250 to 300 miles in twenty-four hours. The health officer of a small district, perhaps only a few parishes, knowing probably nothing of an outbreak in a neighbouring district ten miles to windward, if called suddenly to a severe case of diphtheria in a clean, healthy hamlet, might well feel tempted to regard it as a spontaneous development, and take no note of the direction of the wind.

§ 16.—From the foregoing considerations I think we must admit that there is no physical impossibility in aërial infection over a distance of several miles; and supposing such infection can really take place, I think we can see reason why it should take place preferentially in high, bleak situations. The air rushing over a ridge may be likened to water poured out over the edge of a deep basin; and just as in turbid water particles suspended at different levels will all have to pass within very narrow limits (measured vertically) over the basin-edge, so any matters suspended at different levels in the valley air will all be compressed within much narrower limits of level in crowding over a high ridge of land; and, therefore, if diphtheria germs are diffused through the atmosphere at different levels, or, still more, if they rather affect the strata above the lowest,* persons who live on a ridge are more likely to breathe air containing such germs than persons who dwell on lower ground.

§ 17.—The presence of diphtheria germs in the air may perhaps one day be demonstrated. They may be arrested to leeward of an infected house by some form of aspirator, or by suitable funnel-and-vane apparatus, and be submitted to microscopic examination, and to those processes of experimental cultivation and inoculation which have proved so instructive in some other zymotic diseases. But in the meantime, our task is to gather such evidence as we can find that has any bearing on the question.

Mr. Wynter Blyth, in his paper on the "Prevention and Propagation of Diphtheria," has suggested, as a means of testing the truth of this theory, that disease charts should be prepared, in which the direction of the winds and the localities of diphtheria, with the dates affixed to each, should be plotted out. Such a systematic record of observations over a wide area, for which the health officer of a large district has exceptional opportunities, would probably soon solve the question.

§ 18.—As a contribution towards this end, I have collected from reports of the medical inspectors of the Local Government Board a number of instances of diphtheria outbreaks—as many as I could find in which the date of the first attack was pretty accurately known—and have noted the wind facts for the fortnight preceding each outbreak, together with any information I could obtain of recent previous diphtheria in the neighbourhood. The wind facts are taken

* As suggested by Dr. Slade King in "Notes on the Spread of Diphtheria."

from the Registrar-General's weekly returns of births and deaths in London. They are true for Greenwich, and when the wind is steady they may be taken as true for the greater part of the country, but when the wind is shifting they cannot be trusted as strictly applicable to distant places. I should say, however, that in comparing the Greenwich records with the daily weather-charts of the Meteorological Office as published in the *Times*, I have found no such disagreement as would materially affect the question at issue.

The results arrived at appear at first sight highly significant. Of thirty outbreaks thus examined, twenty seem to satisfy the requirements of the wind theory—that is to say, within eight or nine days of the first attack the wind was blowing from a quarter in which diphtheria had recently prevailed, at an average distance of seven miles; in nine the agreement is doubtful as regards the probable source of infection, though the wind conditions appear favourable; in only one are the wind conditions apparently inadequate.

A list of these thirty outbreaks is given below in a tabular form:—

Place of Outbreak.	Date of First Attack.	Probable Date of Infection.	Direction of Wind.	Previously Infected Place.	Distance in Miles.
	1875				
1. Coggeshall, Essex	Nov. 11	Nov. 6	W. and W.S.W.	Braintree or Black Notley	6
2. South Ash, Kent	Dec. 27	Dec. 20–24	S.W., S.S.W. and W.S.W.	Sevenoaks sub-district	9
3. Stokesay, Shropshire	Dec. 24 1876	Dec. 19, 20	S. and S.W.	Onibury	3
4. Bromfield, Shropshire	Feb. 3	Jan. 29–31	S.E., S., S.S.E.	Ludlow?	3
5. Penkridge, Staffordshire	Feb. 14	Feb. 6–8	N., N.N.E.	Stafford	6
6. Brailes, Warwickshire	May 6	April 30–May 3	N.E., N.N.E.	Tysoe	5
7. Padstow, Cornwall	June 18	June 14, 15	S.S.W.	New Quay	12
8. Radwinter, Essex	June 22 1877	June 15, 16	S.E.	Coggeshall?	18
9. Anlaby, Yorkshire	April 24	April 16–18	E., E.N.E., N.E.	Hull	3
10. Swanland, Yorkshire	May 11	May 4–6	E., E.N.E., N.E.	Hull	6
11. Denbigh	Aug. 10	Aug. 2–4	W.N.W.	Llanfairtal-haiaru	8
12. Wrexham, Denbighshire	Nov. 23 1878	Nov. 18–20	N.W., W.N.W.	Brymbo	3½
13. Cranfield, Bedfordshire	June 26	June 14, 15	N.E., N.N.E.	Bedford?	7
14. Bleadon, Somersetshire	Oct. 10	Oct. 5–7	S.S.E.	Wedmore	8
15. Staple Cross, Sussex	Nov. 4 1879	Oct. 28–30	W.N.W.	Rotherfield	15
16. Swanscombe, Kent	July 26	July 21, 22	N.W.	Barking	12
17. Trotterscliffe, Kent	Sept. 15	Sept. 8–12	S.W.	Shoreham sub-district?	6
18. Felstead, Essex	Sept. 12	Sept. 7–9	S.W.	London?	30
19. Henley, Suffolk	Nov. 21	Nov. 12, 13	N.W.	Needham	4
20. Woolfardisworthy, Devonshire	Aug. 30	Aug. 25–27	S.W.	Kilkhampton subdistrict	7
21. Llandewi'rcwm, Brecon-shire	Oct. 23	Oct. 16–19	N.W., S.W.	?	...
22. Ridgeway, Pembroke-shire	Sept. 18	Sept. 10	W.N.W.	St. David's sub-district?	25
23. Jamestown, Pembroke-shire	Nov. 17	Nov. 12, 13	W.N.W.	St. David's sub-district?	25
24. Llanllyfni, Carnarvon-shire	Sept. 26	Sept. 22, 23	S.S.W.	?	...
25. Glanaber, Carnarvon-shire	Oct. 31	Oct. 26–29	E.N.E.	Denbigh?	30
26. Tremadoc, Carnarvon-shire	Dec. 1	Nov. 24–28	N.E.	Garddllygaid-ydydd	5
27. Raunds, Northampton-shire	Nov. 15 1880	Nov. 11	S.W.	Wellingborough	7
28. Forest Row (1) Sussex	April 1	Mar. 25–27	E.N.E.	Tunbridge Wells	10
29. Forest Row (2) Sussex	May 26	May 17, 18	N.E.	Forest Row (1)	1
30. West Hoathly, Sussex	Aug. 15	Aug. 11–13	E., N.E., E.N.E.	Forest Row	4

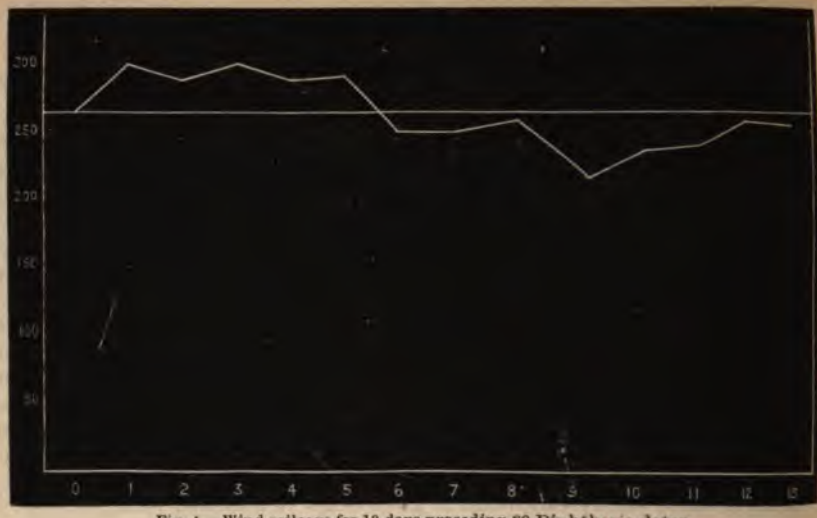


Fig. 4.—Wind-mileage for 13 days preceding 30 Diphtheria dates

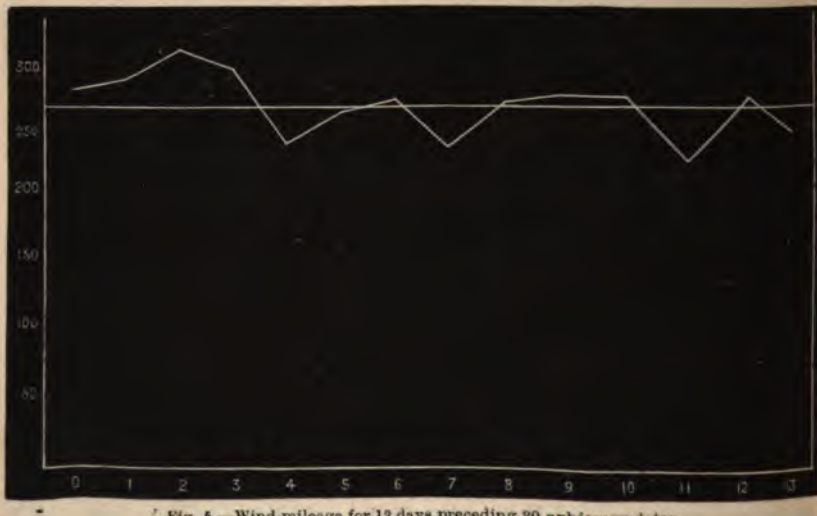


Fig. 5.—Wind-mileage for 13 days preceding 30 arbitrary dates.

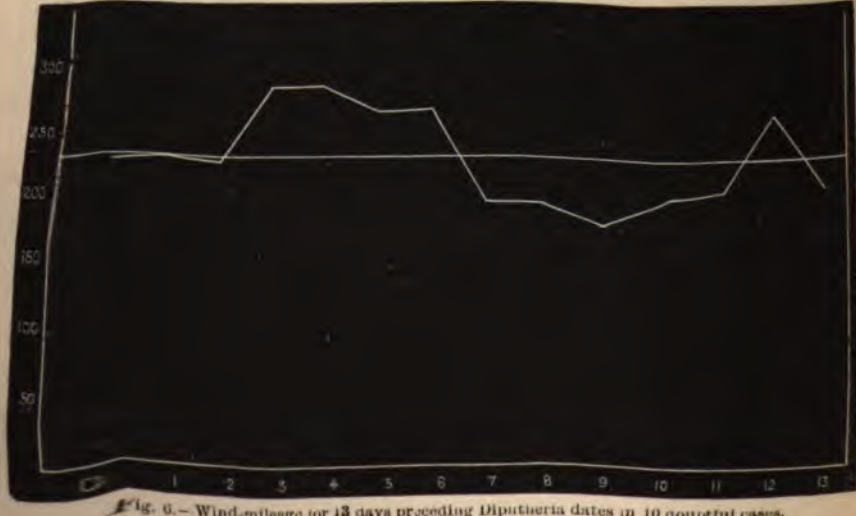


Fig. 6.—Wind-mileage for 13 days preceding Diphtheria dates in 10 doubtful cases.

§ 19.—In estimating the bearing of these facts there are several points to be noticed and several fallacies to be guarded against. At first one is struck by observing how often it happens (in twenty-nine cases out of thirty) that the date of an outbreak is preceded at an interval of a few days (averaging three to six) by a breeze of more than average daily velocity. The impression is confirmed if we take the mean for the whole thirty cases of the number of wind-miles in each day of the fortnight preceding an outbreak; for it then appears that, on an average, during the five days immediately preceding the date of outbreak, the wind has exceeded its average run, while during the eight days before then it was below the average. The curve is shown in fig. 4. Similar results are obtained by taking the mean daily pressure of the wind instead of the number of miles travelled.

The impression thus obtained is somewhat moderated, though not destroyed, if, in order to test its value, we apply a similar analysis to an equal number of dates taken at random. Thus, treating the last day of each month in the years 1878 and 1880, and the 15th of September, October, and November, just as we have treated the above thirty diphtheria dates, we find, in the first place, that in twenty-three out of the thirty arbitrary cases (as against twenty-nine out of the thirty diphtheria dates), there was a breeze of more than average mean daily velocity, preceding the arbitrary date at an interval averaging three to seven days; and taking the mean for the whole thirty, we obtain a curve of wind-mileage (fig. 5) presenting quite as much inequality as that belonging to the diphtheria dates in fig. 4. The inequalities, however, in this second curve (fig. 5) are of a capricious character and unsustained, whereas in the former I think we discern a structure with features large enough to bear a meaning. It seems not unreasonable to suppose that a series of comparatively quiet days followed by a series of comparatively windy ones might first favour the accumulation of the diphtheria poison at infected places, and then its dispersion in considerable quantity to a distance.

We must not expect (even assuming the theory to be true) to find every case exhibiting close agreement with the wind-curve here presented; for, not to mention the probably wide limits of diphtheria incubation, there is the chance of error in the date of first attack; the probability that some of the outbreaks were really due to unnoticed personal or mediate communication; and the possibility that, besides all modes of contagion, the disease may at certain seasons of the year arise through the agency of an external organism, though even then the wind may be the chief disseminating agent. In reference to this last point, the possibility of an exogenous origin in some cases, it may be noticed that of the ten doubtful cases in the list I have given, one occurred in February, two in June, four in September, two in October, and one in November, showing a preponderance of doubtful cases in the autumn.

The wind-curve for these ten cases (fig. 6) does not, however, materially differ from that for the whole thirty (fig. 4); and this agreement of the partial curve with the general curve gives additional value to the latter.

We may further test the representative value of the general curve (fig. 4) by dividing the thirty cases which give that general curve into three groups of ten each, and plotting out partial curves for these three groups. The curve for the first ten cases is shown in fig. 7, that for the second ten in fig. 8, and that for the third in fig. 9.

I think we can recognise in these three curves so much general agreement with one another in the main feature of an ascent about a week before the date of diphtheria outbreak as makes them fairly comparable with, and confirmatory of, the general curve for the whole thirty.

Of the ten doubtful cases above mentioned, two are included in the first

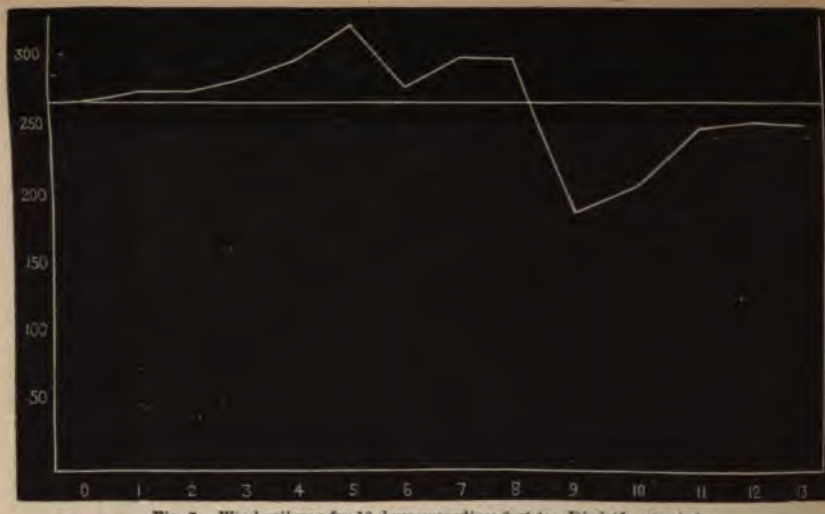


Fig. 7.—Wind-mileage for 13 days preceding *first ten* Diphtheria dates.

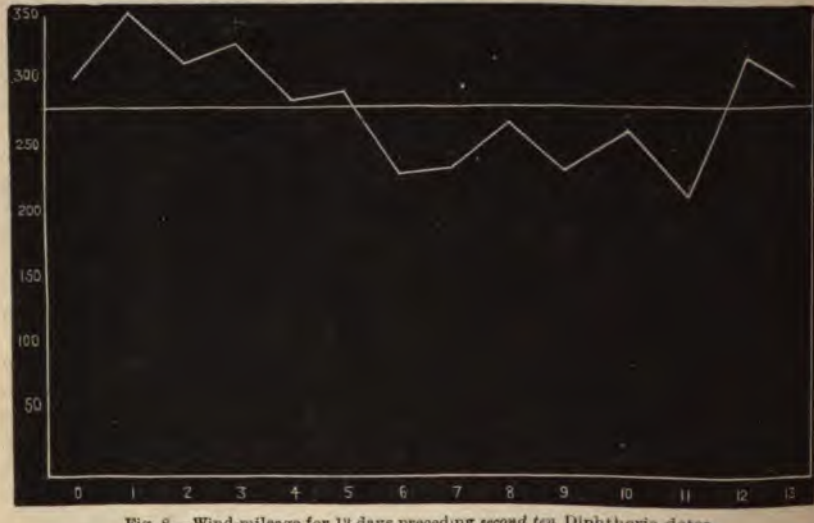


Fig. 8.—Wind-mileage for 13 days preceding *second ten* Diphtheria dates.

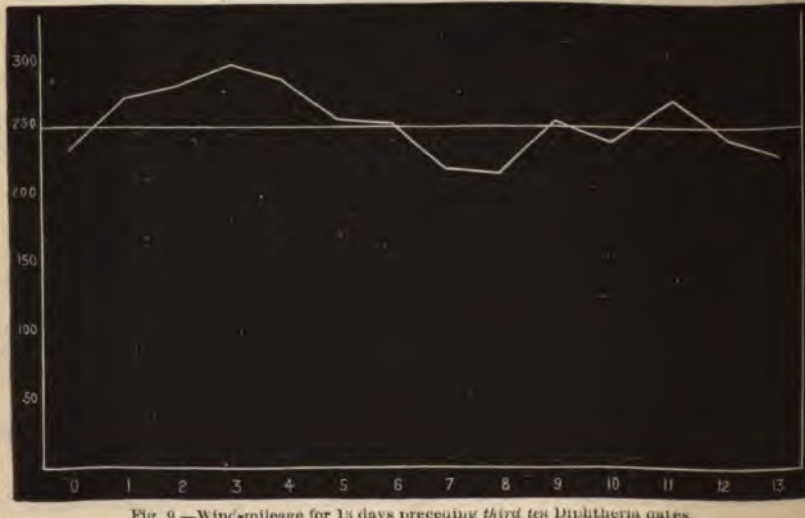


Fig. 9.—Wind-mileage for 13 days preceding *third ten* Diphtheria dates.

group of ten, three in the second, and five in the third. We have, in fact, four partial groups, each confirming the evidence given by the whole.

§ 20.—In applying the wind-theory to any given set of diphtheria dates, there is room for fallacy owing to the frequent variations of the wind within the space of a fortnight. We ought, therefore, by way of test, to apply arbitrary winds (supposed in the direction of previous diphtheria) to the dates of our thirty outbreaks, and see how far the arbitrary winds find place among the winds recorded in the fortnights preceding those dates. This I have tried to do by taking in succession the winds that held good for the several diphtheria dates and applying them to all the other dates in turn. The result is, that I find the arbitrary winds fairly represented among the actual wind-facts in 439 cases out of 900—nearly one-half. In order to correct the exaggeration caused by the greater frequency of south-west winds, I made trial in another way, by applying sixteen arbitrary winds (from the sixteen chief points of the compass) to fifteen arbitrary dates in 1880, with the result that these winds were found among the actual facts in only 78 cases out of 240. The same sixteen arbitrary winds applied to the thirty diphtheria dates found place in 213 cases out of 480.

Each of these trials leaves a decided advantage on the side of the theory, which counts twenty successes to ten doubtful cases.

In each of these trials, however, I have supposed, for each imaginary case, only one wind—that is, only one direction in which diphtheria infection could be conveyed by the air. But this is not quite fair, for among the thirty diphtheria localities there were some that were within reach of two or more possible sources of infection, and every additional focus in a new direction would increase the chance of finding among the wind-facts something that would serve the wind-theory.

We must therefore find some test that shall avoid this fallacy. This may be most simply done by applying to the thirty local conditions of outbreak a new set of thirty arbitrary dates with such wind-conditions as those new dates may entail. For dates let us take in succession (say) the twenty-first of each month in 1879 and 1880, with extra days, say the seventh, in the months September, October, and November in each of those years. The result is that all necessary conditions appear to be fulfilled in only ten instances; in five the probable source of infection is doubtful; in thirteen no probable source can be named; and in the remaining two the wind fails to reach the required standard of velocity. Thus the thirty real diphtheria dates satisfy the theory twice as often as the arbitrary dates; but if the theory were groundless, the results ought to be very much the same with either set of dates.

Thus far, then, we find the theory distinctly confirmed by the tests to which we have submitted it.

§ 21.—There are also points of topography to be noticed, which cannot be included in a test, but which contribute to impress the mind of the observer. In several cases in the list of thirty outbreaks given above, the natural features of the country are such as would favour the passage of the wind from the supposed source of infection to the point of subsequent outbreak. For example, in Case 1, the valley of the Blackwater leads straight from Braintree to Coggeshall, and at the latter place it turns southward, so that a west wind at this point would have to force itself up the sloping ground on which the village lies. Similar conditions are present in Cases 5 (Penkridge in relation to Stafford), 15 (Staple Cross in relation to Rotherfield), 19 (Henley in relation to Needham), 27 (Raunds in relation to Wellingborough), 28 (Forest Row in relation to Tunbridge Wells), and 30 (West Hoathly in relation to Forest Row).

Many of the places in the list are in notably high situations, and therefore

liable in an especial degree to be touched by anything that is carried by the wind. Such are Nos. 2 (South Ash), 8 (Radwinter), 15 (Staple Cross), 16 (Swanscombe), 18 (Felstead), 19 (Henley), 20 (Woolfardisworthy), 22 (Ridgeway), 23 (Jameston), 28 (Forest Row, 1), 29 (Forest Row, 2), and 30 (West Hoathly).

Others, somewhat less exposed, are on hill-slopes; only seven out of the thirty are in low situations.

I am far from supposing that the instances and arguments I have here brought forward are sufficient to establish the theory of aërial diphtheria spread; but I think they are sufficient, in conjunction with earlier observations, to give the theory a right to be disproved, and, pending disproof, to be provisionally entertained.

§ 22.—Upon the practical importance of the question in respect of other infectious diseases as well as diphtheria I need not dwell; but I feel bound to return for a moment to the speculative considerations from which I have diverged, to ask what is the bearing of the "aërial infection" theory on the question of the nature of the diphtheritic contagium? If true, it seems to cut away much of the ground from under the "exogenist" theory, by explaining those very cases of apparently spontaneous origin of diphtheria which that theory was expressly framed to account for. It does not, however, explain "progressive development" of simple sore throat into diphtheria; nor does it explain the preference of diphtheria for certain regions, and for a certain season of the year. In short, it does not decide the question. Its bearing is practical rather than ætiological, and its aid might be invoked almost equally by all three of the supposed theorists whose divergent views we have had under consideration.

For my own part, if I may venture to express an opinion, where so many inquirers better qualified than myself are yet in doubt, I still incline, though perhaps with lessened confidence, to regard as most probable the hypothesis that diphtheria is caused by an organism naturally haunting air or water in certain regions, but also capable of living and multiplying in the human body.

I feel that I must apologise at once for the length of this paper, for its inconclusive character, and for the omission of many points of great interest and importance in the dealings of this most subtle and capricious disease.

On the Nature and Propagation of Diphtheria.

Dr. A. Jacobi, New York.

Diphtheria is pre-eminently a disease of childhood. Of 300 cases of laryngeal diphtheria in which I performed tracheotomy, but one was over 50, one over 30, two over 13 years old. Of 501 deaths by diphtheria in Vienna in 1868, but one was 62 years old. Of 10,000 cases of diphtheria in England, according to Thursfield (*Lancet*, August, 1878), 90 per 1000 were under 1 year, 450 per 1000 from 1 to 5 years, 260 from 6 to 10, 90 from 11 to 15, 50 from 16 to 25, 35 from 26 to 45, 25 were 45 or over.

Of 9354 reported deaths of diphtheria in the United States—the States of Massachusetts and New Jersey, and the territories of Wyoming, Arizona, and New Mexico, and the district of Columbia not being corrected, the reports being those of physicians, and not of State Boards—only 445 occurred under one year, 778 in the second, 1034 in the third, 1011 in the fourth, 991 in the fifth; thus, 4219 under five years (45 per cent.), 3057 between the sixth and tenth, that is

as many as in the three years beginning with the third and ending with the fifth year. Between 10 and 15 there were 1275, about as many as in the first and second together, and but few more than in either the third, or the fourth, or the fifth year. There were between

15	and	20	years	329	less	than	in the first year.
21	,,	25	,,	112	,,	,,	
26	,,	30	,,	57	,,	,,	
31	,,	35	,,	28	,,	,,	
36	,,	40	,,	28	,,	,,	
41	,,	45	,,	18	,,	,,	
46	,,	50	,,	13	,,	,,	
51	,,	55	,,	10	,,	,,	
56	,,	60	,,	10	,,	,,	
61	,,	65	,,	4	,,	,,	
66	,,	70	,,	2	,,	,,	
71	,,	75	,,	2	,,	,,	
75	,,	80	,,	2	,,	,,	
81	,,	85	,,	2	,,	,,	
86	,,	90	,,	1	,,	,,	

The age from 91 and over furnishes no mortality from diphtheria.

The cause why children are so much more exposed than adults is mostly anatomical. The mucous membrane of the oral cavity in the child is more succulent, and often the seat of catarrh and inflammation, which dispose to, or are transformed into, a diphtheritic process. The nasal cavities are small, and often affected with catarrh, and but too often cleanliness is insufficiently attended to. With the exception of nutritional changes and atrophy, every change in the mucous membrane predisposes to diphtheria.

The tonsils are large and the pharynx narrow, so that poisons, either chemical or parasitic, will find an easy resting-place. Besides, the size of the child brings it into closer contact with the exhalations of the soil, or sewers, or gutters. This fact is of some account when we remember that the diffusion of gases is very great, and the distance of 50 or 80 centimetres more or less is of importance; and though a direct connection between sewer-gas and diphtheria has not been proven, the general health cannot but suffer from such exposure. Moreover, after the local affection has taken place, the size and number of the lymphatics, which can be more easily injected in the child than in the adult according to Sappey, and have lateral and intercommunicating stomata according to Schenck, aggravate the case by accelerating the appearance of general symptoms.

The number of cases and deaths in the first year is relatively small, as the above figures show. Of those who are taken under a year of age, most are either less than three or more than seven months old. The reason is this: from the end of the third to the seventh month there is a very copious secretion from the infant's mouth, with the result of washing away noxious importations.

Predisposition.—Diphtheria once endured, creates a predisposition. It may be that the mucous membrane undergoes, in the first attack, such changes of a catarrhal or merely anatomical (epithelial) character as to fix that predisposition. It may be that in the enlarged glands diphtheria poison is stowed away for occasional resuscitation, as we know it to be, for instance, in syphilis.

Even that form of diphtheria which is as little apt to give rise to constitutional symptoms as it is very liable to terminate fatally has been known to return. Guersant, Gill, Quincke have performed tracheotomy twice on the same patients.

Predisposition is not only individual, but clings to families. Certainly the

cause must be anatomical; for there is a family pharynx, a family tonsil, a family larynx, a family nasal contour, as there is a family foot or hand. I have seen cases of laryngeal diphtheria, membranous croup, destroying a number of children of the same family, not in the same year or house, but in the course of many years, and while living in different dwellings. In other families I have seen perfect, or almost perfect, immunity for more than twenty years.

Of more than 300 cases of laryngeal diphtheria on which I performed tracheotomy, and of the same number observed by me besides, the majority were males. Of the recoveries the majority occurred in females. Other forms of diphtheria exhibited in my experience but slight differences in regard to the sexes of the patients.

Causes.—Sudden changes in temperature are apt to give rise to diphtheria by producing catarrhal effusions. These changes occur in different months in different countries, or parts of countries. Thus the difference of opinion relating to the prevalence of diphtheria is easily explained, and no invariable season law, as proposed by Besnier, exists. Persons with a predisposition to catarrh and diphtheria will do well to avoid cold and dampness.

Exhalations of sewers, gutters, privies are frequently accused of being of themselves causes of diphtheria. If they be, their direct influence has not been so well proven as is the case in regard to dysentery and typhoid fever. They will swell the number of cases of diphtheria sufficiently by merely lowering the general standard of health of the individual or the community, thus facilitating the final appearance of diphtheria, which is produced by a specific poison. The cases reported as having occurred after drinking contaminated water or milk require farther study. In the latter cases particularly, the facility of carrying the disease in clothing, tools, and vessels, is an established fact.

The nature of the poison I do not wish to make the subject of an extensive discussion here. It is impossible to finish it, or to convince a sceptic, in the course of a discussion. The comparative value of the proofs for the chemical or parasitic nature of the poison can be studied only by referring to a number of facts and a vastness of literature. This I have tried to do in my treatise on diphtheria (New York, 1880). I cannot come to the conclusion that the victory has been won by the parasitists. The war-cry, "No bacteria, no diphtheria," is not any more justified than that fermentation or putrefaction depend on bacteria only. The presence of bacteria in the diphtheritic blood has not been proven. Bacteria are destroyed by alkaline solutions. There is no theoretical ground for assuming that preventing the bacteria contained in a diphtheritic patch from making their way through the underlying mucous membrane will *per se* prevent general diphtheritic infection of the system. On the contrary, the septic and putrid poison is claimed by A. Hiller as distinctly chemical. Of the same nature—viz., chemical—is in all probability the poison of those of the infectious and contagious diseases in which the presence of a characteristic parasite at a certain period of the disease is a recognised fact, as anthrax and relapsing fever. Its regular presence does not prove its being *the* cause.

Entrance of the poison, local or general.—It may be, and frequently is, quite local. Diphtheria may commence in a local wound, or on an exposed mucous membrane or denuded cutis, and from that origin infect the whole system. It may also, though not affecting the system of the patient who suffered locally, be thus communicated to others with all its severe consequences.

Besides, diphtheria is a frequent occurrence after operations, mainly in the pharynx. Within from sixteen to twenty hours the surface of an excised tonsil is covered with diphtheritic membrane, with or without fever, and with or without symptoms of general diphtheria. In common times and under favourable

circumstances this does not occur, but it so does whenever there is an epidemic of diphtheria; that means, when everybody is so predisposed that a slight additional cause is sufficient to produce the visible symptoms. Thus these operations ought to be postponed until after the disappearance of an epidemic. Thus in these latter cases the local cause supersedes the constitutional predisposition. In them, without the latter the former could not act; without the former the disease would not break out.

The constitutional origin of diphtheria is claimed by many for the following reasons:—It bears all the marks of a disease both infectious, contagious, and inoculable. It occurs in epidemics. If it occurs sporadically, the same must be said of measles or scarlatina, which nevertheless do not lose their standing in nosology. It differs from the latter inasmuch as it runs no typical course, and instead of destroying or diminishing, is apt to increase future predisposition.

It does not differ from them in this, that the epidemics are of varying intensity.

There are cases in which general symptoms precede, or appear to precede, the local ones, and in which the whole of the course is followed by general adynamic symptoms, and nervous prostration or paralysis.

Albuminuria is also of a more frequent and earlier occurrence than the height of the temperature would explain.

The constitutional origin, when assumable, must be explained by the direct unobstructed admission of the poison into the lungs, particularly in those cases in which nose, mouth, and pharynx are large and healthy and the tonsils small. The fifth part of the whole amount of blood is within the lungs, the exposed surface of their alveoli, of about two thousand square feet, being very permeable, for it consists only of basement membrane and fine capillary network, with a thin epithelial layer, which, moreover, is sometimes absent.

The constitutional symptoms depend on the amount of poison absorbed, the power of resistance of irritability on the part of the organism, and *very much* on the rapidity of individual metamorphosis and elimination. Protracted slow poisoning with diphtheria or typhoid poisons, accompanied with *rapid and steady elimination*, will result at most in gradual discomfort, malaise, or the ambulatory type of the disease, and not give rise to the outbreak of a serious illness.

Thus the entrance of diphtheria may be local or constitutional—that is, through the lungs or mixed; and accordingly the general symptoms, such as fever, lassitude, &c., may follow or precede the visible local symptoms.

Diphtheria is very contagious, the contagion may be transmitted by the patient, his dwelling, furniture, towel, his visitors, all his surroundings. The cracks in the floor, the carpets and curtains, may communicate the disease after months.

It may be transmitted, too, from some classes of animals in whom it has been observed—kittens, hens, and domestic fowls generally, calves, cows. Perhaps also through milk.

In dwellings it follows the current of air, mostly upwards.

Mild cases may give rise to serious ones, and *vice versa*.

The period of incubation lasts from two to fourteen days. Fresh wounds, however, will produce membranes within sixteen hours. On mucous membranes denuded of all their diphtheritic deposits I have seen thick membranes constructed in seven hours.

State.	SEX AND AGE.																														
	Total.	Under 1.	1.	2.	3.	4.	Total under 5.	5 to 10.	10 to 15.	15 to 20.	20 to 25.	25 to 30.	30 to 35.	35 to 40.	40 to 45.	45 to 50.	50 to 55.	55 to 60.	60 to 65.	65 to 70.	70 to 75.	75 to 80.	80 to 85.	85 to 90.	90 to 95.							
Alabama	23	2	5	7	4	2	20	1	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	31	2	2	6	5	1	16	8	3	3	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...						
	5	...	1	2	1	...	4	...	1	..	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	3	...	...	...	...	...	...	3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
Arkansas	23	4	2	4	3	2	15	6	1	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...						
	17	4	1	...	3	3	11	4	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	3	...	1	...	...	...	1	1	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...						
	5	2	...	2	...	...	4	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...						
California	23	1	1	2	3	5	12	4	3	2	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	36	...	3	6	4	4	17	12	4	1	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...						
Colorado	29	2	3	3	1	4	13	6	4	...	3	3	...	...	...	...	...	...	...	...	...	...	...	...	...						
	35	2	3	3	3	7	18	14	3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	37	3	3	3	4	6	19	12	5	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...						
Connecticut	56	1	3	9	11	5	29	21	2	...	1	1	...	...	...	...	...	1	...	...	1	...	...	...	...						
	2	2	...	...	...	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
Dakota	18	...	...	2	2	1	5	11	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	23	...	2	1	4	3	10	5	6	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...						
Delaware	8	...	3	...	1	4	8	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	8	...	...	2	...	...	2	4	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	1	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
Florida	4	...	1	2	1	...	4	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	2	1	...	...	...	...	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	1	1	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
Georgia	31	3	4	5	...	3	15	10	1	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...						
	28	...	5	7	3	4	19	8	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	7	...	2	...	1	...	3	4	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	17	1	1	4	1	...	7	8	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
Idaho	1	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
Illinois	242	7	21	34	25	30	117	75	33	10	...	3	1	...	...	...	1	...	...	...	...	...	...	...	...						
	228	17	26	23	27	24	117	61	32	10	3	3	...	...	...	...	...	...	...	...	...	...	...	...	...						
	3	...	...	...	...	...	...	3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
Indiana	174	11	12	22	16	31	92	48	20	6	...	3	...	...	1	1	...	1	...	1	...	...	...	...	...						
	259	19	25	37	42	22	145	65	27	9	3	1	1	...	...	1	...	...	...	...	...	...	...	...	...						
	2	...	...	...	...	...	...	1	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...						
	1	...	...	...	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
Iowa	381	12	25	36	50	43	166	102	68	19	8	2	2	1	1	1	...	1	1	...	...	2	...	...	...						
	368	11	19	43	33	27	133	118	81	19	8	1	1	2	...	...	1	...	...	...	...	...	...	...	...						
	1	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
Kansas	205	8	24	17	21	26	96	68	23	10	...	...	...	1	1	...	...	...	...	...	...	...	...	...	...						
	180	5	16	24	16	18	79	62	24	6	3	...	1	1	...	...	...	...	...	...	...	...	...	...	...						
	6	1	...	...	1	1	3	2	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	3	...	...	...	...	...	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
Kentucky	55	2	7	9	8	7	33	12	9	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	52	1	8	10	6	3	28	14	6	...	1	...	1	1	...	...	...	...	...	...	...	...	...	...	...						
	6	...	2	1	1	...	4	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	5	2	1	1	...	1	5	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
Louisiana	5	...	2	...	1	...	3	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	6	...	...	...	1	1	2	3	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	7	...	...	2	1	2	5	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	7	2	1	...	1	...	4	...	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
ne	85	3	4	5	11	9	32	33	13	3	...	...	1	...	...	1	...	1	...	...	...	...	...	...	...						
	96	1	6	12	12	8	39	28	16	7	2	2	1	...	...	...	...	...	...	...	...	...	...	...	1						
	38	2	1	6	7	8	24	10	3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	41	...	7	6	2	6	21	14	6	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	2	...	...	1	...	1	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	2	...	...	1	1	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	246	11	21	29	23	32	116	86	35	3	1	1	...	1	...	1	...	...	...	...	...	...	...	...	...						
	271	9	18	29	14	32	102	96	43	10	5	4	2	...	...	1	...	...	...	...	...	...	...	...	...						
	2	...	...	...	...	...	...	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	112	7	6	4	11	10	38	41	23	5	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	130	3	5	6	9	11	34	53	23	12	5	...	...	...	...	...	...	...	...	...	...	...	...	...	...						
	3698	164	303	428	396	407	1699	1156	528	144	48	29	13	8	4	6	2	4	1	1	1	2	...	1							

ttes, from June 1, 1879, *to May* 31, 1880.

r.	SEX AND AGE.																											
	Total.	Under 1.	1.	2.	3.	4.	Total under 5.	5 to 10.	10 to 15.	15 to 20.	21 to 25.	25 to 30.	30 to 35.	35 to 40.	40 to 45.	45 to 50.	50 to 55.	55 to 60.	60 to 65.	65 to 70.	70 to 75.	75 to 80.	80 to 85.	85 to 90.	90 to 95.	95 and over.	
orward	3698	164	303	428	396	407	1699	1156	528	144	48	29	13	8	4	6	2	4	1	1	1	2	...	1	...	...	60
	3	1	1	...	1	...	3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
. .	10	1	3	2	...	1	7	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1
	6	...	3	2	...	1	6	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	7	1	1	1	1	1	5	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	115	16	13	19	13	16	77	24	8	...	...	1	...	...	2	1	...	...	...	...	...	...	1	...	...	...	1
. . .	130	13	22	17	19	12	83	38	5	...	2	1	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...
	9	2	2	3	...	...	7	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	...	...	...	1
	11	...	2	...	1	1	4	6	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
. . .	5	...	...	...	1	...	1	2	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	7	1	2	...	...	...	3	3	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
. . .	137	3	9	16	16	22	66	48	18	...	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	3
. . .	136	4	10	11	11	10	46	52	30	...	3	...	1	1	2	...	...	...	...	...	...	...	...	...	...	...	1
	1	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	2	...	...	1	...	1	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
pshire	52	4	3	7	6	3	23	15	7	3	1	1	...	...	2	...	...	...	...	...	...	...	...	...	...	...	...
	64	4	3	2	4	3	16	23	12	7	1	...	1	1	2	1	...	...	...	...	...	...	...	...	...	...	...
	445	20	40	57	44	36	197	145	62	17	8	1	...	...	1	...	1	1	...	1	...	...	...	...	...	...	11
. .	418	7	31	44	52	34	168	142	60	16	9	3	3	5	1	1	1	1	...	...	...	...	...	...	...	...	8
	4	2	...	...	...	...	2	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	132	15	21	17	11	15	79	30	12	3	...	1	...	...	...	...	...	...	2	...	...	...	...	...	...	...	5
olina .	139	8	12	16	16	17	69	40	13	5	...	2	...	1	...	1	1	...	...	...	...	...	...	...	...	...	7
	18	2	1	3	3	2	11	6	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...
	19	...	...	4	4	3	11	6	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1
	365	7	28	40	45	42	162	134	40	8	5	2	...	4	...	...	1	...	...	...	...	...	...	...	...	...	9
. . .	346	10	17	35	35	24	121	141	50	13	4	2	1	3	1	...	1	1	...	...	...	...	...	...	...	...	8
	3	1	...	...	...	...	1	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
. . .	21	...	3	1	2	2	8	10	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1
	24	...	2	3	3	3	11	9	4	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	801	33	77	103	90	88	391	258	92	22	3	6	2	1	...	...	1	1	...	...	...	...	...	...	...	...	24
nia .	878	24	53	67	97	93	334	330	142	34	9	3	2	1	3	1	...	2	...	...	...	...	...	...	...	...	17
	2	...	...	...	2	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	6	...	...	...	...	...	...	4	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	19	2	2	4	...	3	11	6	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	1
nd .	24	1	...	1	6	6	14	8	...	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	2	1	...	...	...	...	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	2	...	1	1	...	...	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	27	2	3	3	6	2	16	8	3	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
olina .	26	2	1	4	5	2	14	7	3	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	1
	9	1	5	...	...	1	7	2	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	11	2	2	2	3	1	10	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1
	83	4	11	11	17	12	55	23	4	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...
. .	87	8	8	10	3	13	42	32	11	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2
	16	4	1	2	1	1	9	5	1	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...	...	...	...
	23	4	2	4	...	5	15	3	3	...	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	25	3	2	6	3	6	20	2	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1
. . .	23	1	5	5	2	5	18	4	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	2	...	1	...	...	...	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	5	...	1	1	...	...	2	1	...	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
. . .	57	3	6	1	6	12	28	22	2	4	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1
	67	...	...	5	10	9	24	20	20	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2
. . .	39	3	1	9	2	2	17	12	4	2	2	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1
	34	1	4	2	2	2	11	12	7	4	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	59	2	10	5	6	6	29	15	10	1	1	...	1	...	...	1	...	...	...	...	...	...	...	...	...	...	1
. . .	64	1	6	1	5	5	18	26	12	4	1	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	1
	9	...	...	1	1	1	3	4	...	...	...	1	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...
	14	...	1	...	3	1	5	5	2	1	...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	...
	23	...	1	3	1	2	7	7	6	2	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
on Ter.	14	...	...	1	...	1	2	2	4	6	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	2	...	...	...	1	...	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	98	2	9	5	4	20	40	35	16	4	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2
ginia .	81	2	5	14	9	6	36	28	11	5	...	...	...	...	...	...	...	...	...	1	...	...	...	...	...	...	...
	3	...	...	...	1	1	2	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
	2	...	...	...	1	...	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
. . .	191	7	18	25	21	15	86	56	32	7	5	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	5
	198	5	10	9	19	14	57	86	35	8	2	1	1	...	...	...	...	...	...	...	...	...	...	...	...	...	8
d total	9354	405	778	1034	1011	991	4219	3057	1275	329	112	57	28	28	18	13	10	10	4	2	2	2	2	1	...	...	185

DISCUSSION.

Dr. SQUIRE, London: In answer to Dr. Airy, I would say that exposure o germs to air oxidates and destroys them. I think the remark of Dr. Jacobi that infection diminishes rapidly with distance, is well founded. One of hi reasons for the predisposition of children to diphtheria is their greater proximity to the infecting source. I would extend this to their nearness to their nurse and to other children, as well as to sewers and to exhalations of the ground. quite agree with Dr. Jacobi as to the important part played by all the cause of individual predisposition in determining an attack of diphtheria, and a glad to see that he also fully admits the influence of contagion, and the ca necessary against its conveyance by clothes and utensils.

Dr. ASHBY, Manchester: I would draw attention to the curious distributio of diphtheria, including its varieties. Thus, in comparing large cities, diphtheri or diphtheritic croup is abundantly common in London and Liverpool, but com paratively rare in Manchester. In the latter city a few cases appear in th suburbs, but very few in the city or adjoining township of Salford. The reaso for this is far from obvious. With regard to the question of the conveyance the diphtheritic poison by means of wind, while no doubt dust, smut, an various vegetable spores are carried long distances in this way, it is contra to all our ideas concerning the spread of zymotic diseases. Modern treatme of specific febrile affections, and the precautions taken to prevent their sprea very strongly point to the destruction of the poison, whether "vital" or chemica by the abundant mixture of fresh air.

Ueber den Zusammenhang der Chorea mit dem Rheumatismu in besonderer Beziehung auf die Herzgeräusche, die häufig Chorea begleiten.

Dr. STEFFEN, Stettin.

Seit geraumer Zeit haben verschiedene Autoren annehmen zu müssen glaubt, dass ein bestimmter Zusammenhang zwischen Chorea und Gelen Rheumatismus bestehe. Man hat dies zum Theil daraus gefolgert, dass m die eine Krankheit der anderen hat vorausgehen oder derselben folgen sehe Man hat sogar Fälle zur Unterstützung herangezogen, in welchen Pausen v Monaten und Jahren zwischen beiden Processen bestanden haben. Einen wi tigeren Grund für die Wechselbeziehung beider Processe fand man in der End carditis, welche nach der Annahme vieler in der Regel beide Krankheiten gleiten und so in gewisser Weise ein Bindeglied zwischen beiden darstell solle.

Es scheint mir, dass der Zusammenhang der Chorea mit dem Gele Rheumatismus in verschiedenen Gegenden sehr verschieden ist, in manch beobachtet man, dass dieselben sehr häufig zu einander in Beziehung steh in anderen, wie z. B. in Stettin und Umgegend, wird dies äusserst selten funden. Gelenk-Rheumatismus kommt bei uns im kindlichen Alter sehr selt vor, wogegen man Chorea, namentlich in der ärmeren Klasse, ziemlich häu auftreten sieht, so dass Exemplare dieser Krankheit fast stets in dem, un meiner Leitung stehenden, Kinderspital zu finden sind.

Was das Verhältniss des Gelenk-Rheumatismus zur Endocarditis betrifft,

kommen Fälle des ersteren ohne die letztere vor, wenigstens ohne dass diese klinisch nachgewiesen werden könnte. Man hat dann das Recht anzunehmen, dass sie nicht vorhanden gewesen ist, weil man wohl im Stande ist, die Diagnose derselben zu machen.

Andererseits kommen Fälle einfacher Endocarditis zur Beobachtung, in welchen von Rheumatismus gar keine Rede sein kann. Man kann unmöglich in solchen Fällen, in welchen keinerlei Zeichen von Rheumatismus existiren, annehmen, dass die Endocarditis eine rheumatische Ursache habe und in Folge von Erkältung entstanden sei. Wollte man sich solchen Auffassungen anbequemen, so würde man den Rheumatismus als eine äusserst häufige Ursache der verschiedensten Krankheitsprocesse hinstellen können.

Die Häufigkeit des Auftretens einfacher Endocarditis ohne Complication mit Rheumatismus, scheint in den verschiedenen Gegenden verschieden zu sein. Wahrscheinlich würde dieser Process häufiger zur Beobachtung kommen, wenn man es sich zur Regel machte, bei jeder ernstlichen Krankheit das Herz genau und mehrfach zu untersuchen. In dem 3ten Bande, N. F. des Jahrbuchs für Kinderheilkunde 1870, p. 393, habe ich in einem Aufsatze: "Beiträge zur Lehre von den Herzkrankheiten," bereits mehrere Fälle einfacher Endocarditis zusammengestellt, welche ohne vorgängigen Rheumatismus unter meinen Augen im Spital zur Entwickelung gekommen und nach Wochen mehr oder minder rückgängig geworden waren, ohne dass sich irgend ein Symptom von Rheumatismus gezeigt hatte.

Wenn wir also vom Rheumatismus absehen, und bloss die Beziehungen der Chorea zur Endocarditis in das Auge fassen, so findet sich bei aufmerksamer Beobachtung, dass beide Krankheiten häufig vereint vorkommen und dass in unserer Gegend dies vielleicht die Regel ist. Ich habe nie Chorea auf Grundlage von Endocarditis sich entwickeln, sondern letztere nur im Verlauf der ersteren auftreten sehen. Es scheint mir, dass in den Fällen, in welchen man die Endocarditis als die primäre Krankheit gesehen zu haben glaubt, die unscheinbaren Anfänge der Chorea der Beobachtung entgangen sind.

Da die pathologische Anatomie des Processes der Chorea für uns im Dunkeln liegt, so lässt sich eben so wenig ein Grund für die Complication mit Endocarditis nachweisen.

Um die klinischen Beziehungen zwischen beiden Processen nachzuweisen, ist zunächst eine genaue Feststellung der Erscheinungen, mit welchen die Endocarditis auftritt und verläuft, nothwendig. Bekanntlich sind wir in der glücklichen Lage, im kindlichen Alter die Lage und Grösse des Herzens mit der grössten Genauigkeit festsetzen zu können. Bei hinreichender Uebung kann man sogar in den meisten Fällen die Grenze zwischen den Vorhöfen und Ventrikel bestimmen. Die Methode dieser Untersuchung findet sich in dem oben angeführten Aufsatz ausführlich auseinandergesetzt. Hat man diese Untersuchung genau ausgeführt, so ist es nicht schwierig, die pathologischen Veränderungen des Herzens zu constatiren.

Jede frisch auftretende Endocarditis ist von Fieber (40° bis sogar zu 42° hinauf) begleitet. Die örtlichen Erscheinungen differiren, je nachdem die Entzündung von vorn herein die Klappen oder das Endocardium der Wandungen betroffen hat. Es braucht kaum erwähnt zu werden, dass es sich in der Regel nur um Erkrankung der linken Herzhälfte handelt.

Bei der primären Affection der Klappen findet sich der erste Ton über der Herzspitze von einem meist sausenden Geräusch begleitet. Ein präsystolisches Geräusch wird im kindlichen Alter selten beobachtet. Der zweite Pulmonalarterienton ist stärker accentuirt. Nach wenigen Tagen folgt eine Dilatation des Herzens, welche hauptsächlich oder fast ausschliesslich die linke Herzhälfte betrifft. Diese Dilatation wird bedingt durch eine seröse Durchfeuchtung und

Erschlaffung der Herzwandungen, deren Endocardium von dem entzündlichen Process mit ergriffen ist. Mit dem Beginn der Dilatation rückt die Herzspitze mehr nach aussen und auch etwas nach unten. Seltener findet man die Herzdämpfung am rechten Rande des Sternum verbreitert. Diesem Befunde entsprechend, lässt sich eine Zunahme der Breite und Länge des Herzens nachweisen. Entweder erlangt eine solche Dilatation in wenigen Stunden ihr Maximum, oder sie schreitet allmählig von Tag zu Tage fort, bis sie ihren Höhepunkt erreicht hat. Es lassen sich Zunahmen der Herzlänge um 1–3½ C., der Herzbreite um ½–2 C. constatiren. Den geringsten Grad von Dilatation findet man, wenn dieselbe sich in wenigen Stunden entwickelt und damit ihren Höhepunkt erreicht hat.

Wenn der entzündliche Process zuerst das Endocardium der Wandungen ergreift, so kann derselbe in nicht seltenen Fällen auf diese Region beschränkt bleiben. Ich habe einen solchen Fall bei einem Knaben von 13 Jahren beobachtet und die Diagnose durch die Autopsie bestätigen können. Der verstorbene Professor Traube theilte mir mündlich mit, dass er ebenfalls einen derartigen Vorgang post mortem constatirt habe. Bei vorhandener Chorea trat durch einen hinzutretenden Process der exitus lethalis ein. Die Endocarditis hatte sich durch keine Veränderung der Herztöne noch sonstige auffällige Symptome kundgegeben.

Die Fälle, welche ich im Spital sich unter meinen Augen habe entwickeln sehen, zeichneten sich neben lebhaftem Fieber zuerst durch eine Zunahme der Herzdämpfung aus. Die Herzspitze überschritt die, bei der Aufnahme festgesetzte Stelle nach links und etwas nach unten, seltener trat eine Verbreiterung der Herzdämpfung auf der rechten Seite des Sternum auf. In der Regel entwickelt sich diese Dilatation nicht plötzlich, sondern lässt sich schrittweise von Tag zu Tage nachweisen, lässt auch zuweilen nach und nimmt wieder zu. Die Zunahme der Breite und Länge des Herzens hält die gleichen Maasse ein wie in den Fällen, in welchen der Process primaer die Klappen ergriffen hatte. Bei der lebhaften und frequenten Action des Herzens sind die Töne lauter, namentlich der erste, welcher auf der Höhe des Processes so verstärkt sein kann, dass der zweite schwächere kaum gehört wird. Zuweilen ist jedoch schon um diese Zeit, jedenfalls aber mit dem Nachlass des Fiebers der zweite Pulmonalarterienton stärker accentuirt.

Wenn die Entzündung auf das Endocardium der Wandungen beschränkt bleibt, so vergehen mehrere Stunden, in der Regel Tage, bis die Dilatation einen solchen Grad erreicht, dass eine mechanische Insufficienz der Klappen bewirkt wird. Da der linke Ventrikel die allein oder hauptsächlich ergriffene Region ist, so wird man das entstehende Sausegeräusch in der Regel auf den linken Ventrikel und speciell auf die Herzspitze beschränkt finden. In seltneren Fällen findet man es weiter über die Herzdämpfung verbreitet. Selbstverständlich ist es systolisch.

Ich habe in einem derartigen Fall, welcher in meinem Spital beobachtet wurde, die Diagnose durch die Autopsie bestätigen können. Es war dies ein Knabe von 13 Jahren, welcher mit einer jauchigen Periostitis der linken Tibia und der rechten Ulna aufgenommen wurde. Chorea war freilich nicht zugegen, doch instruirt dieser Fall die Verhältnisse der Endocarditis. Der erste Herzton war von einem schwachen sausenden Geräusch begleitet. Die Zunahme der Breite und Länge des Herzens gestaltete sich folgendermassen:

	BREITE.	LÄNGE.
Am 1sten Tage . . .	8¼	12¼
Am 3ten Tage . . .	8¼	14
Am 5ten Tage . . .	8½	15
Am 6ten Tage . . .	10	15½

Seitdem fand keine Zunahme der Dilatation statt. Der Spitzenstoss, welcher bei der Aufnahme die linke Mamillarlinie kaum nach aussen überschritt, befand sich auf der Höhe der Dilatation $3\frac{1}{2}$ C. ausserhalb dieser Linie. Bei der Autopsie fand sich das Herz dilatirt, die Klappen normal, dagegen war Endocarditis der Wandungen des linken Ventrikel vorhanden, verbunden mit zahlreichen kleinen Apoplexieen.

Wenn sich der Process der Endocarditis von den Wandungen auf die Klappen verbreitet, so findet zunächst keine Veränderung der Symptome statt, aus welcher man diesen Vorgang nachweisen könnte.

Endocarditis, welche sich auf Grundlage von Chorea ohne Complication mit Rheumatismus entwickelt, wird wohl in der Regel rückgängig. Ich habe keinen Fall mit tödtlichem Ausgang beobachtet. Blache (Essai sur les maladies du cœur chez les enfants p. 128) führt den Fall eines zwölfjährigen Mädchens an, welches an Gelenk-Rheumatismus, Chorea, dann secundär an Endocarditis und Pericarditis erkrankte und nahezu geheilt entlassen wurde. Derselbe Autor referirt dagegen über einen analogen Fall bei einem Mädchen von vierzehn Jahren, welches von denselben Processen heimgesucht war und an doppelseitiger Pleuritis zu Grunde ging. Die Autopsie entsprach dem klinischen Befund. Die Mitralklappe war insufficient und geschrumpft.

Man sieht in der Regel mit dem Rückgängigwerden der Endocarditis die Symptome der Chorea allmählig oder auch mehr plötzlich in ihrer Heftigkeit nachlassen. Das Fieber, welches im Beginn der Endocarditis am hochgradigsten war, lässt nach. Die Herzthätigkeit ist in der ersten Zeit eine unregelmässige, wechselt fast jeden Augenblick ihren Rhythmus. Die Herztöne zeigen Verschiedenheiten in Bezug auf ihre Intensität. Nicht selten findet man den ersten Ton gespalten und den zweiten Pulmonalarterienton abwechselnd stärker accentuirt.

Mit dem Rückgängigwerden der Endocarditis nimmt allmählich die Dilatation des Herzens und die Vergrösserung der Herzdämpfung ab. Die Herzspitze kehrt allmählig vollständig oder nahezu an die Stelle zurück, welche sie vor der Endocarditis eingenommen hatte. Die Breite und Länge des Herzens kehren allmählig ebenfalls vollständig oder nahezu zur Norm zurück.

Als Beispiele dienen folgende Messungen von einfacher Endocarditis.

Ein Knabe von 10 Jahren, welcher ohne Chorea an einer frischen Endocarditis erkrankt war, hatte am Tage der Aufnahme in das Kinderspital:

	BREITE.	LÄNGE.
1ster Tag	$5\frac{3}{4}$	8
2ter Tag	$6\frac{1}{2}$	$8\frac{1}{2}$
7ter Tag	7	10
10ter Tag	$7\frac{3}{4}$	$11\frac{1}{2}$
15ter Tag	7	10
45ster Tag	$6\frac{1}{2}$	$9\frac{1}{2}$

Ein Mädchen von 10 Jahren ohne Chorea an Endocarditis erkrankt:

	BREITE.	LÄNGE.
1ster Tag	7	10
4ter Tag	$7\frac{3}{4}$	$10\frac{1}{2}$
10ter Tag	8	12
13ter Tag	7	10
46ster Tag	7	$10\frac{1}{2}$.

Wenn die Endocarditis nur die Wandungen betraf, die Klappeninsufficienz also mechanischer Natur war, so verliert das den ersten Herzton begleitende

Sausegeräusch mit der Abnahme der Dilatation an Intensität, um allmählig ganz zu schwinden. Gleichen Schritt hält hiermit die Abnahme der verstärkten Accentuation des zweiten Pulmonalarterientones.

Wie vollständig die Erscheinungen der Endocarditis schwinden können beweisen folgende Fälle.

Ein Mädchen von 9 Jahren ist in diesem Jahr zum 4ten Mal wegen Chorea im Kinderspital aufgenommen worden. In jedem der drei letzten Jahre war die Erkrankung aufgetreten. Die beiden ersten Male hatten sich keine Symptome von Endocarditis gezeigt, nur fand sich bei dem zweiten Mal bei normaler Grösse, Lage und Tönen des Herzens, die Action desselben hier und da unregelmässig und zuweilen etwas verlangsamt. Im dritten Mal war Endocarditis mit deutlicher Dilatation des Herzens aufgetreten und vollständig rückgängig geworden, wie der Befund bei der vierten Aufnahme des Kindes nachweist. Es wurde vollkommen normale Lage und Grösse des Herzens constatirt. Die Action desselben war nicht immer regelmässig, in Folge davon die Intervalle zwischen den Tönen oft ungleich, der erste Ton war zeitweise von einem schwachen sausenden Geräusch begleitet. Zwei und eine halbe Woche nach der Aufnahme steigerte sich die Chorea in dem Maasse, dass die Kranke das Bett hüten musste. Zwei Wochen später fand sich der erste Ton zeitweise von einem dumpfen klingenden Geräusch, wie von dem einer Basssaite begleitet. Wenige Tage später, am 5. März d. J., Abends, traten unter lebhaftem Fieber die Zeichen von Endocarditis auf. Die Herzaction war ausserordentlich beschleunigt, der erste Ton sehr stark accentuirt, der zweite kaum zu vernehmen. Am folgenden Tage konnte bereits Dilatatio cordis nachgewiesen werden. Das Herz hatte seine Lage nicht verändert, dagegen hatte sich die grosse Herzdämpfung nach links verbreitert, die Herzspitze stand über 1 Centimeter ausserhalb der linken Mamillarlinie, während sie sich bei der Aufnahme genau in derselben befunden hatte. Bei beträchtlicher Beschleunigung der Herzaction war der erste Ton verschärft, der zweite beinahe ausfallend. Am 7ten März erschienen beide Töne gleich stark. Vom folgenden Tage ab war der erste Ton in der Regel von einem deutlich sausenden Geräusch begleitet und der zweite Pulmonalarterienton stärker accentuirt. Die Herzerweiterung schwankte, indem die Herzspitze bald nur $\frac{1}{2}$, in der Regel aber über 1 Cent. die linke Mamillarlinie überschritt. Die höchste Höhe des Fiebers hatte 400 betragen. Am 4ten Tage der Endocarditis begann das Fieber nachzulassen, schwand aber erst vollständig am 7ten Tage. Mit dem Schwinden des Fiebers mässigten sich die Erscheinungen der Chorea. Allmählig liess die Dilatation des linken Ventrikel nach. Am 31sten März war das Herz zu derselben Grösse zurückgekehrt, welche bei der Aufnahme der Kranken constatirt werden konnte, die Herzspitze stand wieder in der linken Mamillarlinie, der erste Ton war klar, ohne irgend welches Geräusch, und die Accentuation des zweiten Pulmonalarterientones geschwunden. Seitdem sind diese Verhältnisse dieselben geblieben und die Symptome der Chorea immer mehr zurückgegangen.

Ein Knabe von 14 Jahren wird am 28sten Mai 1880 mit Chorea aufgenommen. Die Untersuchung des Herzens weist normale Verhältnisse nach, namentlich steht die Herzspitze etwas nach innen von der linken Mamillarlinie. Die Erscheinungen der Chorea sind sehr hochgradig. Zwei Tage später treten rheumatische Schmerzen ohne wesentliches Fieber auf, welche wechselsweise verschiedene Gelenke befallen, das Herz ganz intact lassen und nach Na salicyl. vollkommen schwinden. Etwa vierzehn Tage, nachdem der Rheumatismus vollständig beseitigt war, entwickelt sich Endocarditis der Wandungen des linken Ventrikels unter mässigem Fieber. Die Herzdämpfung ist nach allen Richtungen um $\frac{1}{2}$–$1\frac{1}{2}$ C. vergrössert, der erste Ton ist überall von einem lauten haften Sausegeräusch begleitet, der zweite Pulmonalarterienton verstärkt,

Herzspitze steht 1 Cent. nach aussen von der linken Mamillarlinie. Nach anderthalb Wochen lässt sich eine allmählige Abnahme der Dilatatio cordis nachweisen. Sechs Tage später hat die Herzdämpfung nach allen Richtungen um 1–1½ C. abgenommen, also etwa die Maasse wieder erreicht, welche bei der Aufnahme des Kranken constatirt worden waren. Die Herzspitze stand in der linken Mamillarlinie, überall konnte man bei dem ersten Ton ein schwaches Sausgeräusch nachweisen. Bis zu der 3 Wochen später erfolgten Entlassung des Kranken waren diese Verhältnisse unverändert geblieben. Mit dem Rückgängigwerden der Endocarditis lassen die Erscheinungen der Chorea schnell nach, um bald gänzlich zu schwinden.

Es bleibt in diesem Fall, der mir später nicht wieder zu Gesicht gekommen ist, unentschieden, ob es sich bloss um eine wandständige Endocarditis gehandelt hat, oder wie weit die Klappen, namentlich die Mitralis, von dem entzündlichen Process betroffen worden sind.

Wie vollständig eine wandständige Endocarditis rückgängig werden kann, beweist schliesslich noch der folgende Fall, in welchem es sich nicht um Chorea sondern um Gelenk-Rheumatismus mit Endocarditis handelt.

Ein Mädchen von 12 Jahren wurde am 7ten Juli 1880 mit heftigem Rheumatismus im linken Hüft- und Knie- und im rechten Hand-Gelenk, aufgenommen. Lebhaftes Fieber. Lage und Grösse des Herzens normal. Schwaches Sausegeräusch bei dem ersten Herzton, der zweite Pulmonalarterienton nicht verstärkt. Allmähliger Rückgang der Erscheinungen bis Ende September, wo die Kranke fällt und sich in Folge davon eine neue heftige Entzündung des linken Kniegelenks entwickelt. Vier Tage nach diesem Unfall lässt sich von Neuem ein schwaches Sausegeräusch constatiren, welches den ersten Herzton begleitet und am deutlichsten über dem linken Ventrikel gehört wird. Vierzehn Tage nach dem Beginn des Recidius lässt sich eine beträchtliche Vergrösserung der Herzdämpfung nachweisen. Der Spitzenstoss, welcher sich innerhalb der linken Mamillarlinie befand, zeigte sich jetzt 2–3 Centimeter ausserhalb derselben, der erste Ton war von einem starken Sausegeräusch begleitet. Nachdem diese Erscheinungen bis Ende October ziemlich constant gewesen waren, trat Anfang November ein allmähliger Nachlass derselben ein, die Herzdämpfung verkleinerte sich schrittweise, bis der Spitzenstoss einen halben Centimeter innerhalb der linken Mamillarlinie sich befand. Gleichzeitig wurde das sausende Geräusch schwächer und war völlig geschwunden, als das Herz seine normale Grösse wieder erreicht hatte. Anfang December waren vollständig normale Verhältnisse des Herzens eingetreten und sind dieselben bis Ende März d. J., innerhalb welcher das Kind mehrfach untersucht worden ist, unverändert geblieben.

Diese Beispiele beweisen, dass eine Endocarditis der Wandungen so vollständig rückgängig werden kann, dass keinerlei von der Norm abweichende Erscheinungen zurückbleiben. Zuweilen hält sich noch, nachdem die Grösse des Herzens wieder normal geworden ist, eine Zeit hindurch ein schwaches sausendes Geräusch, welches den ersten Ton begleitet und allmählig auch vollständig schwindet. Wenn die Endocarditis nicht auf die Wandungen beschränkt war, sondern primär oder secundär die Klappen ebenfalls ergriffen hat, so können dieselben, wenn der Process nicht zu ausgedehnt und zu hochgradig war, wahrscheinlich wieder schlussfähig werden. Wenigstens ist durch Autopsieen bei Erwachsenen festgestellt, dass nach Schrumpfung eines Klappenzipfels wieder vollständiger Schluss des Ostium dadurch bewirkt werden kann, dass die andere oder die anderen Klappenzipfel, wenn drei vorhanden sind, sich allmählig mehr und mehr ausbuchten.

Wenn nach Endocarditis der Klappen der Process rückgängig wird, aber Schrumpfung der befallenen Zipfel eintritt, so sieht man zunächst ebenfalls die

Grösse der Herzdämpfung abnehmen und allmählig mehr oder minder nahezu die normale Grösse erreichen. Später bildet sich dann, wenn die Klappen-Insufficienz bestehen bleibt, allmählig Dilatation und zur Compensation derselben Hypertrophie des Herzens aus.

Als Zeichen der bei Chorea acut auftretenden Endocarditis sind also anzusehen:

Ein plötzlich aufspringendes heftiges Fieber, Zunahme der Grösse der Herzdämpfung in Folge von Dilatatio cordis, lebhafte Herzaction, sausendes Geräusch, welches den ersten Herzton begleitet, Verstärkung des zweiten Pulmonalarterientons. Wenn diese Symptome nicht sämmtlich vorhanden sind, wird man eine Ausbildung von Endocarditis nicht annehmen dürfen. Ist man nicht in der Lage gewesen, den Beginn derselben beobachten zu können, so kann das Fieber vollkommen bereits geschwunden sein, doch müssen die übrigen Symptome vorhanden sein, um die Diagnose auf Endocarditis stellen zu dürfen.

Unter den übrigen Erscheinungen der Endocarditis steht die acute Dilatatio cordis in erster Linie. Ohne diese kann von einer acuten Endocarditis keine Rede sein. Doch muss man im Auge behalten, dass es Fälle von plötzlich auftretender Dilatatio cordis giebt, welche nicht in Folge von Entzündung des Endocardium entstanden sind. Hierher gehören Krankheitsprocesse, welche schnell und in grosser Ausdehnung die Blutcirculation in den Lungen beschränken, wie ausgebreitete croupose Pneumonie, diffuse Bronchitis und Lungenödem. In solchen Fällen betrifft die Dilatation überwiegend das rechte Herz, doch wird ziemlich regelmässig auch das linke in Mitleidenschaft gezogen. Systolische Sausegeräusche werden dabei nicht beobachtet, doch findet sich der zweite Pulmonalarterienton stärker accentuirt.

Einen sehr interessanten Fall von acuter Dilatatio cordis ohne ein den ersten Herzton begleitendes sausendes Geräusch und ohne Verstärkung des zweiten Pulmonalarterientons habe ich bei septischer Infection beobachtet.

Ein Knabe von 11 Jahren, welcher wegen Caries im rechten Kniegelenk in das Kinderspital gebracht worden war, wurde am 27sten October 1880 unter allen Vorsichtsmaassregeln amputirt. Am folgenden Tage lebhaftes Fieber, Spuren von Carbolintoxication im Urin. Am 30sten October Osteomyelitis im Stumpf, Schüttelfrost, Collapsus. Am nächsten Tage war Dilatatio cordis deutlich zu constatiren. Die Herzspitze überschritt die linke Mamillarlinie um mehr als 1 Cent., während sie bei der Aufnahme etwas nach innen von derselben gestanden hatte. Der Puls war unregelmässig, sehr frequent, klein und weich. Am Abend Nachlass des Fiebers. Am 3ten November fieberfrei, die Dilatation bedeutend geringer, der Spitzenstoss in der linken Mamillarlinie, die Arterien gespannter, die Pulsfrequenz geringer. Kein Eiweiss im Urin. Am 6sten November trat ein neuer Schüttelfrost mit den gleichen Erscheinungen wie der erste, auf. Das Fieber war hoch, es entwickelte sich wiederum Dilatatio cordis, welche mit dem Schwinden des Fiebers ebenfalls rückgängig wurde. Als sich am 10ten November derselbe Vorgang wiederholte, betrug am folgenden Tage die Breite des Herzens $7\frac{1}{2}$, die Länge $11\frac{1}{2}$ gegen $6\frac{1}{4}$ und $10\frac{1}{4}$ bei der Aufnahme, die Herzspitze schlug über einen Centim. nach aussen von der linken Mamillarlinie an. Am 13ten wurde ein Theil des Knochenstumpfs resecirt. Am folgenden Tage war die Dilatatio cordis geringer. Am 23sten November wurde constatirt, dass die Grösse des Herzens vollständig wieder die normalen Grenzen erreicht hatte. Die Genesung machte dauernde Fortschritte. Die Grösse und Lage des Herzens zeigte dauernd normale Verhältnisse. Die beiden ersten Schüttelfröste wurden mit Hydrochinon, der dritte mit Natr. salicyl. bekämpft.

Die kurze Dauer des Bestandes der Herzerscheinungen, das schnelle und vollständige Schwinden und das wiederholte Auftreten desselben Vorganges nach kurzem Intervall beweist, dass es sich hier nicht um eine Endocarditis,

sondern um eine Erschlaffung der Herzwandungen in Folge der septischen Infection gehandelt hat. Beweisend dafür ist auch die jedes Mal mit dem Anfall auftretende Kleinheit und Weichheit des Pulses.

Es wird nicht schwer halten, eine septicämische Dilatatio cordis von einer durch Endocarditis bewirkten zu unterscheiden.

Der Unterschied zwischen der letzteren und den durch Behinderungen im Lungenkreislauf entstandenen Dilatationen des Herzens liegt zunächst darin, dass die letzteren zunächst die rechte Herzhälfte ergreifen und an und für sich nicht mit Fieber verknüpft sind, das Auftreten der Dilatation also auch nicht durch die Steigerung eines etwa vorhandenen Fiebers markirt wird. Ferner sichern die nachweisbaren Ursachen der Dilatation die Diagnose und schliesslich werden die Fälle, in welchen sich in Folge von Erkrankung der Lunge eine Dilatatio cordis deutlich constatiren lässt, in der Regel lethal ablaufen.

Wenn keine Dilatatio cordis bei Chorea vorhanden ist, so kann es sich um keine florirende Endocarditis handeln, wenn auch der erste Ton von einem Geräusch begleitet und der zweite Pulmonalarterienton verstärkt ist. Man hat dann eine im Ablauf begriffene Endocarditis vor sich, in welcher die Dilatation geschwunden und das Herz zur normalen Grösse zurückgekehrt ist. Dann wird auch das systolische Geräusch und die Verstärkung des zweiten Pulmonalarterientones allmählig schwinden oder, wenn der Process Klappen ergriffen und zum Schrumpfen gebracht hat, meistens bestehen bleiben und nach und nach secundaere Dilatation und Hypertrophie des Herzens zur Entwickelung kommen.

Nun giebt es aber Fälle von Chorea, in welchen unzweifelhaft keine Endocarditis vorhanden ist. Es fehlt die Dilatation, das Fieber, dagegen ist der erste Herzton, entweder hauptsächlich an der Herzspitze oder auch verbreitet über der ganzen Herzdämpfung von einem sausenden Geräusch begleitet, oft auch der zweite Pulmonalarterienton verstärkt. Charakteristisch ist für diese Fälle, dass diese Erscheinungen nicht dauernd sind, sondern, oft nach ganz kurzem Bestehen schwinden und wieder auftreten. Hier handelt es sich nicht um einen Process, welcher das Endocardium und speciell die Klappen betroffen hat, sondern um eine Störung in der Herzthätigkeit. Man braucht nur wenige Minuten lang das Herz eines an Chorea leidenden Kindes zu auscultiren und zu palpiren, um sich zu überzeugen, wie wechselnd die Herzthätigkeit und wie unregelmässig der Rhythmus der Contractionen ist. Der Grund hiervon liegt theils in den, durch die Chorea mit afficirten Nerven des Herzens, theils in der wechselnden Störung der Blutcirculation. Die Ursache dieser letzteren besteht theils in der krampfhaften regellosen Respiration, theils in den krampfhaften Bewegungen des Körpers überhaupt, so dass also diese Erscheinungen von Seiten des Herzens um so eher zur Beobachtung kommen, je hochgradiger die Chorea entwickelt ist, je beträchtlicher die dadurch bewirkten Störungen der Circulation des Blutes und die davon abhängigen erhöhten Ansprüche sind, welche an die Arbeit des Herzens gestellt werden.

On the Relationship of Chorea to Rheumatism, with Special Reference to the Heart Murmur which so frequently attends Chorea.

Dr. Stephen Mackenzie, London.

As a means of ascertaining the relationship of chorea to rheumatism, I have carefully examined the records of the cases of chorea admitted as in-patients

into the London Hospital during the last six years (1875 to 1880 inclusive). During this period the records have been kept with precision by successive medical registrars, who have also exercised supervision over the ward-work. The cases possess for the purpose of this examination the advantage that, having been under the care and observation of many different physicians, any personal bias on the point investigated is practically neutralised. At the same time, the conclusions based on such observations can only be regarded as approximately correct. The fifteen minutes to which the reading of this paper is limited obliges me to confine myself strictly to the question on which I was asked to write—namely, the relationship of chorea to rheumatism, with special reference to the heart murmur. I am greatly indebted to Dr. Mills, of Montreal, for valuable assistance in preparing and arranging the material on which this paper is based.

During the six years there were 172 cases of chorea with sufficiently complete records for the purpose of the inquiry.

The sex and ages of the patients is shown in the following table:—

Sex.	No. of Cases.	Under 5.	5 to 10.	10 to 15.	15 to 20.	20 to 25.	25 to 30.	Over 30.
Males . .	51	...	11	28	10	2	...	...
Females . .	121	1	22	53	31	10	3	1
Total .	172	1	33	81	41	12	3	1

It will be seen that most of the cases occurred between the ages of five and twenty. As a matter of fact, there were only thirteen cases eight years of age or younger, and of these eight were eight years old.

Particulars as to the heart were obtained in 164 cases:—

In 89 or 54·26 per cent. there was a distinct murmur.
„ 21 „ 12·8 „ { the heart sounds were modified, but there was no distinct murmur.
„ 48 „ 29·27 „ there was no abnormality.
„ 4 „ 2·43 „ the signs were equivocal.

The nature of murmurs was as follows:—

A systolic murmur at apex in	73
A presystolic murmur at apex in	5
A presystolic and systolic murmur at apex in . .	7
A systolic murmur at base in	1
A systolic murmur at base and apex . . .	3
	89

It will be seen that in the cases in which a murmur was heard, it was at the apex in all except one case; in three cases there was a murmur at the base in addition to the apical bruit. Excluding these last cases, the relative frequency of apical to basic murmurs was as 85 to 1. Under the term "modified heart sounds" are included cases in which the heart sounds were "murmurish" or prolonged, but which the observer hesitated to describe as with a distinct bruit; cases in which the heart sounds were reduplicated, and cases in which the heart's action was irregular or tumultuous. These cases form a debatable ground as to whether the modification of the heart's sounds are indicative of valvular defect or organic lesion of any kind. Dr. Ernest Sansom has brought forward evidence that reduplication of the first sound may be due to mitral stenosis.*

* "On the Causes and Significance of Reduplication of the Sounds of the Heart," *Medical Times and Gazette*, January 9 and 16, 1881.

If to the cases in which there was an undoubted murmur be added the cases in which the heart sounds were modified in some way, we should have 89 + 21 = 110 cases, or 67·07 per cent. in which some cardiac abnormality existed. Keeping, however, to the cases in which a distinct murmur was present, we have 89 or 54·26 per cent. with bruits.* It must be remembered this is a calculation for all ages.

Having shown the frequency and clinical features of the cardiac abnormalities observed in chorea, I shall next proceed to examine into the essential nature of the lesion on which the alterations in the heart sounds depends. For that purpose there is evidence of two kinds:—(1.) The evidence afforded by examination of the heart after death. (2.) The evidence afforded by clinical examination of the heart at a period subsequent to the attack of chorea.

I.—*Condition of the Heart in Fatal Cases.*—Out of the whole number of cases analysed, seven proved fatal, and in six of these a post-mortem examination was made.

I give the briefest possible extracts of the cases:—

1.—Male, æt. 17. First attack. Had had rheumatism. Systolic murmur at apex. *Necropsy.* Vegetations on mitral valve.

2.—Female, æt. 19. Second attack. No history of rheumatism; no murmur. Pregnant. *Necropsy.* Vegetations on auricular surfaces of mitral valve.

3.—Female, æt. 10. Second attack. No history of rheumatism. Systolic and diastolic murmurs. Great anæmia. *Necropsy.* Incompetent aortic and mitral valves, with vegetations on each. Contracted mitral orifice. Hypertrophy and dilatation of left auricle and ventricle.

4.—Female, æt. 19. History as to rheumatism and antecedents not ascertainable. Systolic murmur at apex. Three months pregnant. In consequence of severity of symptoms, abortion induced. *Necropsy.* Pyæmia, heart fatty.

5.—Female, æt. 27. Second attack. Stated never to have had rheumatism. Six months pregnant. No murmur. No anæmia. *Necropsy.* Numerous vegetations on mitral valve.

6.—Male, æt. 13. Had had subacute rheumatism; subcutaneous fibrous nodules. Mitral systolic and presystolic murmurs; pericarditis. The patient died from exacerbation of pericarditis, the chorea having subsided. *Necropsy.* Old endocarditis, and recent vegetations on mitral and tricuspid valves. Acute pericarditis.

7.—Female, æt. 13. Fourth attack. Had had rheumatic fever. Presystolic and systolic murmurs at apex. Pneumonia. *No necropsy.*

Thus in all except one in which a post-mortem examination was made, vegetations were found on the mitral valve. In the case in which no vegetations were present, though pyæmia was the actual cause of death, the patient died *with* chorea. The heart, it will be observed, was fatty, and probably during life allowed of regurgitation, for a systolic mitral murmur was present. In the case in which no post-mortem examination was made, the patient had, whilst living, unmistakable physical signs of mitral stenosis.

It will thus be seen that heart disease of an organic kind existed in all the cases dying of or with chorea. Further, that there was endocarditis affecting the mitral valve in all except one case, where the heart was fatty. In two of the fatal cases with endocarditis no bruit was heard during life. From

* It is instructive, as showing that this is probably a very nearly correct estimate of the frequency of heart murmur in chorea in hospital patients at all ages, and not a biassed calculation of the author, to compare these figures with those of successive registrars during the same period. In 1876 Dr. Herman gives 57·14 per cent.; in 1877 Dr. Warner gives 47·7 per cent.; and in 1880 Dr. Gabbett gives 56·09 per cent. as the frequency of bruits. The mean of these is 53·79.

this it may be inferred that absence of murmur is no proof that the heart is unaffected with organic disease. This raises an important point. In chorea occasionally a murmur which is not present at first develops in the course of the disease. In such cases it is from the above possible that the disease of the valve, which ultimately causes the murmur, may have been present from the first; and also, that had the fatal cases in which no murmur was heard lived longer, a bruit might have become developed. In the case of the fatty heart without endocarditis, the patient was a pregnant female, and it might be assumed from this that vegetations are not found in the chorea of pregnant women. This conclusion is by no means warranted by the present series of facts, however, for in the two other fatal cases of chorea occurring in the pregnant, vegetations were found on the mitral valve.

II.—The evidence afforded by physical examination of the heart at some period subsequent to the attack of chorea.

This is a line of inquiry which, as far as I know, has been but little investigated. There are great practical difficulties in following up hospital cases after their discharge, but the trouble is repaid by the results. Those engaged in family practice might collect some really valuable information on this point. With the assistance of Mr. George Greenwood and Mr. Grün,* students of the London Hospital, I have been able to follow up thirty-three cases of chorea at periods varying from one to five years subsequent to their leaving the Hospital. I give in a table, as an Appendix, the results of my examinations of these thirty-three patients, but these results may be briefly summarised.

Condition of the heart in cases of chorea, examined at periods from one to five years subsequent to the attack for which the patients were in the Hospital:—

Cases with a distinct murmur	17
Cases with some cardiac abnormality otherwise than a murmur	7
Died of heart disease	3
Died of unknown causes	3
Heart normal	3
	33

It will be seen that there was a distinct murmur (as well as other signs of heart disease) in 20 (17 + 3) cases, or in 60·6 per cent. If to these cases with unquestionable heart disease be added the cases in which the heart sounds were distinctly altered, but in which there was no well-marked murmur, it gives 20 + 7 = 27 cases, or 81·81 per cent., in which some cardiac derangement persisted after the attack of chorea observed. If, as is possible, two, or all three, of the other fatal cases died of or with heart disease, the percentage of permanent cardiac abnormality would be still further increased.† Objection may reasonably be made to considering cardiac abnormalities other than distinct murmurs as evidence of organic heart disease. I would therefore be content with the statement that indisputable heart disease persisted in 60·6 per cent. of the series of chorea cases examined. But I think that hearts that show such abnormalities as the following at periods of from one to five years after chorea may fairly be regarded as at least suspicious of some organic lesion. These abnormalities were —1. Occasional reduplication of first sound, which is rather prolonged at apex. 2. Reduplication of first sound at apex. 3. Murmurish first sound at apex, especially on exertion; accentuated second sound. 4. First sound a little pro-

* Mr. Greenwood and Mr. Grün visited the patients at their homes, and sent them to see me at the Hospital, where I examined and noted the condition of the heart.

† The condition of the heart whilst in Hospital in the three fatal cases of which the cause of death was not ascertained was: 1. Systolic and presystolic murmurs. 2. Systolic murmur at apex; dilated heart. 3. Mitral systolic murmur.

longed, especially after exertion. 5. Excited action of heart; second pulmonary sound accentuated. 6. Intensified first sound; heaving beat. 7. Slight accentuation of pulmonary second sound.

The number of cases examined is small, however, and requires corroboration.

With reference to the PRE-EXISTENCE OF RHEUMATISM, the cases are divided into four classes:—

CLASS I.—Cases in which it was distinctly stated that the patient had suffered from rheumatic fever, or in which the patient had been laid up in bed with swelling of, and pain in, the joints.

CLASS II.—Cases in which the patient had suffered from "rheumatic pains," "pains in the joints," "growing pains," &c., but had not been laid up in bed with the attack; probably subacute rheumatism.

CLASS III.—Cases in which the history of rheumatism was doubtful, *i.e.*, the patient had had some illness, the nature of which could not be ascertained owing to the parents' or friends' ignorance, or to their want of opportunity of obtaining information on this point. It will be conceded that, in hospital practice in this metropolis, this class will embrace a good many cases in which the possibility of rheumatism having occurred will be considerable.

CLASS IV.—Cases in which no history of rheumatism could be obtained, even on inquiry on this point. The remark made on the last class applies almost equally to this. It cannot be safely assumed that rheumatism has not existed in cases where the parents and friends (of this class of society) are unaware of its occurrence.

Dividing, now, the cases into these four classes, we have:—

I. With a distinct history of rheumatism	47	or 27·32	per cent.*
II. With a history of pains of various kinds	30	„ 17·44	„
III. With a doubtful history of rheumatism	25	„ 14·53	„
IV. With no history of rheumatism	70	„ 40·69	„
	172	„ 99·98	„

If we separate the first attacks of chorea from the remainder, we have the following figures:—

CLASS I.	22	cases or 21·76	per cent.
„ II.	23	„ 22·54	„
„ III.	14	„ 13·72	„
„ IV.	43	„ 42·15	„
	102	„ 100·17	„

in which there was a history of rheumatism. It must be borne in mind that deduction should be made for what may be termed the age incidence of rheumatism, as well stated by Dr. Octavius Sturges.† This observer has, in dealing with a considerable number of cases, separated those below from those above eight years of age. The number of cases in this collection below eight years of age are so few (thirteen) that conclusions based on so small a number might mislead. A certain deduction should, however, doubtless be made for the natural incidence of rheumatism—one or two per cent.

Dealing with the whole number, without reference to the number of the attack, it has been shown that there was unmistakable evidence of rheumatism in 27·32 per cent. To these, I think, without straining the figures, the cases in

* The calculations of successive registrars agree very closely with my figures as to the frequency of a distinct history of rheumatism. Thus Dr. Herman, in 1876, gives 28·57; Dr. Warner, in 1877, gives 27·27; Dr. Gabbett, in 1880, gives 29·26 per cent. The mean of these numbers is 28·36 per cent. of cases of chorea with a distinct history of rheumatism.

† *Lancet*, 1879, vol. ii. p. 791; *Brain*, July 1881, p. 169.

Class II.—the patients who had suffered from pains of various kinds, probably subacute rheumatism—ought to be added. We should then have 47 + 30 = 77 cases, or 44·76 per cent. (nearly one-half) in which rheumatism was directly associated with the chorea. And treating the cases of first attacks of chorea in a similar manner, 22 + 23 = 45 or 44 per cent. (nearly) in which rheumatism *preceded* the chorea.

But I believe that the 44 per cent. in which rheumatism had *almost* certainly (27·32 certainly) existed, by no means represents the probable prevalence of rheumatism as an antecedent to the first or later attacks of chorea, and that in a very considerable (though not ascertainable) proportion of cases rheumatism must have occurred, though no history of its occurrence was obtained. I base my belief on this point mainly on the following grounds :—

1.—The difficulty of obtaining from the friends or parents of chorea patients of the class admitted into hospitals the nature of previous illnesses unless of a very definite character.

2.—The ill-marked characters which rheumatism in children, especially when subacute, often presents.

3.—The frequency with which heart murmurs were found in cases of first attacks in which rheumatism could not be ascertained to have pre-existed.

The last reason, I admit, seems dangerously like begging the question under discussion ; but seeing the proved connection between rheumatism and chorea, and the marked influence of the former in producing valvular disease of the heart apart from chorea, it appears to me a legitimate reason, until it is shown conclusively that chorea independently of rheumatism causes similar cardiac disease. Thus it is instructive to compare the condition of the heart in the four classes into which I have divided the cases with reference to rheumatism :—

Class I.—47 cases with a distinct history of rheumatism.

With systolic murmur at apex	26
„ systolic at base and apex	1
„ presystolic and systolic at apex	7
„ murmur supposed to be hæmic (excluded)	2
„ heart sounds modified, no distinct murmur	3
„ no murmur	8
	47

With a distinct murmur of some kind 34 or 72·34 per cent.

Class II.—28 cases with a history of pains (probably rheumatic).

With systolic murmur at apex	11
„ presystolic at apex	1
„ murmur supposed to be hæmic (excluded)	1
„ heart sounds modified (no bruit)	8
„ no murmur	7
	28

With a distinct murmur of some kind 12 or 42·85 per cent.

Class III.—23 cases with doubtful history of rheumatism.

With systolic murmur at apex	10
„ presystolic at apex	2
„ murmur supposed to be hæmic (excluded)	1
„ heart sounds modified	1
„ no murmur	9
	23

With a distinct murmur 12 or 52·17 per cent.

CLASS IV.—66 cases with no history of rheumatism.

With systolic murmur at apex	26
„ systolic murmur at base and apex	2
„ systolic murmur at base	1
„ presystolic murmur at apex	2
„ murmur supposed to be hæmic (excluded)	2
„ heart sounds modified	9
„ no murmur	24
	66

With a distinct murmur of some kind 31 or 46·96 per cent.

Comparing these classes we find :—

CLASS I.	47 cases with murmur	in 34	or 72·34	per cent.
„ II.	28 „ „	12 „	42·85	„
„ III.	23 „ „	12 „	52·17	„
„ IV.	66 „ „	31 „	46·96	„

It will thus be seen that whilst the frequency of heart disease was greatest amongst the most distinctly rheumatic, it was very high in the cases in which rheumatism was probable, in which it was doubtful, and in which no history of rheumatism was obtainable.

Again, separating the first attacks of chorea from the rest of the cases, we have :—

CLASS I.	22 cases with murmur	in 16	or 72·72	per cent.
„ II.	23 „ „	11 „	47·82	„
„ III.	14 „ „	6 „	42·85	„
„ IV.	43 „ „	20 „	46·51	„

It is thus shown that in primary attacks of chorea a murmur existed in something approaching 50 per cent. in cases in which a history of rheumatism was doubtful or denied. In *five* only of the cases, all first attacks, did the murmur develop *during* the attack. In none of these five cases was there a history of rheumatism. In all the remaining cases a murmur was present when the patient came under observation, presumably early in the attack ; and as a murmur only developed under observation in such few instances, it was probably in the remainder caused by some antecedent condition, or some influence acting at the commencement of the attack. Chorea, *per se*, has not been shown in these cases to produce a murmur at all frequently. As we know of no influence likely to produce heart disease with anything like the frequency with which it occurred in Classes III. and IV., I think the conclusion is almost irresistible that rheumatism must have obtained in a very considerable proportion of cases in which no history of its occurrence could be elicited. I have inquired into the histories of the cases for other occasional causes of endocarditis, such as scarlet fever, measles, smallpox, anæmia, &c., but taken collectively, they do not equal the frequency of rheumatism as an antecedent, and they are nearly equally distributed amongst the rheumatic and the non-rheumatic. This latter statement is of importance, as it may be presumed that on failure to elicit a history of rheumatism, additional care would be taken to trace other antecedents.

In some of the cases "subcutaneous nodules," as described by Dr. Barlow and Dr. Warner, were present.

The condition of the heart in old and recent cases is the same as that seen in rheumatism old and recent. It is, of course, admitted that endocarditis from various causes assumes the same form.

Finally, I believe that from the analysis of these cases the following conclusions may be drawn :—

1.—That a murmur was present in more than half the cases of chorea.

2.—That the murmur, in all except one case, was due to endocarditis affecting almost exclusively the mitral valve.

3.—That in 60 per cent. for certain (and in over 80 per cent. possibly) the heart lesion persisted.

4.—That absence of murmur is no proof of absence of organic heart disease.

5.—That rheumatism had pre-existed in nearly half the cases, and that there are strong grounds for believing that it had been an antecedent in a very much larger proportion of cases.

6.—That no other very frequent exciting cause of endocarditis is shown to have pre-existed, or to have been more frequent amongst the non-rheumatic than the rheumatic.

7.—That the form of heart disease is that seen in connection with rheumatism.

8.—That rheumatism is in nearly all cases the cause of the heart murmur which so frequently attends chorea.

APPENDIX.

CASES OF CHOREA examined with reference to the condition of the heart at periods of from one to five years subsequent to the attack for which they were in the London Hospital.

No.	Name.	Age.	Condition of Heart on Leaving.	Present Condition of Heart.
	LEFT HOSPITAL IN 1876—(*Five Years*).			
1	M. L. . .	29	Systolic at apex.	Occasional reduplication of first sound, which is rather prolonged at apex; soft blow over pulmonary artery.
2	A. L. . .	12	Mitral diastolic.	No bruit; reduplication of first sound at apex.
3	A. W. . .	15	Mitral systolic.	No bruit; apex beat in 5th space, diffused, moderately strong.
4	F. A. . .	16	Mitral systolic.	First sound murmurish, occasionally developing into bruit; apex in nipple line.
5	G. F. . .	9	Systolic at apex.	Sounds normal, beat normal.
6	K. A. . .	8	Mitral systolic.	Systolic at apex, second sound intensified at base; beat in 4th and 5th spaces nipple line.
	LEFT HOSPITAL IN 1878—(*Three Years*).			
7	S. N. . .	14	Systolic at apex.	Soft systolic at apex, intensified on exertion; apex beat in 4th space and nipple line, slightly heaving.
8	A. W. . .	10	Mitral systolic.	Murmurish first sound at apex, especially on exertion; accentuated second sound.
9	C. W. . .	16	Systolic at apex.	Died of heart disease in 1881. Had subcutaneous nodules.
10	H. B. . .	9	Mitral systolic, which disappeared.	Died of heart disease in 1881.
11	M. A. W. .	10	Systolic and (?) presystolic at apex.	Loud systolic at apex; maximum intensity in 3d space; second sound intensified and occasionally reduplicated.

CASES OF CHOREA—*continued.*

Name.	Age.	Condition of Heart on Leaving.	Present Condition of Heart.
LEFT HOSPITAL IN 1878—(*Three Years*)—*continued.*			
P. K. . .	10	Normal.	First a little prolonged, especially after exertion; no murmur.
E. A. . .	...	Systolic and presystolic.	Dead.
C. T. . .	...	Systolic at apex.	Systolic at apex.
W. B. . .	...	Systolic at apex; transient suspicion of presystolic.	Systolic at apex; reduplicated second sound; apex heaving.
G. K. . .	12	Systolic at apex; dilated heart.	Died of "blood tumour of brain," 1880.
A. L. . .	14	Soft basic systolic.	Excited action of heart, second pulmonary sound accentuated; no bruit.
A. P. . .	9	Presystolic at apex.	Loud blowing systolic at apex, heaving beat.
LEFT HOSPITAL IN 1880—(*One Year*).			
H. M. . .	...	Mitral systolic.	Dead.
C. H. . .	...	Mitral systolic.	Mitral systolic, second sound intensified.
W. W. . .	...	Systolic at apex.	Systolic at apex.
A. P. . .	11	Systolic at apex.	Systolic at apex, heaving beat.
E. S. . .	16	Systolic at apex.	Intensified first sound, heaving beat.
L. B. . .	16	Systolic at apex.	Normal, except for slight accentuation of second sound.
A. S. . .	13	Systolic, (?) presystolic.	Loud systolic at apex, heaving beat.
M. O. . .	21	Systolic at apex.	Blowing systolic at apex.
A. L. . .	16	Systolic at apex.	Loud blowing systolic at apex, heaving beat.
F. G. . .	16	Systolic and presystolic.	Systolic at apex; palpitation.
M. A. J. .	13	Systolic at apex, disappeared.	No bruit; very anæmic.
M. M. . .	15	Systolic at apex.	Blowing systolic; short presystolic.
A. N. . .	9	Systolic at apex.	Soft systolic at apex; beat outside nipple.
A. H. . .	11	Systolic at apex.	Died of heart disease.
C. H. . .	16	Systolic at apex.	Systolic at apex; second sound slapping.

On the Modes of Origin of Chorea in Relation to the Question of its Pathology and Treatment.

Dr. STURGES, London.

During the last five years I have had charge of 177 children and g adults suffering from chorea, and have taken note especially of the wing points:—(1.) *The morbid associations of the disorder.* (2.) *Its e of origin and of progress in the several groups of muscles which it ts.* (3.) *Its cause* (near and remote), and *its final issue* (whether in recovery therwise). The precise conclusions under these several headings must be d numerically,* but as figures and percentages do not convey a very vivid ression, I shall here speak of the results in general.

These 177 examples of chorea are made up of 132 received from Great ond Street into the hospital at Highgate devoted to chronic cases, and

* The cases are detailed *seriatim* in an appendix of my work on "Chorea and Other ment Disorders of Early Life," just published.

forty-five admitted into the Westminster Hospital. The former series consists of children at or under the age of eleven; the latter series includes as many as ten who are over thirteen. In the first series of 132, the girls are to the boys in proportion of three to one. Of the whole 177, something less than a third (fifty-two) have had previous attacks, but only ten of the whole number have had two previous attacks, and only four more than two.

I.—As regards my first point, the *morbid associations of chorea*, it must be remembered that the *brief lives* of these little children afford but small opportunity for judging of nervous temperament or proclivity for particular diseases. But amongst infantile affections, there is one which has at least this alliance with chorea, that it is (in parts at least) a nervous disorder, and that it shows a marked preference for girls—I mean whooping-cough. It is worthy of remark, therefore, that in 120 cases of the Children's Hospital series, where this point was inquired into, only twenty, or one in six, had escaped whooping-cough (viz., sixteen of the ninety-two girls, and four of the twenty-eight boys).

In order to ascertain how this proportion of whooping-cough amongst choreic children might compare with its proportion amongst children in general, I take from the records of Cromwell House, during my connection with it, three independent series, each consisting of 120 patients taken consecutively. The proportion of children who escaped whooping-cough in these three series respectively varies but little. It is very nearly expressed by saying that two in every five have so escaped.* *It would thus appear that whooping-cough has more than double the frequency in choreic children that it has in others, the proportion being as twelve to five.*

II.—The *mode of origin* of chorea may be considered from many different points of view. It has been here observed in reference, first, to those *parts of the body which the disorder selects by preference*, whether as its sole seat, or as the starting-place whence it spreads to other limbs; and, secondly, with reference to the *cause or causes on which the disorder seemed immediately to depend.* The first inquiry—viz., that which concerns the starting-place of chorea—while it is the more manageable and reliable of the two, is also the one which has received least attention.

The mode of onset of chorea, in the sense just mentioned, was made the subject of particular inquiry in 132 of the 177 cases here discussed. In (forty-one) *nearly a third* of these, the muscular disorder was represented as being *general* from the first. Yet with the probability of partial chorea for a while escaping notice, it would be hardly safe to conclude that in so large a proportion of instances it is from the first a general invasion. Greater weight attaches to the remaining examples, where the first onset of the disorder was distinctly observed in a particular place, and the results were, I think, remarkable. In seventeen cases chorea is represented as *right-sided*, the meaning being that the arm and leg of that side were first or more especially affected. In nineteen, or what is practically the same number, it is represented as *left-sided*. Right and left sided chorea, therefore, thus generally described, may be taken as equal. But here equality ends; and I now quote figures as showing the disparity better than proportions. *Chorea, in its commencement, affected the upper parts of the body alone in forty-five cases; it affected the lower part of the body alone in five (or less than a tenth of the former).*

To particularise: Chorea *began in the hands* (one or both) in as many as thirty-seven cases, or more than a third of the entire number in which its starting-place was noticed. In about half of these (eighteen) it began *in both hands;* but when this was not so, it was *right-handed* in fourteen, and *left-handed* in only five. On the contrary, we have the *right* arm, the first to

* *i.e.*, 1 in 2·9 in first series, 1 in 2·9 in second series, 1 in 2·15 in third series.

be unsteady with less than half the frequency (four) of the left (ten). The affection began in *both legs* in but two cases; in the left leg in two; in the right leg in one. It began in the face, according to the observation (which is probably faulty in regard to so mobile a part), nine times.

Thus *the order in which the several muscular groups yield to chorea is the following*:—The hands most, the right hand, the left arm, the face next (with a sudden fall in frequency, but little difference between themselves), the left hand, the arms, the right arm, the legs, the left leg, the right leg, these last appearing so seldom that it is obviously impossible to rate and compare one with the other.

On the whole, and making abundant allowance for error, the muscles which chorea affects, both first and most, are the same that are concerned in those higher uses which are not only the most intricate, but which, apart from civilisation, are never reached at all; while the parts which the disorder affects later and more rarely, concern movements which are so far *natural* and removed from any special process of education that savages perform them as well, and, indeed, far better than civilised men.

And there are other points to be kept in view in reference to the relative stability of different parts of the body, which need no support from statistics, since they are matters of common knowledge. Notice, for example, *the several distinct modes of development of chorea*; the fact that choreic disturbance (often, it is true, not so accounted, because sensitive facial movement is easily excused) will appear in the face and spread no further, whilst, on the contrary, the infrequent chorea which begins in the legs will almost always spread to the upper limbs. And the mode of recession is as remarkable as the onset. A general chorea mainly recovered from elsewhere will sometimes linger in the hand,* and exhibit extreme severity in this one place; but the comparatively rare chorea of the feet and legs is both the last to come and the first to go.

One other matter may be mentioned apart from the direct sanction of the tables. It is that *in the most violent chorea the muscles still maintain their accustomed concert.* The muscular over-movement, except for its exaggeration and disregard of the will, is the same as the natural movement. Putting aside the paresis, which is sometimes conjoined with chorea, it is not true to say (as has been said), that it will select certain muscles out of a group and move them after a manner of which mere will is incapable.

That this conjoint action of the muscles remains intact is shown especially in this: that in those parts of the body where the several muscular uses are most distinctly differentiated, there, in exact proportion to the degree and perfection of such differentiations, do we find a more or less perfect separateness of choreic movement. Thus, for example, the hand and the fingers are concerned in many tasks in which the arm takes no direct share, and the hand and fingers are often choreic without the arm. On the other hand, the simple movements of the foot are commonly those which it shares with the leg. Foot movements alone are as rare as foot chorea alone. It is the same in regard to the two sides of the body. Movement of one arm is common enough without need or suggestion of movement in the other, but the movement of one leg almost implies movement of the other. Similarly, chorea of a single arm is very common, and chorea of a single leg is very rare. But most of all do we find this rule exhibited in the face. One-sided chorea of the face is the exception; one-sided paralysis is the rule.

To return to the tables. I have taken account of *rheumatism* in regard only to the 132 cases furnished by the Hospital for Children; the others may

* In this, as in some other features, chorea is not only an affection commonest in childhood; it is *peculiar* to that time of life.

be supposed to derive a bias from the preconceptions of the narrator. These 132, let it be remembered, came to me with their histories already taken, and I have only put down what was told me by a succession of observers, the several medical registrars, that is, acting in conjunction with the three physicians of the Hospital. The rheumatic account thus derived is as follows:—In the whole 132, six were doubtful, and in five the particular fact was not ascertained; fourteen or fifteen had had pains, probably rheumatic; seven had had rheumatic fever. Or, stating the facts in somewhat different form and less open to error, there are 97 cases out of the 132, or, excluding eleven where the point was not ascertained, there are 97 *out of* 121, *free from any rheumatic association.* In other words, *chorea has nothing to do with rheumatism in three-fourths of the cases.* And when we come to inquire what is the *nature* of the rheumatic association, the *degree* of its being what we see, it appears that in six out of the 132 (two of these six being cases of *acute* rheumatism) the chorea arises directly out of the rheumatism in such manner as to make the one seem the direct *cause* of the other.

I know that these figures are very much out of accordance with those of others, and that it is said by many that we are not sufficiently alive to the various forms of child's rheumatism, and especially to the transient character of the joint symptoms, and the frequent implication of the heart in the rheumatism of children. In reply to such contention I would only say this—First, that it must be a matter of extreme difficulty and doubt to accept, on the evidence of mothers and nurses, such indefinite signs as these of bygone rheumatism; and, secondly, that if a latent form of rheumatism is to be reckoned in this way, we are in fact maintaining an old hypothesis upon a new basis; for the old doctrine of the rheumatic origin of chorea asserted plainly an alliance between it and the ordinary power of articular rheumatism. However the truth may be, it is at least certain that *no one in this country has succeeded in finding any large proportion of cases immediately connected with rheumatism*, while as to those to whom a rheumatic history is assigned, it is obvious that *until we agree upon the particular signs and symptoms which are to be accepted as valid evidences of rheumatism, we have no common factors to deal with, and may expect the remarkable discrepancies in result which actually appear.*

My own belief, derived not from these tables alone, but from ampler grounds, to which I have referred elsewhere, is that *acute articular rheumatism*, although rare in the history of chorea, occurs *in such association with it as to justify the assumption of some direct relationship* existing in a very small proportion of examples; while, on the other hand, *chronic rheumatism* is so difficult of identification, and so easily confused with the joint and limb pain which properly belongs to chorea, that (although on this very account it is easy enough to impute it) *no such connection can be asserted on behalf of rheumatism generally.*

Very different must be the statement in regard to the *mental or psychical causes of chorea.* In the first series of 132 cases, the *immediate* cause was thought to be ascertained in 112, and in 97 (or 93) of these, say *in about half*, that cause was some mental shock or strain. In the second series of 45 cases there were as many as 28 (not to speak of those where the cause was unknown), *say two-thirds, where the cause was either fright or something allied to fright.* It is only by a perusal of the tables that the very real nature of this, the great, at all events the great *exciting*, cause of chorea can be adequately appreciated. "Pursued by a drunken man," "Sent for a policeman to quell a family disturbance," "Frightened by drunken father," "Locked up in a dark cellar"—is there anything in any suggested pathology of chorea which will

explain half so well as such an enumeration as this the undoubted fact that chorea is a disease of poor, ill-cared for children, and especially of timid girls?

I do not suppose that the immediate cause of chorea is *always* a mental one. Yet, supposing that were so, we should hardly expect, I think, to find it actually appearing in our records in much larger proportion than it does. There are other causes of mental distress, both for children and for men and women, besides those which openly appear—terrifying dreams, night terrors, sights in the street, incidents of school life, unequal competition with stronger and cleverer companions. Chorea is not more often to all appearances spontaneous than is the sudden and causeless change of disposition which will affect young children, and especially girls, at an age too early to be attributed to sexual development, disappearing in the same unaccountable way that it came. Considering, I say, that for half, or more than half, the instances of chorea that come before us we have a distinct cause in some great terror or such like, it would not be asking too much if we said of the rest that the many hidden, undiscovered causes of mental distress which lie in wait for little children, and especially *poor* little children, sufficiently account for them.

How are *the heart symptoms of chorea* to be reconciled with such a view of its causation as this? In 132 choreic children of the first series there was heart defect in one-third (45); in 45 children and young adults of the second series there was heart defect in one-third (16); "heart defect" being understood to mean either a mitral systolic murmur or an uneven action. But the observations hitherto made upon this point need, as I believe, to be supplemented by others having reference not merely to the cardiac sounds, but to the *rate* and *variability* of the cardiac action. Fully admitting the obscurity of this part of the subject, I would venture to repeat here the substance of some remarks I have sought elsewhere to justify.* The heart, I would submit, suffers in chorea (apart from the rheumatic association altogether), because chorea affects the same parts and organs which ordinary emotion affects; because, to use bolder language, it is an affection of the emotional centres. The commonest heart symptom in chorea, as in emotion, is increased frequency, but the most obvious and most observed symptom is systolic murmur, due to blood reflux through the auriculo-ventricular orifices, the consequence of fatigue *paresis* on the part of the papillary muscles.

I come to speak, in the last place, of *the final result*. Here are nearly 200 examples of chorea of varying severity, taken without selection from public practice during a period of five years. Excluding some few of the elder ones, and especially youths between thirteen and sixteen, in whom the disorder is very intractable, *they all recovered*. It cannot be said that they recovered altogether without drugs, because a large proportion of them, variously treated at an early stage of their disorder, were on their way to recovery before they came under my care; but it *may* be said of the cases which came to the chronic wards, and of the Westminster cases from first to last, that no medicinal agents had any share in their treatment.

Chorea in children is a self-recovering affection, and if it be urged that the question still remains as to the means by which it may be cured the quickest, I would venture to answer that we have yet to learn in what sense the term "cure" is applicable to an affection which no one professes to be able to alleviate immediately and out of hand, and which is of such uncertain duration that it is impossible to say what is a long and what a short term for it. Putting aside, indeed, extreme chorea, or about one case in a hundred, it seems of small importance whether the limbs of a young child return to such control as they are likely to endure this week or next. Any manifestation of anxiety for a

* "The Heart Symptoms of Chorea," *Brain*, July, 1881.

[illegible] effect it produces [illegible].

[illegible] of chorea—of the [illegible]—we must be careful of [illegible] consider the violent and sometimes [illegible] as is well known, very soon [illegible] the mental disturbance [illegible]—these are symptoms which so [illegible] themselves too often the cause [illegible] signs of sinking, the patient [illegible]. It is true that [illegible] is an embolic [illegible] soon to develop [illegible] inevitable death—not [illegible]. Yet we are to remember that [illegible] shows its alliance with [illegible] symptoms as formidable as the [illegible].

[illegible] to be legitimate inferences [illegible]:—

1.—[illegible] the same which are devoted to [illegible] subjects of the [illegible].

2.—[illegible] those which depend upon a [illegible] purposeful and [illegible].

3.—[illegible] immediate causes of chorea [illegible].

4.—[illegible] independent, that is, of any [illegible] that the heart is concerned [illegible] the pattern of emotion.

5.—[illegible] several times in the same [illegible] without the employment of [illegible] relates to the [illegible] it has been first [illegible] an affection of very [illegible].

6.—[illegible] its subjects, mode [illegible] spontaneous recovery—are all [illegible] as a functional disorder.

[illegible] *especial* [illegible] *Murmur which so* [illegible]

[illegible]

Although chorea has been a well-recognised affection since the days of Sydenham, and although in recent years, owing to the very great interest taken in the investigation of diseases of the nervous system, the attention of physicians has been directed to it with renewed vigour, still there is at the present time not only no unanimity, but the greatest diversity of opinion as to its nature.

In regard to chorea there are many points well worthy of study, but the two features of it which stand out prominently, and which the Committee of this Section have chosen for discussion to-day, are :—

1.—Its relationship to rheumatism.

2.—The nature of the heart murmur which so frequently attends it.

Having had an opportunity of observing a number of cases of chorea during the past two years at the Belfast Hospital for Sick Children, I have embodied their clinical features, especially as they relate to the question of rheumatism and the nature of the heart murmur, in the following table :—

Analysis of Cases of Chorea from the Belfast Hospital for Sick Children.

No.	Name.	Age.	No. of Attacks.	Rheumatism.	Heart.	General Remarks.
1	Maggie M. .	11	1	Acute rheumatism two weeks previous to admission into hospital. Three days after the rheumatism commenced chorea set in.	Soft systolic apical murmur during the attack, which gradually ceased to be heard.	Case of right hemichorea commencing during the course of an attack of acute rheumatism. Slight anæsthesia and muscular weakness of right side. The choreic movements specially severe in face and arm. During recovery the murmur passed away. Child seen again four months after attack and no murmur heard.
2	Ellen M. .	9	1	Sister of No. 1; admitted on the day that her sister was discharged, suffering from acute rheumatism.	On admission systolic murmur at apex.	Acute rheumatism commenced four days before admission to hospital. On the seventh day after admission the chorea began. Both sides affected, but especially the right. Removed home by friends on the eleventh day (in hospital) and died three days afterwards. No. p.m. A cousin of this child's mother had an attack of severe general chorea after a fright.
3	Agnes C. .	14	2 First attack followed a fright when she was nine years of age. No cause of second attack assigned.	Subacute rheumatism during decline of second attack. No history of rheumatism with first attack.	Mitral systolic murmur during second attack. Condition of heart during the first attack not known.	Was in hospital during second attack, which was general. As she was getting better, pains came on in the joints accompanied by extreme anæmia, œdema, and purpuric spots (Purpura rheumatica ?). She was seen two years after this, when mitral murmur was still heard.
4	Margt. J. .	12	2 Rheumatism and fright before the first attack. Cause of the second not known.	Rheumatism before the first attack.	Apical systolic murmur heard four years after the first attack. Condition of heart during the attack not known.	Child brought to out-patients' room complaining of pains in joints (subacute rheumatism). Mitral systolic murmur heard. Mother stated that the child had had chorea four years before, which was preceded by rheumatism, but which set in immediately after a fright (chased by a dog). There was another attack a year after first, but no cause could be given.

Cases of Chorea—*continued.*

No.	Name.	Age.	No. of attacks.	Rheumatism.	Heart.	General Remarks.
5	Mary T. . .	11	1	Rheumatic pains came on a fortnight after scarlatina, followed by chorea.	Very faint systolic murmur occasionally present at apex.	Choreic movements se severe and confined right side. On dischar hospital heart normal.
6	Maggie E. .	21	1	Rheumatic pains before the attack.	Double murmur at apex: a soft systolic heard to left of apex and also in axilla: a rough pre-systolic heard in right of apex, and accompanied with a thrill.	Case of right hemi Both murmurs persis recovery from the chor
7	Thomas J. S.	13	1	Rheumatism two years before the attack of chorea.	Condition of heart during attack not known.	Was brought to out-p room suffering from bro (secondary to heart affe Mother stated that he h a severe attack of chor years before. When s had a rough systolic murmur at the base, an systolic murmur at the ap
8	Catherine E.	12	2 First attack came on suddenly after a fright. Second attack, three years afterwards, came on gradually. Attributed to over-study.	No history of rheumatism.	Action of heart irregular: no murmur.	Severe case of general c the movements, however marked on left side. emotional excitement. T attack (second) came on ally.
9	Sarah J. D. .	8	1	None.	Soft mitral systolic murmur.	Attack commenced al after a fright (chased cows. Movements b but commenced in righ and were always more in it. When she left h the murmur still presen
10	Ellen O. . .	10	1	None.	Nothing abnormal detected.	Attack came on gradually and arms principally af Child very emotional causes assigned for the c
11	Sarah S. . .	7	1	None.	Irregular action of heart; no murmur.	No history of rheumati fright. Attack sudden onset. Movements v and very severe.
12	Annie E. S. .	6	1	None.	Nothing abnormal detected.	Very protracted and seve of general chorea, whic on suddenly, but no could be given. No h of fright or of rheumati

Of the twelve cases of chorea, the details of which are given in the annexed table, eleven were girls and one a boy. Cases 1 and 2 were sisters, and a cousin of their mother is also stated to have had chorea.

In five of these cases there was no history of rheumatism, while in the remaining seven there was. In five of these seven the chorea was preceded immediately by the rheumatism; in one case the rheumatism occurred two years previous to, while in another it set in during the decline of, the chorea.

With reference to the condition of the heart, it was noted that during the choreic attack there were cardiac murmurs in five cases out of the twelve; in two others the action of the heart was irregular. In other two the state of this organ during the attack is not known, but there were valvular murmurs in one (Case 4) four years afterwards, and in another (Case 7) five years afterwards. So of the twelve cases, in three only did the evidence afforded by physical examination warrant the statement that the condition of the heart was normal.

In five of the seven cases with cardiac murmurs the murmur was single, apical, and systolic; in the remaining two there was also in each a mitral systolic murmur; but in one (Case 7) this was accompanied with a rough systolic murmur at the base, and in the other (Case 6) with a mitral obstruction with presystolic thrill.

Of these seven cases with heart murmur there was in six a rheumatic history; in the remaining one this was absent, the chorea having come on after a fright. In one of the six cases complicated with rheumatism there was also a history of fright.

In four cases the murmur persisted on recovery from the chorea; in three of these there was a history of rheumatism; in the fourth there was no history of rheumatism, but of fright.

The evidence afforded by these cases warrants the conclusion that a mitral systolic murmur is of frequent occurrence in chorea, and that in many cases there is a close relationship between chorea and rheumatism.

How then is this heart murmur to be explained? I think it will be generally admitted that in the great majority of cases this murmur is caused by some organic change affecting the mitral valve: this seems to be rendered tolerably certain by the following facts:—

1.—The clinical fact that the murmur which was present during the chorea frequently persists after the attack has passed away.

2.—The evidence afforded by post-mortem examination. Raymond in his article (Danse de Saint Guy) in the "Dictionnaire Encyclopédique des Sciences Médicales" has given a table of seventy-nine fatal cases of chorea in which a post-mortem examination was made. All these cases, which he has brought together (in his table) from various sources, were observed between the years 1850 and 1878, and the autopsies made in them show that, as far as the heart is concerned, the predominating lesion is valvular endocarditis.

Again, Dr. Wilks, one of our ablest English pathologists, states that he has never seen a fatal case of chorea in which there was not some evidence of previous endocarditis, "the inner surface of the mitral valve being lined by a narrow row of bead-like vegetations."

Admitting, then, that in many cases the murmur is organic, what is the cause of the endocarditis which gives rise to it? Rheumatism will account for a number of these cases, as it is a well-known fact that in the rheumatism of children, even in cases where few joints are affected, cardiac complications are frequent; but it will not explain cases such as Case 9, where there has been no history of rheumatism, but where the attack took its origin in fright. In this case the murmur remained after recovery, and there seems to be no other explanation of its occurrence than that it was caused by the chorea. But even in some

cases in which no murmur has been heard during life, post-mortem examination has shown a morbid condition of the mitral valve. Thus Wilks gives the history of two fatal cases of chorea, both following a fright. In neither was a cardiac bruit heard during life, in neither was there any history of rheumatism, and yet in both there were found vegetations on the mitral valve.

But there still remain a number of cases in which the murmur, although present for a time, eventually passes away.

In explanation of this evanescent murmur, the following views may be stated:—

1.—It is held by some that the murmur is due to spasmodic contraction of the papillary muscles, producing a temporary insufficiency of the mitral valve. But it may be objected to this view that there is no good proof that the involuntary muscular organs participate in these choreic disorders; and further, if this theory were correct, the murmur should vary from time to time in its character, whereas we know it will remain often for a lengthened period without any change.

2.—Even in these cases many think that there is some lesion of the valve, and that the explanation of the cessation of the murmur is that the recent lymph is either washed away or absorbed.

3.—It is well known to careful clinical observers that murmurs heard at the mitral area do not always indicate that there is any organic change in the valve. Thus in severe cases of anæmia, a mitral systolic murmur may sometimes be detected, which, with the restoration to health of the patient, gradually disappears. The same phenomenon is occasionally met with in enteric fever; and Hayden has observed it in purpura, masturbation, and in excessive tobacco-smoking. Bristowe has given a series of cases, and my former teacher, Dr. Cuming, has described two very interesting cases, in both of which there was a mitral systolic murmur. In one case the murmur ceased under tonic treatment, but in the other, in which there was anasarca and congestion of the lungs, the patient died; but post-mortem examination showed nothing wrong with the mitral valve. In all such cases there is regurgitation at the mitral valve; and while it is difficult to give an explanation that will apply to all the different cases in which this phenomenon has been met with, it is probable that in the majority, if not in all, there is a weakened condition of the cardiac muscle, or of the *musculi papillares*, which, by preventing the normal closure of the valve, causes regurgitation, and thus gives rise to the murmur. With the gradual return to health the weakened muscle is restored to its normal condition, the valve again acts as in health, and the murmur disappears.

The same explanation has been applied to the evanescent murmur of chorea?

We conclude, then, that the cardiac murmur met with in chorea, while in certain exceptional cases it may be functional, is in the great majority of cases, whether there has been a history of rheumatism or not, due to some lesion of the mitral valve.

There still remains the question of the relationship of rheumatism to chorea.

In cases in which the rheumatism precedes the chorea, and in which endocarditis is present as shown by the cardiac murmur, the theory that the endocarditis is the connecting link between the two (the chorea being caused by capillary embolism) is certainly a very attractive one.

Cases 1 and 2 support this view. Thus the child, Case 2, was lying in bed suffering from acute rheumatism, and with a mitral systolic murmur present, when suddenly without any other exciting cause violent chorea set in. Here all the factors that produce capillary embolism were present.

This embolic theory is also supported by cases of hemichorea, as, *i.e.*, Case 1,

where there was also anæsthesia and weakness on the same side as that on which the movements were present. Cases of hemichorea have also been known to pass on to hemiplegia. Further, Hughlings Jackson says that embolism is the very process which could cause that unstable or excitable condition of the nervous system present in the disease.

The very great frequency of endocarditis, whether there has been a history of rheumatism or not, lends additional weight to this embolic theory.

Although Tuckwell, Broadbent, and Jackson have found emboli in fatal cases of chorea, still the cases in which emboli have been met with are few; and on the other hand, Dickinson, Wilks, and Moxon, and others have failed to find any microscopic emboli in the numerous cases which they have examined. It is difficult, moreover, to understand why chorea is not more common in adults, in whom embolism frequently occurs.

But even granting that this theory may be proved to be true, it will not explain the relationship of the rheumatism to the chorea when the former succeeds the latter (as in Case 3), nor cases where the chorea has followed the rheumatism, but in which no cardiac bruit has been detected. Further, there are still a large number of cases with no history of rheumatism at all. What we want, then, is some theory that will account for all cases of chorea, as well those in which there is a rheumatic history as those in which this is absent, and which will also explain the frequent presence of the cardiac complications.

Impressed with the necessity of giving an explanation that will cover all cases of chorea, some look upon a morbid condition of the blood as the cause, but it is more probable that the nervous system is the part primarily at fault, a view which receives additional weight from the fact that there is considerable evidence to show that rheumatism itself is a disease of the nervous system, and in this way can be explained the frequent association of rheumatism with chorea.

From the foregoing I would draw the following conclusions:—

1st.—That in those cases of chorea in which there is a cardiac murmur, this murmur is generally organic.

2d.—That the murmur is usually heard at the mitral area, and is systolic.

3d.—That in some cases, however, this murmur may be inorganic.

4th.—That in those cases in which the murmur persists after recovery from the chorea, there has often been a rheumatic history.

5th.—While in a large number of cases a close connection exists between rheumatism and chorea, and while there is considerable evidence to support the view that rheumatic endocarditis is the connecting link between the two (the chorea being due to emboli), that this embolic theory will not explain all the cases.

6th.—That there are cases of chorea without any cardiac bruit and without any history of rheumatism.

7th.—That in many fatal cases of chorea, some in which there had, and others in which there had not, been a history of rheumatism, and some in which there had, and others in which there had not, been a cardiac bruit, post-mortem examination has demonstrated a morbid condition of the mitral valve.

On Subcutaneous Nodules connected with Fibrous Structures, Occurring in Children the Subjects of Rheumatism and Chorea.

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In the study of rheumatism and chorea we have observed during the last six years a number of cases in which subcutaneous nodules have occurred in different parts of the body. The subjects of our observations have been either children or young people, not more than eighteen years of age; but we find that similar lesions have been noted (although rarely) in persons of middle life.

These nodules being often inconspicuous, may be overlooked alike by the patient, the friends, and the doctor, and as they are mostly painless, and spontaneously subside, they are of little therapeutic import. But we venture to think that their natural history is of interest (1.) as showing their conformity to the general type of rheumatic inflammation, and (2.) as helping to establish the existence of the rheumatic diathesis in a given case where, as is so often the case, in childhood the evidence of well-marked arthritis may be wanting, and (3.) as helping to establish in some cases of chorea the relationship with rheumatism, obscure and empirical though that relationship may be.

We propose to give (1.) a summary of the facts which we have observed in regard to these nodules; (2.) a tabular account of 27 of the cases which we have separately or conjointly investigated; (3.) an abstract of the observations of French and German authors on the subject.

1.—The nodules in these cases varied in size from that of a pin's head to that of an almond. They were strictly subcutaneous; the skin over them being simply raised without infiltration. In no case was there any local heat or spontaneous pain.

In one case (XVI.) the child had cried out on account of pains referred to joints in the neighbourhood of which nodules existed; but pressure on the nodules elicited no complaint.

In another case (VII.) nodules situated under the scalp were slightly tender on pressure, and combing the hair gave rise to distress. With this exception there was no tenderness.

In all but two (XVI. and XVIII.) there was no redness of skin over the nodules, and in these it seemed possible that local pressure or friction had induced the redness.

The nodules were separable from the skin, and to a varying amount movable on the subjacent structures.

The back of the elbow, the malleoli, and the margins of the patella were the commonest sites. Other situations were the neighbourhood of the vertebral spines, the spine of the scapula, the crista ilii, the extensor tendons of the foot and hand, the temporal ridge, the forehead, the superior curved line of the occiput, the pinna of the ear.

They were often symmetrical.

They occurred discrete or in clusters.

In total number present in a single patient they varied from one (IV.) to fifty (XVIII.)

The small nodules were often more palpable than visible.

To demonstrate their existence it was often necessary to stretch the skin over them; those in the neighbourhood of the knee and elbow joints were best brought into relief by flexing the said joints.

In regard to evolution, the following varieties were noted :—

1.—Nodules appeared in one crop, *i.e.*, several nodules appeared simultaneously, in different parts of the body.

2.—Successive nodules appeared.

3.—Individual nodules when first noted had attained their maximum ; they remained stationary for a time and then gradually subsided.

4.—Individual nodules greatly increased in size under observation (III., XV.)

5.—Duration of the nodules varied considerably ; the shortest period noted was three days from eruption to disappearance (XII.) The longest period that they were noticed to persist without diminution in size was five months (I.) In another case (XIV.) some persisted for five months ; others had diminished in size, and others had disappeared. The duration varied between these limits.

6.—Nodules partially subsided and then underwent recrudescence (I.)

7.—After complete subsidence, so far as manipulation could determine, a new crop appeared (VIII.)

8.—A nodule which, so far as palpation could determine, had subsided entirely, was nevertheless found post-mortem still to exist though very small (XVII.)

9.—In many cases the temperature was normal when the eruption of nodules occurred.

10.—In some cases there was pyrexia, but there were other conditions present, such as pleurisy, pericarditis, &c., which might possibly have determined the pyrexia.

11.—(α) In no case were the nodules found to become bony, or (β) infiltrated with urate of soda ; and (γ) in no case did suppuration occur.

By the first of these characters the nodules were distinguished from the "nodi digitorum" of Heberden, which according to Charcot are always bony (*vide* "Maladies des Vieillards").

By the second character they were distinguished from gouty nodules ; by the third from scrofulides. From erythema nodosum they were distinguished by the absence of skin implication, and from syphilomata by there being no infiltration of surrounding structures.

We had the opportunity of making post-mortem examinations on three cases (V., VII., XVII.)

In regard to anatomical site, we found that they were attached to tendons, to the deep fasciæ, and in one case (XVII.) to the pericranium.

The cellular tissue was not involved. In Case XVII. they were numerous, and each about the size of a pin's head.

Corresponding with many of the nodules, there was a minute pit in the subjacent bone, apparently due to pressure. In no other case was there any evidence of the bone being implicated. The nodules found over one of the vertebral spines and near the knee and elbow joints had no relation to the periosteum, but entirely belonged either to the deep fascia or to tendinous expansions. They were round or oval, semi-transparent fibrous bodies, in appearance somewhat like boiled sago grains.

A nodule was removed during life by Mr. R. W. Parker from over one of the dorsal spines in Case XVIII., and Mr. Parker kindly made some microscopic sections of it. It was also attached to the deep fascia. On section it presented wavy strands of fibrous tissue, with caudate, spindle-shaped, nucleated cells and abundant vessels. It had, indeed, many of the characters of organising granulation tissue.

We shall now consider some of the clinical features of the subjects of these nodules. They were either children or adolescents, the limits of age being four and a half years and eighteen years. Seventeen were female, ten were male.

With regard to evidence of *rheumatism*, in two cases there was no history

of joint affection to be obtained. One of the two had suffered from a fever lasting for three weeks, but no other information was forthcoming. Of the remaining twenty-five :—

Eleven had under observation either unquestionable rheumatic fever, or at least arthritis with effusion in one or more joints.

In eight others there was a history of rheumatic fever, or of some joint trouble in which there was swelling present.

In six there was arthralgia (without swelling).

Other evidences of rheumatism were stiff neck (I.), thickenings along tendons, with contraction (XVIII.), acrid sweating (XXII.), and an attack of hyperpyrexia, which subsided within a fortnight, and during which a crop of miliaria rubra appeared (V.)

In a large number of the cases the skin was dry and harsh.

We believe that as a general rule it may be stated that in the rheumatism of childhood the skin is much less moist than in that of adults.

We adhere strongly to the view which has been held, that some forms of erythema are rheumatic manifestations. We think it important, therefore, to note that seven of our patients had erythema marginatum and erythema papulatum whilst under observation at or near the time of eruption of the nodules, and that another at a subsequent period developed this rash.

One had urticaria and another purpura.

With regard to the heart, we believe that in all these cases there was some morbid condition.

In one the only physical sign was an altered ("murmurish") first sound. This was the only case about which there could be any doubt, in all the rest the evidence of disease was abundant and conclusive.

In eight cases pericarditis developed under observation, and in two, in addition to extensive valvular disease, it was believed that there was adherent pericardium. In twelve the cardiac disease was progressive—that is to say, the valvular murmurs altered, and the dilatation increased under observation, in spite of the employment of digitalis and rest. Severe dyspnœa was a common symptom. Eight out of the twenty-seven patients died from the result of heart disease. Autopsies were obtained on five. In all there was mitral disease with dilatation ; in four there was disease of mitral and tricuspid valves ; in three, of mitral, tricuspid, and aortic valves ; in four, pericarditis.

With regard to chorea, ten of our patients were the subjects of this neurosis whilst under our observation, and another had suffered from it after a previous attack of rheumatic fever.

We submit in conclusion :—

1.—That subcutaneous nodules having such a life history as we have described truly conform to the general type of rheumatic inflammation, especially in their spontaneous tendency to subsidence more or less complete, and in their proneness to relapse.

2.—That they may be considered as in themselves indicative of rheumatism, even in the absence of pain.

3.—That when found associated with chorea and heart disease, although no history of antecedent rheumatic fever can be obtained, nevertheless, their presence gives a presumption that the chorea is rheumatic.

4.—That in regard to prognosis and treatment, although the nodules are unimportant in themselves, they are nevertheless of serious import, because in several cases the associated heart disease has been found actively progressive.

5.—That such nodules belong strictly to the fibrous tissues, and in nature are probably homologous with the inflammatory exudation which forms the basis of a vegetation on a cardiac valve.

Name and Age.	Nodules.	Skin.	Temperature.	Heart.	Chorea.	Evidence as to Rheumatism.
CASE I. Percy S. 8½ years. July, 1875. Fatal.	On extensor tendons of first and second fingers, both sides. These were left after first attack of rheumatic fever. They partially subsided. Patient was seen again sixteen months afterwards. At this period (three months after a second attack of rheumatic fever) he had nodules on tendons of extensor indicis and of palmaris longus of both sides. These diminished, but did not disappear. One month afterwards a nodule appeared in front of left knee, and four months afterwards many fresh nodules about the metacarpo-phalangeal joints.	Frequently moist. No erythema observed. After one of the febrile attacks he had some desquamation.	Elevated during the attacks of rheumatic fever; but on many occasions when the nodules were present there was no pyrexia.	Hypertrophy and dilatation. Mitral regurgitation(?). Adherent pericardium. Death from cardiac dropsy.	None.	History of two febrile attacks, which lasted about three weeks. There was pain in the joints and sweating. After the first attack there was a stiff neck, and the first crop of nodules appeared. After the second attack the knuckles were a little swollen, and the second crop of nodules appeared. The knuckles gradually enlarged a little.
CASE II. Mary C. 8 years. Aug. 1877.	Movable nodules over vertebral spines of middle dorsal region; over olecranon and metacarpo-phalangeal joints of ring and little fingers of both sides. Over both knees.	No eruption. Skin dry.	101° F.; but observe there were some signs pointing to tubercular disease of right apex, which might have accounted for the pyrexia.	Mitral regurgitation.	None.	No history of arthritis.
CASE III. Charles C. 15 years. Feb. 1878.	Right elbow:—One above external condyle, one on external condyle, one below external condyle, five around olecranon. Right hand:—Three connected with extensor tendons on back of hand; one on each knuckle. Left upper limb—nodules as on right. Lower limbs—nil. Three days after first eruption, several small nodules appeared over the styloid process of the left ulna. Also some on the dorsum of each foot. Eighteen days after first eruption a group of three nodules, each the size of a hempseed, appeared on the posterior surface of the pinna of the right ear. Twenty-three days after first eruption there were no fresh nodules, but several had increased in size. One hundred and nine days after first eruption the nodules had entirely disappeared.	Had two attacks of erythema marginatum whilst under observation.	Pyrexia during one attack of erythema marginatum, 100°—101°. Probably no pyrexia at efflorescence of nodules; certainly none during considerable time when they were under observation.	Mitral and aortic regurgitation. Dilatation. Probably adherent pericardium. Much cardiac dyspœna. Bruits altered. Was readmitted on account of severe cardiac dropsy.	None.	Stated to have had rheumatic fever two years ago, and to have been laid up five months with it. Since then breath short, and has frequently suffered with painful swellings of ankles, knees, and knuckles. Admitted with slight swelling around both knee-joints; and eight days after had some swelling of the ball of left great-toe. This was followed by an attack of erythema marginatum, and seven weeks afterwards by the eruption of nodules.

Case V. Emma [illegible] 6 years. Dec. 1878 Fatal.	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Case VI. Kate B. H. 6 years. Dec. 1878 Fatal.	Over external condyle of humerus on both sides, over right external malleolus, three of which one is attached to the tendon of the peroneus longus, and two far from it; one over right internal malleolus; one connected with tendon of right extensor proprius hallucis; two over left external malleolus connected with the peroneus longus. These nodules had not been noticed by the parents, so that the period of their onset cannot be fixed. During the three months that the child was under observation they diminished in size. No fresh ones appeared.	[illegible] day	[illegible]	Mitral obstruction and regurgitation. Pericardial adhesions. Double cardiac murmur under observation. A high pitched murmur at aortic development, which subsequently disappeared. Extensive dyspnœa. Died with fresh access of pericarditis. Post-mortem refused.	Moderately severe at first but rapidly subsided after admission.	[illegible]

Martin D. 8 yrs. 11 mos. April, 1879. Fatal.	One over each superior curved line of the occiput (these were tender on pressure: the boy cried when his hair was combed); one over left external condyle of the humerus; two over left olecranon; one attached to the extensor tendon of the right middle finger over the metacarpo-phalangeal joint; one over the upper portion of the external condyle of each femur; some thickening of the right tendo-achillis and over the left external and internal malleoli. The nodules above his knee-joints had been noticed by the boy's mother for one month before admission. During the eighty-seven days that he was under observation no fresh nodules appeared. Several subsided, but many remained. Post-mortem: the nodules about the elbows and knees were found attached to tendinous expansions; they were semi-transparent fibrous bodies, in appearance somewhat like boiled sago grains, but less defined.	No erythema. Skin generally dry.	For three weeks normal. Subsequently, normal in the morning; not more than 100·6° in the evening.	Presystolic and systolic murmurs. Rapidly progressive dilatation. Post-mortem: — Slight recent pericardial effusion, with thin shreds of lymph. Great dilatation of all the cavities. Vegetations on all the valves, except those of the pulmonary artery. Large nutmeg liver. Congested kidneys. Slight serous effusion right pleura. Small caseous mass in middle of upper lobe of right lung. An adherent thrombus in the left anterior cerebral artery. A great number of small cortical extravasations on the surface of the brain, behind the fissure of Silvius and over the tempero - sphenoidal lobes, (?) the result of capillary embolisms.	Had existed for eighteen days before admission; not severe, bilateral. Entirely disappeared in nine weeks after admission.	History of pains in knees for one month before admission. No effusion when admitted.
CASE VIII. Emma D. 12 years. July, 1879.	Over upper part of both patellæ. One over the right internal malleolus; two over the right external malleolus, of which one is attached to the tendon of the peroneus longus; one over the left external malleolus; one over the left olecranon. These nodules appeared twenty days after admission, and after the subsidence of the rheumatic symptoms, with the exception of slight effusion in the sheaths of tendons on the back of the left hand and a soft cardiac murmur. Twenty-one days after their eruption all had subsided except one small nodule over the right internal malleolus.	Erythema papulatum and erythema marginatum on admission. This continued for ten days. No copious sweating at any time, but subsequently skin a little moist.	Marked hectic for the first twenty-one days —99° morning, 101°–102° evening. The day when the nodules appeared there was a morning temperature of 99° and evening temperature of 101·6°. During week afterwards, 99° morning, 100° evening.	Pericarditis, systolic murmur heard at both base and apex, which afterwards cleared up.	None.	Said to have had rheumatic fever four times; the last attack was four months ago, for which she was in hospital two months. She had pyrexia, slight sweating, effusion in knee-joints, one shoulder, and in sheaths of tendons on back of each hand and about the ankles. She had also two nodules over the right elbow and two on the back of one hand. In the present attack there were effusions in the sheaths of the tendons on the back of each hand and around the ankles.
CASE IX. Alice H. 12 years. Aug. 1879.	One over spine of right scapula; one over olecranon.	No erythema.	Not taken. Skin cool.	Mitral regurgitation.	None.	History of pains in limbs for six weeks, and of swelling in ankles and wrists. Child poorly, and off her appetite. Tongue furred. Has pains in ankles now, but no swelling.

CASES OF SUBCUTANEOUS NODULES—*continued.*

Name and Age.	Nodules.	Skin.	Temperature.	Heart.	Chorea.	Evidence as to Rheumatism.
CASE X. Mary G. 5 years. Aug. 1879.	Two over right olecranon.	No erythema.	Not taken. Skin cool.	Mitral regurgitation. Hypertrophy.	None.	Brought for debility. Mother gives a history of the child having had a fever one year ago, which lasted for three weeks. No history of joint affection. There is no arthritis at present.
CASE XI. Arthur E. 7 years 3 months. Aug. 1879.	One over each olecranon; one over each patella. Date of onset not known. Subsided within two months after the boy was admitted into the hospital.	Dry. One year and ten months afterwards was readmitted to hospital with extensive erythema marginatum.	Generally normal.	Mitral obstruction and regurgitation. Dilatation. Probable adherent pericardium. Much dyspnœa. Brought to hospital on that account.	None.	History of a bad attack of rheumatic fever eight months previously, which attack kept him in bed three months, and left him short-breathed and exceedingly pale. No history of arthritis in present attack. *Postscript.* — One year and ten months subsequently was readmitted with erythema marginatum, but without nodules. Some effusion in one knee-joint, and some fresh percarditis developed whilst under observation.
CASE XII. Mary A. W. 10 years. Nov. 1879.	One on tendon of right tibialis anticus above internal malleolus. This was first noticed fourteen days after admission. It disappeared in three days.	Dry. No erythema.	Normal.	Mitral regurgitation.	Bilateral. Not severe. Appeared five weeks after onset of rheumatic symptoms.	History of pains in legs and arms ten weeks ago, which continued for five weeks, but were not bad enough to compel her to go to bed.
CASE XIII. Rosina B. 11½ years. Nov. 1879.	Over right olecranon, and over external condyle of left humerus; over left patella.	No erythema.	Normal.	Mitral regurgitation.	Bilateral. Had existed for three months.	History of rheumatic fever four years ago.
CASE XIV. Walter B. 11 years 10 months. April, 1880.	Over external condyle of humerus; over olecranon; over lower end of radius on both sides; over both patellæ; above each external malleolus. Size, split pea to hempseed. These diminished but did not entirely disappear during the three weeks that he was under observation.	No erythema.	...	Mitral and aortic regurgitation. Dilatation. Bruits altered under observation.	None.	History of two previous illnesses, in which the boy did not keep his bed, but had pains in joints, the knees being swollen and subsequently stiff. Present attack began five months ago with pains in joints, palpitation, and short breath. Three weeks after the onset nodules were noticed by his mother about the elbows, wrists, and knees.

Middlesex Hospital.	the back of each hand connected with the extensor tendons; connected with the palmaris longus and flexor carpi radialis tendons; connected with the hamstrings and the tendons of the common extensor on the dorsum of one foot. The nodules on the back of each hand were first noticed three weeks after admission. During the forty-four days afterwards that he was under observation several fresh crops appeared; those noted on the forehead increased from day to day. Some of the individual nodules grew to be as big as Barcelona nuts, but the average size was that of a split pea. Three months after his discharge, that is to say, nearly five months after onset of nodules, they had entirely disappeared. The patient states that a former severe attack of rheumatic fever (five and a half years ago) left him with lumps like the above, and that they entirely disappeared after a time.		evening temperature of about 100°, and a morning temperature of 98·4°. It was noted, however, that on two occasions fresh nodules appeared on days when both morning and evening temperatures were normal.			throat, and short breath. There was effusion in both knee-joints, swelling about ankles and one wrist, and some thickening of metacarpo-phalangeal joints. Evening temperature 102°. Pain and swelling rapidly subsided, but relapses, especially in the wrist, occurred, and the heart mischief increased. There was much wasting and anæmia, and slight œdema of the feet appeared whilst in the hospital. This cleared up, and then the patient was discharged with still a number of nodules present.
CASE XVI. Mary I. V. 5 years 2 months. May, 1881.	One on external condyle of right humerus; two over right olecranon; one on knuckle of right index finger; one on external condyle of each femur; three on right internal malleolus; two on left internal malleolus; one on left external malleolus. A large nodule on outer side of each tendo-achillis, near its insertion; two higher up on the right tendon; one on the left. Over the nodules about the malleoli (and over these only) there was slight redness. There was also a little complaint of pain about the ankles; nevertheless, pressing the nodules did not evoke any complaint. Some of the nodules were noticed by the mother six weeks before admission. The mother spoke positively about the appearance of successive nodules. During one week that the child was an out-patient fresh nodules formed, but during the nineteen days that she was in the hospital in bed no fresh nodules appeared, and those present on admission soon subsided. Thus the total duration was not probably more than two months. There were no black and blue changes in the skin over the nodules about the ankles, where there had been slight redness.	No sweating. No erythema marginatum. Slight redness over some of those nodules which were situated about the ankles.	Normal when nodules were first seen, at which period fresh ones were forming. During nineteen days that the child was in hospital, when the nodules were subsiding, temperature normal.	Slight hypertrophy. Mitral regurgitation.	None.	There is a strong family history of rheumatism on the mother's side. This child five months ago, after a sea-voyage, had complained much of aching in her joints. The joints were not swollen, but they were stiff. For about five weeks before she was brought to the hospital she had complained much of pains in her joints, especially at nights. There had been no swelling or redness except over some of the nodules before described. She had not sweated at all. During the period that she was in the hospital there was no arthritis.

CASES OF SUBCUTANEOUS NODULES—*continued.*

Name and Age.	Nodules.	Skin.	Temperature.	Heart.	Chorea.	Evidence as to Rheumatism.
CASE XVII. Mary L. J. 5 years 11 months. Aug. 1881. Fatal.	One over the eleventh dorsal spine appeared three days after admission into hospital, and twenty-four days after the onset of joint pains. When first felt it was the size of a split pea; During fifteen days it subsided so that it could no longer be felt. But post-mortem a small nodule was found having the appearance of a boiled sago grain, situated in the deep fascia over the eleventh dorsal spine. On the tenth day after admission a nodule appeared on external condyle of the right humerus. This subsided in a few days. Some minute nodules were found post-mortem which had not been felt during life. They were of the size of a pin's head, and were situated in the substance of both temporal fasciæ and the pericranium between them. They were at least thirty in number. Corresponding with many of those situated in the pericranium there were minute pits in the outer surface of the parietal bones, especially on the left side. These pits were apparently caused by pressure.	Erythema marginatum and papulatum for one month before admission. Situated on trunk and lower limbs. Faded in two days, leaving slight staining. Fresh patches appeared on the seventh, eighth, ninth, and tenth days after admission. No sweating.	For eight days the morning temperature was generally normal. During the succeeding ten days it was generally 102·2°. The evening temperature ranged from 100° to 102°.	On admission, systolic and presystolic apex murmurs. On the sixteenth day after admission well-marked pericardial friction was heard. She had had vomiting and much dyspnœa for four days previously. Post-mortem :—Universal pericarditis, with one ounce of serous effusion, Both auricles dilated. Endocardium of left auricle thickened and opaque. Mitral valve thickened; many recent vegetations on it; some small vegetations on one aortic valve. Left ventricle slightly hypertrophied. Heart and pericardium together weighed 10 oz. Slight recent pleurisy left side. Liver nutmeg. Brain and membranes healthy. Nodules *vide ante.*	Slight for two days before death, affecting face, muscles, and speech.	History of rheumatic fever, in which all her joints were affected one year ago. She was in bed for one month with it, and has not been strong since. Present illness:—Has had flying pains in joints for three weeks. When admitted the pains ceased, and there was no arthritis. On the seventeenth day there appeared distinct effusion with pain and redness in the right wrist-joint. No other joints were affected.
			DR. WARNER'S CASES.			
CASE XVIII. Olive B. 9¼ years. July 31, 1878.	One over each spinous process; large ones over both scapulæ at spine and acromion, also at the angles. That at angle of left scapula had the skin over it a little reddened; it had been rubbed with some liniment. Others over prominences of elbows and wrists; small ones over knuckles and inter-phalangeal joints. Large ones surrounded the patellæ; others over extensor tendons of ankles. There were forty or fifty in all. They appeared five weeks before admission; no fresh ones developed under observation. They gradually disappeared in ten weeks from the time of onset. The nodule was excised from the back	No erythema.	No pyrexia.	A distinct mitral regurgitant murmur which did not vary.	Followed the joint pains and development of nodules. Well marked, principally affecting right side.	Father rheumatic. Six weeks before admission joints painful; this pain, much increased by movement, increased and laid her up. When admitted, joints painful but *not swollen.* The fingers of both hands contracted and flexed; the attempt to straighten them caused pain. The sheaths of palmar tendons presented ridges, and were tender; this condition passed away completely from the right hand, but still existed in the left hand when she was dis-

	later erythema returned accompanied by fresh nodules over the sternum and metacarpal bones.	ed in successive crops. Patches increased in size and were not painful. Rash and nodules co-existed.		lated and hypertrophied. Vegetations on mitral and aortic valves. Tricuspid valve thickened. Lungs congested.		
CASE XX. Selina H. 18 years. Nov. 19, 1880.	At base of first phalanx of left middle finger flexor surface an elongated lump ¼-inch long, not tender, movable laterally but not lengthwise; apparently connected with sheath of tendon; skin over it movable. (This developed early in the case.) Small movable nodule over head of left radius; thickening over extensor tendon of left great-toe; thickening over right and left peroneal tendons. These developed when patient was convalescing. There was a coincident onset of pleurisy and a rise of temperature.	...	100°-101° F. It was elevated during the eruption of the nodules, but this was probably due to the coincident pleurisy.	Aortic and mitral disease; probable active endocarditis in hospital. Murmurs subsequently noted as less distinct. Heart's action at times tumultuous with dyspnœa.	None.	Arthritis under observation. Pains in all the joints. In-patient two years ago for rheumatism and endocarditis, a mitral regurgitant bruit then developed.
CASE XXI. George I. 14 years. Feb. 3, 1880.	Situated on acromial end of both clavicles; over right olecranon nodule ¼-inch long; left only a slight thickening; others over knuckles. Over each patella 5 nodules size of split pea, symmetrically placed. They were said to be of several weeks' duration, and to have been preceded by pain in the limbs; they were never painful, and became smaller in a fortnight, finally all disappeared.	Skin harsh and dry; no erythema.	99°-100° F. subsided in a few days.	Mitral regurgitation on admission. Pericarditis subsequently developed in hospital.	None.	Mother had had rheumatic fever. Patient complained of pain in limbs of several weeks' duration. Slight tenderness of several large joints with distinct effusion.
CASE XXII. William W. 14 years. Nov. 22, 1880.	First noted a few days after onset of chorea. Situated over knuckles of second and third fingers of right hand. Ill defined over right external condyle of humerus; others over lower part of sternum. They began to disappear in three weeks.	Clammy with sour sweat.	...	Mitral regurgitation; bruit became more developed.	Present: Three previous attacks—the first, five years ago, lasted three months; the third attack followed on rheumatic fever.	Rheumatic fever two years ago. Sour sweating in present illness.
CASE XXIII. Emma H. 12 years. Aug. 2, 1879.	Situated over both elbows and hands on extensor tendons; over external malleoli. They did not appear till the subsidence of the erythema and pericarditis. When patient left hospital, five weeks after their eruption, they were subsiding.	Urticaria and erythema in patches on back and limbs; it came out with itching. A second crop of urticaria was not followed by nodules. Sweating.	102°-103° F. on admission; a week later normal; so also on the eruption of nodules.	Mitral regurgitation and pericarditis on admission; a week later the bruit at apex became musical. A fortnight later an aortic systolic bruit developed.	None.	Laid up with rheumatism about a year ago. In present illness arthritis in large joints, with sweating.

CASES OF SUBCUTANEOUS NODULES—*continued.*

Name and Age.	Nodules.	Skin.	Temperature.	Heart.	Chorea.	Evidence as to Rheumatism.
CASE XXIV. Emma G. 4½ years. Dec. 11, 1878.	Numerous; situated over both olecranon processes; left internal and right external condyle of humerus; external malleoli. Extensor tendons of toes over metatarso-phalangeal joints; over spine of scapula. Most disappeared in a month.	...	99 °F.	Mitral regurgitation on admission. No change in bruit.	Slight; commenced after nodules appeared.	Brought to hospital on account of lumps. Has had sub-acute joint pains. Family history:—Father rheumatic; sister has had rheumatic fever and heart disease. From history it appears certain that she also had nodules.
CASE XXV. Keziah I. 16 years. Oct. 1880. Fatal.	Right hand: — Over knuckles of first and middle finger, six in all. Left hand:—Eleven in all, distributed over the four knuckles. One over head of left ulna. All appeared in one eruption, along with erythema marginatum, fifteen days after admission, and had completely disappeared within one month.	Erythema marginatum on trunk, and slight itching, along with eruption of nodules. Fresh patches three days later, without additional nodules. Disappeared within a week. Skin moist.	101° F. on admission; after a few days, normal. No rise with eruption of nodules. Subsequently 102° F., with severe cardiac dyspnœa.	Mitral regurgitation and dilatation; intense dyspnœa, followed by death. Post-mortem:—Left ventricle dilated and full of clot; vegetations on aortic, mitral, and tricuspid valves; lungs œdematous.	None.	Six months ago laid up eight weeks with rheumatic fever, and has been short of breath ever since. On admission this time several joints painful, but no effusion.
CASE XXVI. Emma K. 17 years. Nov. 26, 1880. Fatal.	Over spine of right scapula; several over both condyles of right humerus and olecranon; several over both condyles of left humerus; over dorsal and lumbar vertebral spines; two on left patella. Date of onset could not be fixed. During the six weeks that she was under observation there was slight diminution in size, but none of the nodules disappeared.	No erythema.	98°–99° F. Once it reached 101° F.	On admission, mitral regurgitation and dilatation; dyspnœa on exertion and cardiac pain. Subsequently double aortic bruits. Whilst walking about, sudden syncope set in, and she died.	Had chorea in 1875, after first attack of rheumatism. Not in present illness.	Had rheumatic fever in 1875, and then had mitral regurgitation only. Joint pains, but no effusion in present illness.
CASE XXVII. Emma B. 13 years. Nov. 15, 1880.	One over first inter-phalangeal joint, and over knuckle of right index finger (these appeared during relapse of arthritis). A second crop a week later on other phalangeal joints; also five on external condyle of each humerus. Small nodules over left olecranon; several over both patellæ and over each external malleolus. Some of them diminished under observation.	Sweating. Erythema marginatum appeared after the arthritis subsided, and lasted three days; it returned with the nodules. Purpuric spots of legs during convalescence.	On day of admission 105° F.; fell next day. Average, 100°–101° F.	Slight mitral regurgitant murmur, which increased. Thrill developed under observation.	None.	Mother has had rheumatic fever. This patient had rheumatic fever twelve months ago, with pain and swelling of joints. In the present attack she had been laid up with pain, swelling of joints, and sweating. The arthritis had subsided previous to admission, but relapsed under observation in the hospital.

BIBLIOGRAPHY.

We find that nodules of the kind which we have described have been referred o by other writers. Our first case was observed in 1875, but Dr. Meynet of Lyons had under his care in 1874 a boy aged fourteen years who presented imilar lesions. His case is recorded in the *Lyon Médical*, No. 49, 1875. The patient had had two previous attacks of rheumatic fever, and was the subject of heart disease. He was admitted to the "Charité" after having suffered for six weeks with pains in the joints and palpitation. When admitted he had very little pyrexia, and scarcely sweated at all, but there was some effusion in the sheaths of the tendons about the hands. Around the metacarpal and phalangeal joints there were numerous small deposits which were movable. Connected with the flexor tendons in the forearm and on the inner edge of each ulna were other nodules, varying in size from a hempseed to a Barcelona nut. Other positions for the nodules were:—around the knee-joints, the tendons of both feet, the vertebral spines, the forehead, and the crown of the head.

Dr. Meynet observed several of these growths from day to day, and that they were not affected by the use of iodide of potassium. Many had subsided at the end of a month, but there were still several deposits to be seen.

Dr. Rehn, in his article on Acute Rheumatism in "Gerhardt's Handbook of Children's Diseases" (p. 21, 1st div. 3d vol., 1878), has described similar tumours in a girl aged ten years suffering from relapsing rheumatism. These, at the outset, were decidedly painful, but afterwards became insensitive to pressure, and disappeared without treatment. Since the above article was written, Dr. Rehn has seen another case of nodules in a child aged four and a half years, who was the subject of articular rheumatism, with pleurisy, pericarditis, and peritonitis.

In this case also, at the outset, the nodules were a little painful, but they soon became insensitive to pressure, and diminished in size and number.

Dr. Hirschsprung of Copenhagen has written a valuable and exhaustive paper on the subject in the *Jahrbuch für Kinderheilkunde*, March, 1881.

He records four cases which came under his own observation. The first of these, after the lapse of seventeen months, had a subsequent attack of rheumatism, in which nodules reappeared. Some of them appear to have been distinctly tender on pressure, and the skin over them was a little reddened. Dr. Hirschsprung had an autopsy on one of his cases in which the heart lesions were very severe.

Two nodules removed from the tendon of the triceps brachii at the olecranon were examined microscopically. Besides the strands of fibrous tissue, spindle-shaped and caudate cells, and the abundant vascular supply to which we have referred in one of our cases, Dr. Hirschsprung found in some places multinucleated cells, isolated and in groups, and some areas where there was slight indication of necrobiosis.

In the *Revue de Médecine*, April, 1881, Messrs. Troisier and Brocq have recorded one case of rheumatic nodules which came under their observation.

This was in an adult aged forty-five, who had had two previous attacks of acute articular rheumatism, and was admitted into hospital after a fortnight's illness for a third. He had effusions in the large joints, high fever, and a mitral regurgitant murmur. In eight days he developed double pleuritic effusion, but was convalescing, when, on the forty-third day, some small tumours appeared on the occiput. The patient has been led to notice them on account of the pain caused by resting his head on the pillow. He affirmed that exactly similar tumours had formed on his forehead and on the scalp in the course of his

second attack of rheumatism. There were about ten of these tumours. They varied in size from that of a pea to that of a small hazel-nut. They were not the seat of spontaneous pain, but continued pressure gave rise to distress. They were very slightly movable on the bone, and the scalp was movable over them.

About two months after admission, the patient had a relapse of joint pains and an attack of pericarditis, and three days afterwards some fresh nodules appeared on the forehead. In two days more there was a nodule on each ear, and fresh rheumatic pains in the knees and elbows. After this there was a gradual subsidence in number and size, and about fifteen weeks after admission there were no nodules to be found anywhere.

Messrs Troisier and Brocq, not having had any autopsy, and not being acquainted with the valuable memoir of Dr. Hirschsprung, can only speculate on the anatomical site and nature of these lesions. From our observations, based on post-mortem examination, it will be seen that we agree with the belief of these authors, and also of Dr. Meynet, that the lesions belong strictly to the fibrous tissues and not to the cellular tissue, and that they are attached specially to the sheaths of tendons, to the deep fascia, and, in the case of the head, to the pericranium.

It appears from the foregoing memoir that Jaccoud and Besnier have both referred to the rare occurrence of these nodules in rheumatic subjects, and M. Féréol has also recorded the case of a lady who comes of an arthritic stock, but has not herself suffered from rheumatism. This patient is the subject of obstinate pityriasis of the scalp and of severe migraine. She has on many occasions had subcutaneous nodules on the scalp, which have come and gone very rapidly. Whether this is one of the same group must remain, we think, an open question.

DISCUSSION.

Dr. RANKE, Munich: I wish to point out the great importance of the study of the geographical distribution of disease. I think that future International Congresses might do most useful work if general attention were drawn to this point and followed up. In regard to chorea, I am inclined to think that it is much less frequent in some places than in others. I have lately searched my books, and found that amongst 40,723 children treated by me in the University's dispensary for sick children since 1867, there have been only nineteen cases of chorea, and amongst these nineteen only three have presented a systolic murmur; in all the others the heart sounds were found normal. Amongst the three cases with mitral trouble, there was only one in which an attack of acute rheumatism had previously occurred. Of the nineteen cases, two followed immediately after fright; in all the rest an immediate cause could not be discovered.

Sur l'Erosion des Dents considerée comme Signe retrospectif de l'Eclampsie infantile.

Dr. MAGITOT, Paris.

MONSIEUR LE PRESIDENT, MESSIEURS,—Tous les médecins savent, et en particulier ceux qui s'occupent spécialement des maladies des enfants, qu'on observe fréquemment à la surface des dents permanentes, et plus rarement des dents

temporaires, une certaine altération de tissu de nature congénitale, et consistant soit dans des échancrures du bord libre, soit dans des sillons annulaires plus ou moins profonds et plus ou moins nombreux, et toujours symétriques aux dents homologues d'une même mâchoire. Cette lésion a été désignée sous le nom générique d'*érosion*.

Or l'histoire de cette altération appartient au domaine de la séméiologie générale, car elle est, de l'avis unanime des auteurs, l'indice permanent et indélébile d'un trouble de formation de la couronne dentaire, trouble survenu pendant la période intra-folliculaire, et dont la cause pathologique est nécessairement contemporaine de la période du développement à laquelle correspond le niveau même de l'altération.

Une première assimilation, très-légitime d'ailleurs, rapproche de l'érosion dentaire la production du sillon unguéal de Beau, qu'on pourrait appeler justement l'érosion de l'ongle, mais avec cette différence, que l'érosion de l'ongle se montre de suite à la base de celui-ci et disparaît par le fait de son rapide développement, tandis que l'érosion intra-folliculaire d'une dent n'apparaît que plus tard, à l'époque de l'éruption ; la première est conséquemment fugace, tandis que la seconde est permanente et indélébile.

Une autre assimilation rattache encore à ces deux érosions *unguéale* et *dentaire*, la lésion connue sous le nom de *cataracte zonulaire congénitale*, décrite par les auteurs allemands, et en France par M. Nicati, et ce triple rapprochement est d'ailleurs conforme à la loi physiologique qui classe dans le même ordre de tissus, les *produits*, *l'organe dentaire*, *l'ongle* et le *cristallin*.

Or, pour nous borner ici à l'étude de l'érosion dentaire, et afin de fixer les rapports réciproques entre le lieu exact de cette lésion sur une couronne en voie de formation et l'âge de l'intervention de la cause productrice, il nous faut établir quelles sont les phases du développement de la couronne des dents, et plus rigoureusement quel est, à une époque déterminée, l'état du *chapeau de dentine* pour les différents follicules. C'est ce qui résulte du tableau suivant.

Les indications fournies par ce tableau ne doivent pas toutefois être regardées comme absolues, et les diverses époques fixées ainsi d'après nos études personelles, présentent certains écarts suivant les sujets, les constitutions et même les maladies. Ces variations, qui ne peuvent être que de quelques jours pour la première colonne relative à l'apparition du chapeau de dentine, s'accentuent davantage à la seconde colonne, mais surtout à la troisième ; c'est-à-dire, que l'époque assignée à telle ou telle hauteur d'un chapeau de dentine peut varier de quelques semaines et même de quelques mois. Ce sont là en tout cas des données moyennes.

Quoi qu'il en soit, les conséquences de ces indications à l'égard de l'érosion se résument de la manière suivante :

1° Si une intervention morbide porte son action sur un follicule au début de la formation du chapeau de dentine, l'érosion occupera soit le bord libre s'il s'agit d'une incisive ou d'une canine, soit la surface triturante s'il s'agit d'une molaire. C'est ainsi que nous pouvons montrer un fait d'érosion exclusive à la face triturante des premières molaires dont le chapeau apparaît au sixième mois de la vie fœtale. Cette érosion fut due à des accidents de la grossesse auxquels la mère a d'ailleurs succombé.—(Observation 21 du tableau analytique, No. 3.)

2° Si cette même influence morbide intervient plus tardivement, l'érosion aura pour siège un point plus ou moins distant de ce bord libre, mais toujours précédé d'une région saine dont l'étendue est proportionelle à la durée même de la période de santé qui a précédé les accidents.

Quelques mots maintenant sur la nature exacte de cette lésion et sur ses formes diverses. Nous serons bref, car les auteurs anciens et modernes sont d'accord sur cette question :

L'érosion dentaire est caractérisée essentiellement tantôt par un simple trouble dans la formation simultanée de l'ivoire et de l'émail, tantôt par une suspension complète de cette formation. S'il y a trouble léger sous l'influence d'une cause faible et fugace, l'érosion se traduira par un sillon simple peu profond, parfois même difficile à reconnaître. Si le trouble est intense ou prolongé ainsi que la cause qui l'a provoqué, l'érosion sera étendue en surface ou en profondeur, formant quelquefois une véritable zone annulaire plus ou moins large avec absence congénitale d'émail et altération pénétrante dans l'ivoire. Enfin, si les crises morbides ont été fréquemment répétées et très-rapprochées dans leurs apparitions, une surface très-étendue ou la totalité de la couronne pourra être frappée des altérations caractéristiques.

TABLEAU No. 1.

Etat du Chapeau de Dentine pour chacun des Follicules dentaires au Premier Age chez l'Homme.

Désignation des Dents.	Apparition du Chapeau de Dentine.	Dimension en Hauteur du Chapeau de Dentine à la Naissance.	Dimension en Hauteur du Chapeau de Dentine au 6e Mois.
A.—Dentition Temporaire.			
Incisives centrales inférieure	16e semaine de la vie embryonnaire	mm. 3 5	La couronne est entièrement formée et prête à l'éruption.
Incisives centrales supérieure			
Incisives latérales inférieure			
Incisives latérales supérieure			
Canines inférieure		3 2	6mm.
Canines supérieure			
Premières molaires inférieure	17e semaine de la vie embryonnaire	3 0	7mm.
Premières molaires supérieure			
Deuxièmes molaires inférieure			
Deuxièmes molaires supérieure			
B.—Dentition Permanente.			
Incisives centrales inférieure	1er mois de la naissance	...	2mm.
Incisives centrales supérieure			
Incisives latérales inférieure			
Incisives latérales supérieure			
Canines inférieure	Du 3e au 4e mois de la naissance	...	1mm.
Canines supérieure			
Premières prémolaires inférieure	Du 5e au 6e mois de la naissance	...	...
Premières prémolaires supérieure			
Deuxièmes prémolaires inférieure			
Deuxièmes prémolaires supérieure			
Premières molaires inférieure	6e mois de la vie fœtale	2mm.	6mm.
Premières molaires supérieure			
Deuxièmes molaires inférieure	3e année	...	...
Deuxièmes molaires supérieure			
Troisièmes molaires inférieure	12e année	...	...
Troisièmes molaires supérieure			

C'est ainsi que se classent les formes diverses d'érosion de la manière suivante :

1° Erosion *en échancrure ou en coup d'ongle* du bord libre (incisives).

2° Erosion en rugosités ou mamelons de la face triturante (molaires).

3° Erosion *en sillon simple ou pointillé*, unique ou multiple. L'existence de rainures multiples superposées a été désignée par J. Tomes, sous le nom d'érosion *en étages ou en escalier*.

4° Erosion en nappe avec absence congénitale d'émail et état spongieux de

l'ivoire. Elle indique de la part de l'intervention morbide une durée proportionelle à l'étendue de la lésion.

5° Enfin, érosion totale de la couronne de certaines dents, avec durée correspondante de la cause productrice, altération désignée par le même auteur sous le nom de dents en *gâteau de miel.*

Cette dernière forme, par l'intensité et la généralisation de son état, nous paraîtrait même devoir être distraite de la classe des *érosions* proprement dites. Ici, en effet, il n'y a plus de sillons ou d'échancrures symétriques et parallèles ; la couronne est tout entière désorganisée et elle disparaît d'ordinaire très-vite par suite de carie consécutive. La durée de la cause morbide a dû être considérable, et nous pourrions citer maintes observations dans lesquelles, en dehors de toute diathèse syphilitique ou autre, un état maladif de la première enfance a pu causer une telle lésion.

Voici, par exemple, une observation dans laquelle les premières molaires et les incisives des deux mâchoires ont présenté la forme *en gâteau de miel* et une destruction ultérieure complète, consécutivement à une entérite chronique qui a duré depuis les premiers mois de la vie jusqu'à la deuxième année.

Nous en donnerons simplement le sommaire :

Observation : Homme de 23 ans ; entérite chronique grave, ayant débuté au 2e mois et ayant persisté jusque la deuxième année, pendant tout la durée et à la suite d'un allaitement artificiel—aucun soupçon de syphilis, aucune trace de rachitisme; pas d'accidents scrofuleux, pas d'attaques éclamptiques. Au moment de l'éruption de la seconde dentition, les incisives, les canines, et les premières molaires apparurent dans un état de désorganisation complète, dans la forme dit *gateau de miel.* Elles furent très rapidement détruites par la carie, plusieurs prémolaires présentent aussi des caries moins généralisées et qui n'ont pas être mises en traitement. Les deuxièmes molaires, dent d'évolution, répondent à une epoque où la santé de l'enfant s'était rétablie, ne presentent aucune lésion.

Tels sont les aspects divers de l'érosion.

Notre savant ami, M. le professeur Parrot, a tenté de modifier cette classification et en a proposé une autre dans les termes suivants :

1° *Atrophie cuspidienne* portant sur la partie la plus saillante de la dent : elle serait très-commune aux premières molaires permanentes, ce qui impliquerait une origine fœtale, aux prémolaires de la seconde dentition, et plus atténuée à celle de la première dentition.

2° *Atrophie cupuliforme*, avec absence de l'émail et mise à nu de l'ivoire, c'est la forme en nappe associée ou non à la précédente.

3° *Atrophie sulciforme ou en sillon*, c'est la forme en *rainure* simple ou multiple.

4° *Atrophie en hache.* Celle-ci, de l'aveu de M. Parrot, ne serait pas congénitale, mais secondaire et pathologique. C'est de toute évidence une carie consécutive. Elle ne saurait donc, à aucun titre, entrer dans le cadre de l'érosion qui est avant tout une lésion congénitale, une anomalie de nutrition intra-folliculaire.

5° *Atrophie Hutchinsonienne.* C'est la variété en coup d'ongle, échancrure du bord libre.

Nous n'entrerons pas dans la discussion de cette nouvelle classification, et nous demandons la permission à M. Parrot de conserver l'ancienne qui nous paraît plus simple et plus physiologique.

Voici au reste un tableau représentant les types principaux qu'affecte l'érosion. Nous avons fait représenter dix formes non seulement au point de vue de leurs caractères, mais aussi à l'égard des dents affectées et des point qu'elles occupent. Les exemples pris de nature sont d'ailleurs empruntés à la série d'observations que nous publions d'autre part dans un autre tableau d'ensemble.

TABLEAU No. 2.

Principaux types d'érosion.

1 Erosion en *mamelons* et rugosité exclusi aux premières molaires permanentes (face t turante). (Observation 21 du tableau ana tique.)

2 Erosion en *mamelons* des molaires en *sill simple* près du bord libre des incisives et ve le milieu des canines *temporaires* supérieur persistantes. (Observation 22.)

3 Erosion en *mamelons* des molaires et e *sillon simple* des incisives. (Observation 23.)

4 Erosion en *sillon* des molaires et en *coup d'ongle* des incisives. (Observation 24.)

5 Erosion en *sillon pointillé* des incisives et des molaires. (Observation 25.)

6 Erosion en *double sillon* aux incisives et aux molaires. (Observation 26.)

7 Erosion en *nappe* aux incisives, en *sillon* aux canines et aux molaires. (Observation 27.)

8 Erosion en *nappe* avec végétations mamelonnées. (Observation 28.)

Erosion en *sillon simple* portant exclusivement sur la partie moyenne des canines permanentes. (Observation 29.)

Erosion en *sillon simple* portant exclusivement sur la partie moyenne des premières prémolaires. (Observation 30.)

Dans tous les cas, ce qui ressort évidemment de ces considérations, c'est qu'il existe d'abord un rapport étroit entre les trois termes suivants :—

1° Cause morbide produisant la lésion ;

2° Le niveau anatomique de la lésion ;

3° Son étendue.

Les trois termes peuvent ainsi être considérés comme constituant une équation pathologique.

Une autre forme qu'il importe de déterminer ici : c'est *l'identité de nature* de ces différentes formes quels que soient d'ailleurs leur niveau et leur étendue.

L'érosion est une, et les diversités d'aspect résultent simplement de l'époque d'apparition de la cause productrice, de la durée d'intervention de celle-ci, et de l'état du chapeau de dentine au moment de l'invasion des accidents. Les variétés en sillon simple ou multiple s'expliquent ainsi par une ou plusieurs apparitions de la cause ; la variété en nappe, est due à la persistance prolongée de son action, et quant à la forme spéciale, en coup d'ongle ou Hutchinsonnienne, suivant M. Parrot elle semble due à un mécanisme particulier qui est le suivant :

Un trouble de formation du bord libre d'une incisive a pour résultat d'amener au dehors une couronne dont le bord est aminci et très friable. Cette partie désorganisée est limitée par un bourrelet soit rectiligne soit demi-circulaire, et au delà duquel la constitution anatomique de l'organe redevient normale ; or, c'est la fracture de cette partie amincie pendant les premiers temps qui suivent l'éruption qui donne dans la suite l'aspect spécial qu'à décrit M. Hutchinson ; l'érosion en coup d'ongle n'a pas d'autre origine.

Ces faits étant une fois fixés dans leurs rapports réciproques, et reliés ainsi d'un façon systématique, arrivons de suite au but spécial de cette étude, la recherche étiologique de l'érosion.

Or, les opinions émises par les auteurs anciens et modernes peuvent se grouper sous trois chefs :

1° L'opinion qui attribue la production de l'érosion aux *affections quelconques de l'enfance.*

2° Celle qui considère l'érosion comme signe constant de *syphilis héréditaire.*

3° La théorie qui rattache l'érosion à *l'éclampsie infantile.*

Nous allons les passer rapidement en revue.

L'explication qui attribue l'érosion aux affections quelconques de la première enfance est celle des auteurs anciens, et l'on peut dire celle de tous les temps. Ambroise Paré la mentionne ; de même après lui, Bunon, Fauchard, &c. ; plus récemment, Duval, Oudet.

Aujourd'hui encore, mais dans une certaine mesure, elle est soutenue par Tomes en Angleterre et par Broca en France.

Nous pensons qu'une affirmation aussi générale, qui s'adresse à toutes les maladies de l'enfance, ne saurait se soutenir. Si nous envisageons par exemple une grande classe des affections de la première enfance, les fièvres éruptives, il est aisé de se convaincre, par un nombre illimité de faits, qu'elles n'apparaissent guère dans la première ou la deuxième année, et qu'elles sont d'ailleurs impuissantes à produire l'érosion caractéristique, à moins qu'elles ne se compliquent de troubles graves du système nerveux dont nous parlerons plus loin. Il en doit être de même des affections catarrhales ou intestinales qui ne présentent que bien rarement ces deux caractères d'invasion brusque et de perturbation profonde de la nutrition, qui sont les circonstances essentielles du mécanisme de l'érosion. M. Parrot a d'ailleurs developpé lui-même ce point de vue et nous sommes ici tout-à-fait d'accord avec lui.

Quant aux diathèses héréditaires ou acquises, rachitisme et scrofule par exemple, nous leur avons reconnu depuis longtemps une influence considérable sur l'organisation des dents.* Mais cette influence, qui par sa permanence même doit porter sur la totalité de l'organe, se traduit par des altérations de forme générale et de structure intime qui n'ont aucun rapport avec l'érosion. Nous savons, il est vrai, que M. Parrot considère la scrofule et le rachitisme comme des manifestations syphilitiques, mais c'est là une opinion très-personnelle, contre laquelle s'élèvent à la fois le sentiment général des pathologistes et des faits nombreux.

Quoi qu'il en soit, il est certain que les dents des rachitiques, par exemple, sont petites, atrophiées, difformes même, et frappées de défectuosités générales de structure et de constitution chimique. Il y a là sous nos yeux certaines pièces qu'on serait en droit de classer dans cette catégorie, si nous n'avions retrouvé dans les antécédents des troubles nerveux graves, dont la trace est marquée par l'érosion. De la sorte on reconnaît chez le même sujet le vestige du rachitisme et le témoin de l'éclampsie, c'est-a-dire la preuve de la coexistence des deux affections.

La seconde opinion, celle qui attribue l'érosion dentaire à la syphilis héréditaire, est née en Angleterre, d'un travail de M. Hutchinson, publié en 1863,† et elle est défendue en France avec une grande autorité par le professeur Parrot.‡

Cette doctrine n'a point encore de partisans, du moins en France, à l'exception toutefois de M. Lannelongue qui semble disposé à s'y rallier.

En ce qui concerne M. Hutchinson, son travail est, comme on sait, consacré à l'étude de la kératite interstitielle dans ses rapports avec la syphilis héréditaire, et ce n'est qu'un peu incidemment qu'il mentionne chez les mêmes sujets une forme particulière d'érosion, dont il attribuerait en réalité la cause à la stomatite qui accompagne le traitement mercuriel.

M. Hutchinson publie ainsi une série de 102 faits de kératite syphilitique, parmi lesquels nous relevons les points suivants :—

Dans 63 cas, l'auteur mentionne la coexistence de l'échancrure du bord libre des incisives, mais, chose suprenante, sans signaler l'altération des molaires qui est, comme on sait, constante. Dans 39 cas, les conditions des dents n'ont pas été notées ou ont été trouvées normales.

Sur la question des affections concomitantes on reconnaît dans les observations du mémoire des troubles divers se rattachant à la syphilis, mais aucune

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* "Traité des anomalies de l'appareil dentaire," 1877, p. 256.

† "A Clinical Memoir on Certain Diseases of the Eye and Ear consequent on Inherited Syphilis."

‡ "De la Syphilis dentaire, Communication faite au Congrès de l'Association française de Reims, 1880." Clinique de l'Hospice des Enfants-Assistés, *Gaz. des Hôpit.*, 1881, Nos. 74, 78, 80, et 82.

mention de la coexistence ou de l'absence de l'éclampsie. Deux sujets seulement sont dits *épileptiques.*

Tels sont les points principaux que nous avons relévés dans le mémoire de M. Hutchinson, mais il nous semble que les idées de l'auteur se sont notablement modifiées depuis lors et qu'il est disposé à regarder certaines formes d'érosion comme absolument caractéristique de la syphilis.

Pendant notre séjour à Londres, M. Hutchinson a bien voulu nous engager à l'accompagner au *London Hospital* on il désirait nous montrer quelques malades affectés des lésions dont il s'agit, et concurremment avec la kératite interstitielle et la syphilis héréditaire. Cette visite a été pour nous d'un très-grand intérêt, et nous ne saurions trop l'en remercier. Elle nous a permis en effet d'observer plusieurs sujets offrant une certaine forme d'altérations dentaires et particulièrement cette forme que nous avons indiquée comme se rapprochant beaucoup plus des lésions d'ensemble dues aux diathèses que de l'érosion proprement dite. C'étaient des dents mal conformées, des incisives ayant en quelque sorte l'aspect de moignous difformes, de cônes tronqués, de véritables monstruosités morphologiques.

Or ces sujets portaient d'autre part des traces manifestes de rachitisme ancien ou actuel, de scrofule, de syphilis même, et ils ne pouvaient en outre fournir sur leur première enfance aucun témoignage au sujet de l'éclampsie.

Nous n'insisterons pas sur cette discussion en ce que concerne M. Hutchinson, et nous avons hâte d'arriver à M. Parrot qui formule très nettement son opinion en donnant à l'érosion des dents le nom de *syphilis dentaire.*

Les arguments de M. Parrot reposent sur la coexistence avec l'érosion dentaire de certaines lésions osseuses du crâne, des mâchoires ou d'un autre point du squelette ; sur des macules cutanées, traces d'ulcérations éteintes ; sur des ostéophytes, et enfin sur le rapport entre le siège de l'érosion et l'âge d'élection des accidents syphilitiques qui évoluent, suivant lui, depuis le sixième mois de la vie fœtale, jusqu'à la quatrième année.

Or voici nos objections à cette théorie :

1° M. Parrot ne nous a fourni jusqu'à présent aucune observation d'érosion caractéristique avec altérations osseuses spécifiques, accidents cutanés indéniables, et *en l'absence de toute autre intervention morbide ;* en un mot, un diagnostic absolu de syphilis héréditaire. Tout au contraire, M. Parrot, entraîné par l'ardeur de sa conviction, arrive à affirmer *à priori* la syphilis héréditaire sur la seule existence de l'érosion. Une mâchoire d'adolescent, trouvée dans un cimetière gallo-romain, porte deux sillons dentaires superposés ; M. Parrot conclut à la syphilis préhistorique. Un sujet vivant présente la même lésion, il le déclare syphilitique bien qu'on ne puisse saisir dans les antécédents aucune manifestation de cette diathèse. Ce sont là des affirmations dont la gravité n'échappera à personne.

2° Nous pourrions apporter un nombre considérable de faits, dans lesquels des sujets notoirement syphilitiques héréditaires ne présentent aucune trace d'érosion. Nous invoquerons à cet égard le témoignage de M. Alfred Fournier, qui, dans une longue pratique, n'a jamais saisi cette relation. Mentionnons en outre une observation personelle que nous avons faite récemment dans les populations kabyles d'Algérie, chez lesquelles la syphilis serait endémique et héréditaire depuis un temps indéfini, tandis que l'érosion des dents est extrêmement rare. Enfin des sujets affectés d'érosion très-manifeste ont été trouvés porteurs de chancres indurés, alors que la majorité des syphiliographes se refusent à admettre qu'un sujet affecté héréditairement de syphilis puisse la contracter à nouveau.

3° Un grand nombre de sujets, chez lesquels l'observation la plus minutieuse n'a pu retrouver aucune trace de syphilis infantile, présentent l'érosion à ses divers degrés.

4° M. Parrot affirme l'existence de la syphilis aux temps préhistoriques.

Assurément ce n'est pas nous qui nous refuserons à admettre l'ancienneté de la syphilis ; mais sur quels faits raisonne M. Parrot ? Est-ce sur cette mâchoire gallo-romaine déjà signalée tout à l'heure et dont nous donnons le dessin ? (fig. 10).

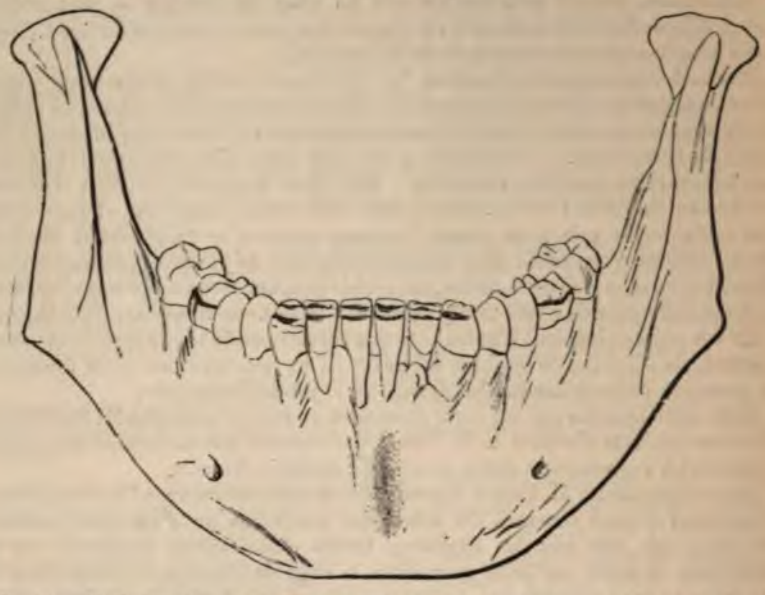

Fig. 10.—Mâchoire d'un jeune Franc de l'époque mérovingienne, et trouvée dans les cimetière de Breny (Aisne) par M. Moreau (Musée de la Société d'Anthropologie de Paris).

Est-ce sur ces pièces trouvée, dans les cavernes de l'époque de la pierre polie et représentant des dents frappée, d'érosion en étages ? (fig. 11).

Fig. 11.—Quatre dents frappée d'érosion en sillons multiples superposés et trouvées dans les dolmens de la Lozère—collection de M. le Dr. Prunières.

Elles ont en effet les sillons caractéristiques. Mais ces pièces sont muettes sur le fait de la syphilis, et nous serions tout aussi bien fondé à affirmer ici l'existence de l'éclampsie préhistorique. C'était d'ailleurs, comme on sait, l'opinion de Broca, quand il émit cette hypothèse que la trépanation crânienne, qui s'adressait aux enfants de l'âge de la pierre polie, avait pour objectif la guérison des affections convulsives. Or la présence des dents frappées d'érosion trouvées dans les mêmes gisements ne vient-elle pas singulièrement confirmer cette hypothèse ?

5° Enfin le dernier argument que nous voulons opposer à M. Parrot, argument péremptoire, c'est l'existence de la lésion qu'il qualifie de syphilitique ailleurs que chez l'homme. Voici, en effet, une mâchoire de bœuf, dont les deux pinces centrales sont frappées d'érosion symétrique.

Nous représentons (fig. 12) l'une des incisives qui présentait de la manière plus manifeste une érosion en nappe occupant le quart de la hauteur de la couronne et limité par un bourrelet d'émail.

Nous n'avons point, il est vrai, sur le premier âge de cet animal de renseignement pouvant prouver qu'il à été atteint de maladie convulsive ou autre. Nous nous bornous à conclure que cette lésion n'est pas syphilitique en ajoutant toutefois que certains états convulsifs ont été observés pendant le premier âge chez quelques herbivores, le mouton par exemple.

Tel est l'ensemble de raisons sous lesquelles tombe, suivant nous, la théorie de la syphilis dentaire.

Abordons maintenant la troisième et dernière partie de cette argumentation, c'est-à-dire la démonstration que l'érosion dentaire, dans ses formes d'*échancrure*, de *sillon*, de *nappe*, &c., est le signe rétrospectif constant de l'éclampsie infantile.

Et d'abord voici un tableau dans lequel nous avons réuni quarante observations d'érosion dentaire dans leurs rapports étroits et exclusifs avec l'éclampsie. Ce tableau présente en colonnes parallèles :

1° La lésion dentaire, son lieu d'élection, le niveau des dents affectées, soit de la dentition temporaire, soit de la permanente ;

2° Dans un autre colonne, l'âge des attaques éclamptiques, leur durée, leur intensité ;

3° Dans la dernière colonne, l'étude des antécédents et des maladies intercurrentes.

Fig. 12.—L'une des deux pinces centrales d'un bœuf Durham, âge de 4 ans.

Qu'on veuille bien étudier une à une ces quarante observations saisissantes par leur netteté et leur précision. Nous aurions pu les multiplier à l'infini, mais il nous semble qu'elles doivent suffire à la démonstration mathématique de l'équation pathologique que nous avons formulée tout à l'heure.

Ce rapport rigoureux entre l'éclampsie infantile et l'érosion est d'ailleurs pour nous un fait très-anciennement démontré. Sans parler de nos recherches personnelles antérieures, nous pouvons rappeler plusieurs monographies qui défendent la même opinion : telles sont les thèses des docteurs Quinet (de Bruxelles), Castanié et Rattier (de Paris), qui renferment des faits conformes.* Broca, ainsi qu'on sait, s'est également rallié à cette théorie.

Si nous interrogeons les auteurs étrangers, nous trouvons que Arlt, dans ses recherches sur la cataracte zonulaire et l'érosion des dents, invoque dans leur production les attaques éclamptiques ; de même Horner et Davidson, bien que moins affirmatifs. Enfin, M. Nicati n'hésite pas à rattacher à cette même cause à la fois l'érosion de la dent et la cataracte zonulaire qu'on pourrait presque nommer *l'érosion du cristallin.*

Cette opinion, appuyée aujourd'hui sur des faits scientifiquement observés, se retrouve d'ailleurs empirique et en quelque sorte banale dans tous les temps. Je fais appel ici aux médecins qui veulent bien m'écouter. Beaucoup d'entre eux, en présence d'une érosion dentaire bien constatée, n'ont ils pas posé cette affirmation : " Ce sujet a eu des convulsions dans son enfance ? " Et la mère de répondre invariablement : " Cela est vrai."

Aujourd'hui, nous ne nous bornons plus, en présence de la lésion dentaire, à affirmer l'éclampsie ; nous allons plus loin, et du niveau de cette échancrure ou de ce sillon, nous remontons à la date des crises, à leur durée, à leur intensité. Le procédé est fort simple : il est tout entier formulé dans ce tableau (No. 1).

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* Voy. Quinet, " Apropos des Dents syphilitiques," *Bulletin de l'Académie Royale de Méd. de Belgique*, t. xiii. 3e série, No. 1, 1879. Castanié, " De l'Erosion ou des Altérations des Dents permanentes à la suite des Maladies de l'Enfance," Thèse de Paris, 1874. Rattier, " Contributions à l'Etude de l'Erosion dentaire," Thèse de Paris, 1879.

TABLEAU No. 3.

Résumé analytique de Quarante Observations d'Erosion dentaire due à l'Eclampsie infantile.

Nos. d'Ordre.	Noms des Auteurs.	Sexe et Age des Sujets.	Forme et Niveau de l'Erosion.	Date et Durée des Attaques Eclamptiques.	Observations.
1	Broca, cité par Castanié (loc. cit., p. 49).	H 17 ans.	Sillon circulaire aux trois quarts de la couronne des incisives, faible aux canines.	Attaques d'éclampsie répétées à la 2e année et durant environ 2 mois.	... ...
2	Broca (loc. cit., p. 50).	H 20 ans.	Sillon circulaire d'un millimètre au milieu de la couronne des incisives, au tiers des canines.	Crises éclamptiques ayant duré 3 mois à l'âge d'un an et demi.	L'éclampsie a laissé après elle une contracture des membres supérieurs; rougeole à 7 ans; variole à 10 ans.
3	David, cité par Rattier (loc. cit., p. 41).	H 28 ans.	Sillon courbe à 2 millimètres du bord libre des incisives; sillon droit aux premières molaires.	A l'âge de six semaines, accès convulsifs durant 8 jours et compliquant une bronchite.	Aucun soupçon de syphilis.
4	Rattier (loc. cit., p. 40).	H 22 ans.	Echancrure du bord libre des incisives; érosion profonde des premières molaires.	Du 12e au 13e mois, série d'attaques éclamptiques.	A 6 ans, rougeole légère.
5	Broca, cité par Rattier (loc. cit., p. 38).	H adulte.	Erosion en nappe occupant le tiers de la hauteur des incisives centrales, le quart des latérales, le sommet des canines et la moitié des premières molaires.	Crises convulsives fréquentes et violentes pendant les 18 premiers mois de la vie.	Pas trace de rachitisme; les convulsions ont laissé à leur suite un tic de la face.
6	Quinet (loc. cit., p. 52).	H 30 ans.	Sillon simple des incisives centrales à 4 millimètres du bord libre, à 2 millimètres aux latérales, près du sommet aux canines; grosses molaires profondément cariées.	Vers l'âge de 3 ans, crises éclamptiques violentes se répétant pendant 3 jours.	Aucune autre affection infantile quelconque.
7	Quinet (loc. cit., p. 54).	H 18 ans.	Erosion en nappe des incisives centrales près du bord libre, moindre aux latérales et aux canines, très marquée aux premières molaires.	Série d'attaques convulsives du 10e au 15e mois.	Strabisme convergent et surdité consécutifs à l'éclampsie; aucun soupçon de syphilis.
8	Quinet (loc. cit., p. 57).	H 40 ans.	Sillon pointillé aux incisives centrales, moins marqué aux latérales, altération des sommets des canines; très-marquée aux premières molaires.	Attaque d'éclampsie grave au 14e mois.	... ...
9	Magitot (inédite).	H 40 ans.	Erosion en coup d'ongle des incisives centrales supérieures, des 4 inférieures; les 4 premières molaires manquent.	Eclampsie grave dans le 2e mois de la vie.	Aucun soupçon de syphilis; le sujet étant médecin s'est prêté à l'examen le plus complet.
10	Combe (inédite).	H 24 ans.	Erosion en sillon au voisinage du bord libre des incisives; rien aux canines; altération profonde des premières molaires.	Série d'attaques convulsives dans le premier mois de la vie.	Aucun soupçon de syphilis; le sujet a un frère aîné et une sœur cadette très-bien portants, sans érosion.
11	Castanié (loc. cit., p. 50).	F 20 ans.	Erosion en sillon à 2 millimètres du bord libre aux incisives centrales, à 1 millimètre aux latérales et aux canines; au voisinage du collet aux premières molaires.	Série d'attaques éclamptiques occupant une période de 6 mois, du 18e au 34e mois.	Aucun soupçon de syphilis.
12	Castanié (loc. cit., p. 51).	F 16 ans.	Erosion en échancrure du bord libre des incisives centrales supérieures, des 4 incisives inférieures; rien aux latérales supérieures; érosion très-marquée aux premières molaires.	Une seule série d'attaques éclamptiques au 9e mois et mettant pendant plusieurs jours la vie en danger.	Aucune autre affection infantile quelconque jusqu'à l'âge de 9 ans.
3	Rattier (loc. cit., p. 39).	F 17 ans.	Erosion en dentelures du bord libre, et sillon de la couronne aux incisives des deux mâchoires; sillon circulaire aux premières molaires.	Au 6e mois, attaque d'éclampsie grave; du 12e au 18e mois, série d'attaques répétées et moins graves.	... ...

TABLEAU No. 3—*continué.*

Nos. d'Ordre.	Noms des Auteurs.	Sexe et Age des Sujets.	Forme et Niveau de l'Erosion.	Date et Durée des Attaques Eclamptiques.	Observations.
14	Magitot (inédite).	F 15 ans.	Erosion en échancrure du bord libre des incisives supérieures centrales, des 4 inférieures; rien aux latérales supérieures; érosion profonde des molaires au voisinage de la face triturante.	Série d'attaques convulsives au 3e mois et ayant duré 24 heures.	Aucune autre affection infantile quelconque jusqu'à l'âge de 7 ans.
15	Magitot, traité des anomalies, p. 270.	F 9 ans.	Aucune altération des dents temporaires restantes; érosion en nappe avec amincissement extrême au voisinage du bord libre des incisives; érosion très-marquée aux molaires.	A un an, série d'attaques éclamptiques graves.	Aucune autre affection infantile quelconque.
16	Magitot, eod. loc., p. 271.	H 14 ans.	Erosion en sillon simple de 1 millimètre de hauteur au milieu de la couronne des incisives centrales, plus rapproché du bord libre aux latérales, près du sommet aux canines; sillon aux molaires.	Au 15e mois, série d'attaques éclamptiques durant de 12 à 15 jours.	Aucune autre affection infantile quelconque.
17	Magitot, eod. loc., p. 270.	H 20 ans.	Sillon très-fin et de niveau identique au milieu de la couronne des 4 canines; les deuxièmes molaires sont détruites par carie; les autres dents sont normales.	A 8 ans, méningite grave avec accès convulsifs pendant 15 jours environ.	Aucune affection infantile quelconque avant cette époque.
18	Magitot (inédite).	H 13 ans.	Sillon simple au milieu des incisives centrales, au tiers des latérales, au quart des canines; sillon aux premières molaires.	Au 13e mois, série d'attaques éclamptiques durant 1 mois.	Aucun soupçon de syphilis; aucune autre affection infantile jusqu'à 12 ans, époque d'une rougeole.
19	"	H 18 ans.	Sillon pointillé vers le tiers des incisives centrales, au quart des latérales, près du sommet, aux canines, au milieu des premières molaires.	Trois séries d'attaques éclamptiques durant plusieurs jours: la première au 7e mois, et les autres à une semaine d'intervalle.	Aucun soupçon de syphilis; famille nombreuse très-bien portante; frères et sœurs plus jeunes, n'yant jamais été malades et ne portant pas trace d'érosion.
20	"	F 20 ans.	Sillon large avec bourrelet très-saillant au milieu de la couronne des incisives, moins élevé aux latérales et aux canines; les premières molaires manquent.	Série d'accès convulsifs du 8e au 20e mois.	... ...
21	"	H 24 ans.	Erosion portant exclusivement sur la surface triturante des premières molaires qui sont couvertes d'aspérités aiguës. (V. tableau n° 2, fig. 1.)	Accidents graves de la mère pendant les derniers temps de la grossesse; accouchement au 8e mois, suivi de mort, quelques heures après la délivrance (éclampsie?)	L'enquête au sujet de la mère n'a pu été poussée plus loin. Pour le sujet, aucune affection infantile quelconque; aucun soupçon de syphilis héréditaire, ainsi qu'il résulte de l'examen très-approfondi des antécédents; pas de traces de cicatrices cutanées sur aucun point du corps.
22	"	H 9 ans.	L'érosion a le caractère d'un sillon simple siégeant à la partie moyenne des canines supérieures temporaires, persistantes, d'une échancrure sur le bord libre des incisives centrales supérieures et sur les 4 inférieures. Aux molaires, elle occupe la face triturante qui est rugueuse et mamelonnée. (V. tableau n° 2, fig. 2.)	Une première série d'attaques éclamptiques s'est produite au début du 2e mois et a duré trois jours avec dysurie et même rétention complète nécessitant le cathétérisme. Cette première attaque a été suivie de cinq autres se produisant au début de chacun des mois suivants avec le même caractère et la même durée.	Aucune affection quelconque jusqu'à l'âge de 10 ans; parents très-bien portants, sans aucun soupçon de syphilis; pas de fausse couche de la mère; le sujet a un frère cadet âge de 7 ans qui n'a jamais été malade et n'a aucune érosion sur les premières molaires qui sont en place, ni sur les incisives centrales supérieures qui ont achevé leur sortie.

Tableau No. 3—*continué.*

Nos. d'Ordre.	Noms des Auteurs.	Sexe et Age des Sujets.	Forme et Niveau de l'Erosion.	Date et Durée des Attaques Eclamptiques.	Observations.
23	Magitot.	F 18 ans.	Erosion en échancrure du bord libre des incisives centrales supérieures et des 4 inférieures; sillon simple des premières molaires, immédiatement au-dessous de la surface triturante; deux de ces molaires sont détruites par la carie consécutive à l'érosion. (V. tableau nº 2, fig. 3.)	Au 3e mois, séries d'attaques éclamptiques ayant duré 24 heures avec menaces d'asphyxie; état grave; imminence de mort.	Aucune affection intercurrente jusqu'à l'âge de 7 ans; aucun soupçon de syphilis.
24	,,	F 15 ans.	Exagération de l'état précédent; érosion en échancrure profonde du bord libre des incisives centrales supérieures, des 4 inférieures, très-marquée aux centrales, moindre aux latérales; rien aux incisives latérales supérieures ni aux canines des deux mâchoires. (V. tableau nº 2, fig. 4.)	Au 6e mois de la vie, série d'attaques convulsives ayant duré 24 heures.	Aucune affection intercurrente de la première enfance, jusqu'à l'âge de 7 ans, époque d'une rougeole bénigne; aucun soupçon de syphilis.
25	,,	F 18 ans.	Erosion en sillon pointillé au tiers supérieur des incisives centrales, au quart inférieur des latérales, au cinquième des canines et à la moitié de la première molaire gauche; même niveau aux dents antéro-inférieures; les 3 autres molaires sont détruites par la carie consécutive à l'érosion. (V. tableau nº 2, fig. 5.)	Attaques d'éclampsie très-graves survenues au 7e mois en trois séries avec intervalle de 2 ou 3 jours, l'ensemble des crises ayant duré conséquemment environ 9 jours.	Aucune autre affection infantile; aucun soupçon de syphilis. La famille, composée de: père, 50 ans; mère, 40 ans, et 3 enfants: 1º celui qui fait l'objet de l'observation; 2º F., 16 ans, ayant eu à 10 ans une rougeole simple; 3º H., 12 ans, ayant eu à 2 ans une fièvre typhoïde très-grave ayant duré 6 semaines. Chez ces deux derniers, aucune érosion. La famille, habitant la campagne, a été observée de tout temps par le médecin de la localité, qui donne sur la question de la syphilis les assurances les plus formelles.
26	,,	F 21 ans.	Erosion en double sillon au tiers inférieur des incisives centrales supérieures, en sillon unique près du bord libre des latérales, en double sillon aux incisives inférieures, et au quart supérieur, en nappe profonde à la partie moyenne des 4 premières molaires; rien aux canines dont l'angle est cependant émoussé. (V. tableau nº 2, fig. 6.)	Deux séries d'attaques éclamptiques: la première survenue au 6e mois est caractérisée par dix attaques se produisant pendant 24 heures suivies de coma et laissant après elles divers désordres de la santé. La seconde apparaissant au 9e mois à peu près de même nature et de même durée totale.	Aucune affection infantile quelconque jusqu'à l'âge de l'éruption. Une enquête très-minutieuse ne permet aucun soupçon de syphilis. Le sujet a deux frères plus jeunes que lui, dont l'un n'a jamais été malade dans son enfance et l'autre a eu une scarlatine grave à 3 ans. Aucun d'eux n'a d'érosion.
27	,,	F 20 ans.	Erosion en nappe des douze dents antérieures et des quatre molaires, sauf les deux incisives inférieures gauches où la lésion a le caractère d'échancrure par suite de la fracture du bord libre. (V. tableau nº 2, fig. 7.)	A 4 mois, série d'attaques convulsives dont l'ensemble a duré 18 heures. Etat très-grave, menaces d'asphyxie; à la suite, contracture permanente des deux membres du côté gauche; réapparition 2 jours après d'une nouvelle série d'attaques durant 15 jours et caractérisées par 2 ou 3 attaques par jour suivies de coma; troubles de l'intelligence jusqu'à l'âge de 18 mois où la santé se rétablit.	Aucun soupçon de syphilis ainsi qu'il résulte de l'examen approfondi de la famille: père et mère vivant encore, frère cadet très-bien portant, sans érosion.
28	,,	F 20 ans.	Erosion en nappe occupant à la mâchoire supérieure à peu près la moitié de la hauteur des incisives centrales, le tiers des latérales, et le sommet des canines. A la mâchoire inférieure le tiers de la hauteur des incisives centrales, et le quart des latérales, le sommet des canines; les quatre premières molaires sont détruites par la carie. (V. tableau nº 2, fig. 8.)	Nombreuses séries d'attaques éclamptiques comprenant dans leur ensemble la période entre le 8e et le 36e mois, et consistant en crises graves suivies de coma durant de 4 à 6 heures.	Le sujet a présenté dans son enfance des signes certains de rachitisme; mais l'examen approfondi de la famille composée de père et mère, 2 sœurs aînées et un frère cadet, tous vivants et bien portants. L'état de ce sujet a été attribué à une variole grave de la mère pendant sa grossesse.

Tableau No. 3—*continué.*

Nos d'Ordre.	Noms des Auteurs.	Sexe et Age des Sujets.	Forme et Niveau de l'Erosion.	Date et Durée des Attaques Eclamptiques.	Observations.
29	Magitot.	F 22 ans.	Erosion consistant en un sillon unique siégeant au même niveau vers la partie moyenne des quatre canines. (V. tableau nº 2, fig. 9.)	A l'âge de 2 ans et sans aucune affection antérieure, méningite grave ayant mis en danger pendant 3 semaines les jours de l'enfant.	Aucune autre affection infantile quelconque; aucun soupçon de syphilis.
30	,,	F 15 ans.	Erosion consistant en un sillon unique siégeant vers la partie moyenne des 4 premières molaires; rien à aucune autre dent. (V. tableau nº 2, fig. 10.)	A 3 ans et 4 mois, série d'attaques convulsives ayant duré 56 heures et suivie d'un coma pendant 10 jours avec perte de connaissance.	Aucune autre affection infantile; aucun soupçon de syphilis.
31	,,	H 17 ans.	Sillon simple vers le tiers des incisives centrales, au quart des latérales, au cinquième des canines; sillon au milieu des molaires.	Au 18e mois, méningite avec 3 jours de gravité et convulsions.	... ...
32	,,	F 14 ans.	Erosion en nappe de 1 millimètre 1/2 de hauteur aux incisives, aux canines, et aux premières molaires.	Série d'attaques convulsives du 10e au 18e mois.	Aucune autre affection infantile quelconque.
33	,,	F 18 ans.	Erosion en sillon pointillé au milieu des incisivescentrales supérieures et des 4 inférieures, près du sommet des canines, et vers le milieu des premières molaires.	Une seule série d'attaques éclamptiques avec coma profond durant 4 heures.	Aucun soupçon de syphilis; parents, frère aîné et frère cadet bien observés et ne présentant aucune trace morbide.
34	,,	F 22 ans.	Sillon de 1 millimètre de largeur au milieu des incisives centrales, au tiers des latérales, au quart des canines. Les molaires manquent.	Au 13e mois, série d'attaques éclamptiques occupant une période de 3 semaines.	Observation très-minutieuse de la famille; aucun soupçon de syphilis.
35	,,	H 25 ans.	Erosion en sillon pointillé tout à fait au-dessous du bord libre des incisives centrales supérieures, des 4 inférieures; rien aux latérales supérieures ni aux canines; érosion profonde des molaires.	Dans le cours du 4e mois, deux séries d'attaques convulsives le même jour et durant ensemble 24 heures.	Ce jeune homme, qui est fils de médecin et étudiant en médecine, donne sur ses antécédents les renseignements les plus précis qui éloignent toute idée de syphilis.
36	,,	F 18 ans.	Erosion en échancrure du bord libre des incisives centrales supérieures, des 4 inférieures; sillon au tiers des premières molaires.	Au 6e mois, une seule série d'attaques convulsives ayant duré 4 heures	Aucune autre affection infantile, quelconque; aucun soupçon de syphilis.
37	,,	F 20 ans.	Erosion en nappe du tiers de la couronne des incisives centrales, au quart des latérales; sillon annulaire au sommet des canines; les molaires manquent.	Au 4e mois, série d'attaques convulsives durant 3 semaines et laissant à sa suite des accidents divers, graves, jusqu'au 18e mois.	L'examen très-attentif de la famille et de deux frères plus jeunes ne permet de retrouver aucune trace de syphilis héréditaire.
38	,,	H 11 ans.	Erosion en escalier comprenant 6 ou 8 sillons vagues superposés aux incisives, aux canines, et et aux premières molaires.	Au 6e mois, début d'une série d'attaques éclamptiques durant plus de 6 mois à 8 à 10 à chaque série; coma consécutif.	Ces deux sujets qui sont frères appartiennent à une famille composée de père et mère très-robustes et de six enfants: deux qui font l'objet de ces observations et quatre autres n'ayant jamais été malades et ne présentant aucune érosion; aucun soupçon de syphilis héréditaire.
39	,,	H 8 ans, frère du précédent.	Erosion en sillon des 2 incisives centrales supérieures; les latérales manquent; sillon aux 4 incisives inférieures, et aux 4 premières molaires.	Série d'attaques éclamptiques au 7e mois de la vie.	
40	,,	H 25 ans, étudiant en médecine.	Erosion en échancrure du bord libre des incisives centrales supérieures, des centrales et latérales inférieures, du sommet des canines et d'une certaine étendue des premières molaires.	Au 6e mois, à la suite d'une chute d'un 1er étage sur la tête, série d'attaques convulsives durant 5 jours.	Le sujet, très-préoccupé des opinions qui rattachent l'érosion dentaire à la syphilis héréditaire, se prête au plus minutieux examen des antécédents et de l'état actuel; aucun soupçon de syphilis.

On voit donc sur quel ensemble de considérations repose la théorie que M. Parrot désigne lui-même sous le nom de *théorie des dents éclamptiques;* mais il nous reste cependant une dernière tâche, celle de répondre en quelques mots à plusieurs objections, fort sérieuses en apparence, que nous a posées M. Parrot.

Ces objections se réduisent aux deux suivantes :

1° L'éclampsie infantile est incapable, dit-on, de produire l'érosion dentaire.

2° Tout sujet porteur d'érosion présente des traces de syphilis héréditaire.

Examinons brièvement ces deux points :

Une attaque d'éclampsie, n'ayant qu'une durée de quelques minutes, ne peut interrompre la formation des tissus de l'émail et de l'ivoire pendant un temps suffisant pour produire une trace indélébile, et M. Parrot ajoute qu'en donnant, par exemple, à une attaque la durée d'une minute, il faudrait, pour produire certaines érosions en nappe, un nombre énorme de convulsions, 42,000 ; 600,000.

Assurément, si nous considérons l'attaque éclamptique en elle-même, nous reconnaissons son impuissance à produire un tel résultat. Mais ne sait-on pas que cette attaque convulsive n'est que la manifestation extérieure, le reflet d'un état profondément troublé du système nerveux et de la nutrition générale ? On n'en saurait douter en considérant que l'éclampsie, quand elle n'amène pas la mort, a parfois laissé à sa suite des lésions graves telles que la perte de l'intelligence, le strabisme, la surdi-mutité, le bégaiement, des contractures des membres, &c.

D'autre part, on sait que l'éclampsie ne se manifeste jamais par une seule attaque, mais par des séries plus ou moins nombreuses et plus ou moins rapprochées, de sorte que l'influence perturbatrice correspond, non à l'attaque isolée, mais à la série. Or, cette série, d'après maintes observations, s'étend parfois à dix, quinze, vingt heures, chaque attaque réapparaissant toutes les demi-heures ou toutes les heures.

Un auteur récent, Kien (de Strasbourg), cite le cas d'un enfant qui a présenté une série d'attaques se reproduisant toutes les heures pendant quatorze heures et suivies d'une période de coma durant six jours, et interrompue encore par des crises plus faibles et plus rares. C'est donc une période de perturbation grave de la nutrition ayant duré chez cet enfant près de sept jours, pendant lesquels on a noté le pouls à 140 et la température à 40°.*

On voit donc que les troubles de nutrition, sous l'influence de l'éclampsie, peuvent durer un temps fort long. Nous possédons des faits dans lesquels ils se sont prolongés pendant des mois et des années.

Mais ce n'est pas tout, et nous avons voulu établir expérimentalement quelle quantité de dentine et d'émail se produit normalement pendant un temps donné dans un follicule et par suite quelle étendue et quelle profondeur présentera la zone de tissu frappée d'interruption.

Voici l'expérience :

" Sur un chien de quinze jours, allaité par sa mère, nous avons enlevé par une incision légère le chapeau de dentine de la canine gauche. L'animal est aussitôt remis à sa niche et continue de téter sans trouble apparent.

" Dix jours plus tard, à la même heure, le chapeau de dentine de la canine droite est extrait à son tour. Ces deux préparations sont lavées et séchées. Les voici sous une petite cloche de verre. Elles présentent une différence de hauteur qui se chiffre par 6 millimètres ; le petit chapeau ayant 6, le grand 12 millimètres."

Six millimètres en dix jours nous donnent 6 dixièmes de millimètres par jour. Par heure, nous obtenons 20 millièmes de millimètres plus une fraction. Négligeons cette fraction et faisons le calcul suivant :

Si une cause morbide était venue interrompre dans ce petit chapeau la

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* *Gazette Médicale de Strasbourg*, 1880, No. 7, p. 73.

formation de l'ivoire et de l'émail pendant seulement cinq heures, le sillon produit aurait plus d'un dixième de millimètre de largeur, c'est-a-dire qu'il serait visible à l'œil nu.

Voici sur des moulages pris sur le vivant des sillons de cette étendue et dus à des séries relativement courtes d'attaques éclamptiques. Voir dans le tableau No. 2, les figures 1, 6, 9, et 10.

Poursuivons ce calcul, et nous verrons qu'une série de crises durant dix heures donnera un sillon de deux dixièmes de millimètre, vingt heures quatre dixièmes, et ainsi de suite.

Qu'on juge dès lors de l'étendue d'une érosion sous l'influence d'un état morbide durant plusieurs mois.

Mais ce n'est pas tout, et M. Parrot s'étonne que certaine dents ne soient jamais atteintes, la seconde et la troisième molaire par exemple, et il trouve la théorie de l'éclampsie incompatible avec ce fait.

Le tableau No. 1 publié ci-dessus me paraît répondre complètement à cette objection. On y voit en effet que le chapeau de dentine de la seconde molaire n'apparaît qu'à $3\frac{1}{2}$ ans en moyenne; or il est notoire que l'éclampsie devient fort rare à partir de cette époque. On sait même que la plupart des médecins en attribuent, empiriquement il est vrai, la production à la première dentition qui est achevée à cet âge. Quant à la troisième molaire ou dent de sagesse l'apparition de son chapeau à la 12e année la met hors de cause.

C'est en raison de ces rapports chronologiques entre l'évolution des dents et l'âge d'élection de l'éclampsie que la première molaire permanente se trouve constamment frappée à des degrés ou à des niveax divers. Son chapeau de dentine débutant, en effet, au sixième mois de la vie intra-uterine tandis que son éruption n'a lieu qu'à la sixième année, elle est ainsi soumise pendant plus de six ans à toutes les perturbations morbides qui atteignent la première enfance.

L'érosion de cette première molaire en particulier porte en outre très souvent à la surface triturante elle-même ce qui implique un trouble fœtal. Il en est de même de l'érosion des dents temporaires, et M. Parrot nous demande d'expliquer ici une telle lésion. Croyons nous à l'existence de l'éclampsie intrautérine?

Nous avouerons à cet égard notre ignorance complète sur ce point de pathologie fœtale. Mais il est fréquent de voir des accidents ou des maladies maternels retentir assez gravement sur l'embryon pour en troubler la nutrition et jouer ainsi à cette période des influences si faciles le rôle que joue l'éclampsie dans le première enfance.

Dans un cas de ce genre où les premières molaires étaient seules atteintes d'érosion (voy. tableau 2, No. 1), l'étude des antécédents nous a permis de retrouver dans l'histoire de la mère des accidents graves de la grossesse survenus vers le 7e et le 8e mois, amenant l'accouchément prématuré au 8e mois et la mort dans les premières heures qui suivirent. Toutefois la question posée de l'éclampsie maternelle n'a pas reçu de solution par insuffisance des souvenirs.

Pour les dents temporaires, on sait qu'elles sont très rarement frappées d'érosion, et dans une de nos observations (obs. 22, tableau No. 2), le mécanisme en a été facilement reconstitué. Mais si M. Parrot croit l'avoir trouvée frequemment c'est qu'il a pu, ce nous semble, confondre avec l'érosion des altérations cadavériques.

On reconnaît en effet que si l'on enlève, dans un follicule clos un chapeau de dentine qu'on abandonne à la dessication spontanée, l'émail incomplètement calcifié se couvre de félures de craquelure par la dissociation et la destruction des prismes. Cet accident simule parfaitement l'érosion et nous l'avons reconnu sur plusieurs des pièces que M. Parrot a présentées l'année dernière au Congrès de Reims.

Arrivons enfin à la dernière objection de M. Parrot, la voici:

Tout sujet porteur d'érosion dentaire présente des vestiges de syphilis

héréditaire. Nous renverrons pour toute réponse à l'analyse des quarante observations que nous venons de résumer. On y trouvera un grand nombre de relations dans lesquelles l'examen le plus minutieux des parents, des frères et sœurs, et du sujet lui-même, n'autorise aucun soupçon de syphilis. La publication détaillée des observations de ce genre qui composent notre dossier personnel allongerait indéfiniment ce travail et n'apprendrait rien de plus que les résumés que nous en avons donnés.

Conclusions.—De l'ensemble des documents et des considérations qui précèdent, nous déduirons les conclusions suivantes :

1° L'éclampsie infantile produit constamment dans la nutrition intra-folliculaire une perturbation qui se traduit par une des lésions caractéristiques de l'érosion dentaire.

2° Le niveau, le nombre et l'étendue de la lésion de la couronne correspondent à l'époque, à la durée et à l'intensité des crises éclamptiques ; l'érosion est une.

3° Les autres maladies de l'enfance, fièvres éruptives, affections catarrhales, intestinales, &c., sont impuissantes à produire l'érosion. Toutefois quelques maladies graves et de très-longue durée peuvent amener une désorganisation totale de la couronne des dents en voie d'évolution, mais non l'érosion proprement dite.

4° La syphilis héréditaire, dont on ne peut nier l'influence sur la constitution générale des tissus osseux et dentaires en voie de formation ne se traduit jamais par les lésions caractéristiques de l'érosion.

Et comme corollaire :

Tout sujet, notoirement affecté de syphilis héréditaire, ne présentera jamais d'érosion que si, dans ses antécédents, on retrouve simultanément la trace de l'éclampsie.

5° L'érosion dentaire constatée dans les gisements préhistoriques serait, d'après les raisons invoquées par Broca et par nous-mêmes, la preuve de l'éclampsie infantile, laquelle constituait le but déterminant de la trépanation crânienne.

6° L'érosion dentaire, dans ses formes les plus caractéristiques, n'est pas exclusive à l'homme et peut se rencontrer chez des animaux domestiques que la syphilis n'atteint pas.

DISCUSSION.

Mr. Moox, London: In discussing this subject, it may be taken for granted that all medical men will admit that hereditary syphilis is very apt to be productive of a deformity of certain among the permanent teeth; but there are not a few in the ranks of medicine and surgery who consider that such malformation may be due to other causes besides syphilis. Now this doubt is the outcome, I believe, of a widespread want of distinctness that prevails as to what a typical syphilitic tooth really is. Mr. Jonathan Hutchinson having, years ago, shown the way, it of course is comparatively easy for others to follow out details of this subject. The diagnostic value of a syphilitic tooth depends greatly on this fact—viz., whether such malformation is distinctive and due to syphilis alone, or whether other causes may bring about the like malformation? Some years ago I collected cases and took models of a large number of cases bearing on this subject, and became thereby convinced that there is a special kind of malformation which may be depended on as indicating hereditary syphilis and nothing else. In order to properly appreciate divergences from the normal form of teeth, it is necessary that some features of what may be called the architecture of a normal tooth should be known. To idate this subject and to lessen the probability of my remarks wearying

you, I have brought down some models and put up some drawings which will, I hope, clearly bring out the normal form of syphilitic teeth, and the shape of such teeth as are apt to be confused with the latter.

A description was then given of the differences between normal and syphilitic teeth, and the modes of distinguishing the latter from other malformed teeth. The different kinds of defective enamel formation were then touched on, and the question suggested that perhaps "mercurio-syphilitic tooth" might be the right term for certain teeth which presented a want of enamel over a semi-lunar space in the centre of the first-formed portion, leading often to the breaking down of the unprotected dentine and the formation of a semi-lunar notch.

Mr. C. S. Tomes, London: exhibited a model of teeth presenting in a marked degree M. Magitot's "erosion," remarking that the history of the patient was perfectly known to him, and that no attack of convulsions had ever occurred. The patient had, however, suffered from a severe attack of inflammatory croup at such an age as to exactly correspond with the lesion of the teeth. During this attack she was salivated.

Mr. Coleman, Streatham: It was my good fortune to be a colleague of Mr. J. Hutchinson at the period when that gentleman's attention was first directed to this subject. I thus had the opportunity of seeing a considerable number of Mr. Hutchinson's earliest cases, and to his first communication—viz., that to the Pathological Society—was appended a report by me of th peculiarities such teeth presented. I confess I was in the first instance a sceptic with regard to the syphilitic origin of these teeth, but as time wore on I can only come to the conclusion of the correctness of Mr. Hutchinson's views. My observations have now been extended over some twenty years, and from holding hospital appointments I have had large opportunities of inspecting the teeth of all classes, and my firm and settled conviction is, that between cause and effect there cannot in my mind be the least doubt. Persons the subjects of inherited syphilis may have well-formed and excellent teeth, but in my belief no person can have the type of teeth described by Mr. Hutchinson who has not inherited syphilis.

Dr. Blache, Paris: In a paper which I published in 1879 on "Malformation of Teeth in Children as a Symptom of Hereditary Syphilis," I quoted five instances of cases having reference to the first dentition. In taking part in the interesting discussion opened to-day, I claim to contribute towards the establishment of the fact of "erosion" taking place in the milk-teeth, not only in cases of syphilis or in the children of syphilitic parents, but sometimes also quite independently of this condition. Thus I have had under observation for four years a child at whose birth I was present, and whose father and mother I know and believe to be absolutely free from syphilitic disease. This child has marks of "erosion" on its lower incisor teeth and on the two canines, which I cannot refer either to syphilis or to those convulsions which my friend and colleague M. Magitot deems to be the origin of this affection.

Dr. Hayward, Liverpool: I wish to contribute a practical case. The parents were perfectly healthy, having six quite healthy children; after this the father had occasional intercourse with a lady with tertiary syphilis during daytime, and at night with his own wife. After this twins were born; both with very definite signs of inherited syphilis, one dying within three months, the other struggling through, but absolutely without mercurial treatment. Its primary teeth were black and soft, the permanent incisors some defective of dentine, with small

cutting edges and large at the neck, and others defective of enamel and showing honeycomb appearance. There was scarcely any medicinal treatment, and certainly not mercurial. The child never had convulsions or any similar disease; only a great liability to bronchitis. This would show marked teeth originating without nervous disease or mercury, but certainly from syphilis; this view being further supported by the fact that both parents afterwards had cutaneous syphilis.

Dr. DALLY, Paris: It is many years since M. Magitot first acquainted me with his views on the origin of erosions of the teeth. Since then I have made extended inquiries in my out-patient practice amongst the children whom I attend for deformities of the organs of locomotion, and my inquiries have very rarely failed to elicit a history of convulsions. At the same time I would add that I have never been able to entirely exclude the suspicion of syphilis. The difficulty is that people are ready enough to admit convulsions, but will always endeavour to conceal syphilis. Why must I exclude one of these two causes? Because hereditary syphilis does not cause disturbances of the nerve centres? Convulsions are only a symptom. There is some morbid cause behind them, and do we know of any *causa morborum* more active, more profound, and more protean than syphilis? Erosion of the teeth is evidence of a violent and passing disturbance of general nutrition. True; and the general conclusion which, as it appears to me, is to be deduced from this discussion is, that all the causes which are capable of interfering with general nutrition—accidental and physiological causes included—are capable of producing erosions of the teeth.

Mr. HUTCHINSON, London: I avail myself with some pleasure of the opportunity which this debate affords me of attempting to correct some misapprehensions in reference to the clinical symptology of the teeth. M. Magitot makes the denial that honeycomb teeth or eroded teeth are indicative of inherited syphilis one of the main points of his paper, and he seems to think that in saying this he is controverting an opinion of mine. But in point of fact we are in accord, for this is what all along I have most carefully asserted. The honeycombed eroded teeth have always for me been indicative of infantile stomatitis (mercurial or other), and never of syphilis. I am aware that mistakes have been made, and that others besides M. Magitot have supposed that this condition of erosion was what I had described as indicating syphilis. Thus, for instance, most of the casts which M. Parrot shows in our museum as syphilitic are not what I should regard as syphilitic at all, but simply "mercurial." This error has been the more frequent because, as a fact, we very frequently—indeed I may say usually—meet with the malformations due to syphilis and those due to stomatitis in the same mouth; for most syphilitic children have taken mercury in infancy. But I cannot myself plead guilty to any responsibility for such mistakes, for both in pictorial representation and description, I have always carefully noted the differences and distinguished the two conditions. Even where the two are co-existent there is no real difficulty in discrimination. Permit me to repeat what I have said over and over again on former occasions, that the dental malformations which denote syphilis are not of the nature of erosion, but consist in very peculiar arrests of development. The only teeth to which I venture to attach much importance are the upper central incisors of the permanent set. In these an arrest in the growth of the middle denticle, leaving a single central notch, is the commonest and most trustworthy condition, but, in addition to it, there is usually a dwarfing of the tooth in all its dimensions, and sometimes a

screw-driver form of tooth is almost as characteristic as a notched one. The defects are usually symmetrical, but sometimes not so. Peculiarities are often to be observed in the other teeth, and are often of much help in corroboration of diagnosis, but in the absence of the peculiarity described in the upper central incisors they are not to be trusted, and for fear of occasioning more mistakes I prefer not to allude to them further. Whilst these malformations of the upper central incisors (notches and dwarfing) are the characteristics of syphilitic teeth, those which imply former stomatitis consist chiefly in defective formation of the enamel. The teeth which show these most constantly are the first permanent molars, the two bicuspids being remarkable for their exemption. The explanation of this is easily given if we remember the dates of the calcification of the several teeth. The first permanent molars may be regarded as the test teeth as regards infantile stomatitis, just as the central incisors are for syphilis, but they are by no means the only teeth affected. Next to them in importance come the incisors, which are almost as constantly pitted, eroded, and of bad colour, often showing a transverse furrow which crosses all the teeth at the same level. M. Magitot, if I have understood him correctly, inclines to the belief that these enamel defects are the consequences not so much of stomatitis as of some interference with development brought about through the nervous system in connection either with infantile convulsions or severe illness in early life. He explains them as we explain the furrows on the nails, so often seen as the record of past disturbances of health. It may be that to some extent he is right. I think it very probable that he is. I have myself long ago published facts bearing upon this very point, and have discussed in some detail the question as to whether, in the cases of enamel defects, so frequently seen with lamellar cataract, and in those who have had convulsions, the latter ought to be regarded as the direct cause of the former. The conclusion to which I have arrived is, that even in cases in which defects are in association with neurosis, still there almost invariably intervenes a stage of inflammation which is the immediate cause of the defects. So it is very possible that some stomatitis teeth may be caused by mercurial stomatitis or that of thrush, and that others may have been induced by a stomatitis due to nervous influences. Still, in both the local process which causes the defect is inflammation. Unquestionably, in the case of the nails, local diseases, certainly inflammatory eczema, &c., produce markings just like those which follow general illness. We are now, Mr. President, I think, in a fair position to discuss the questions, Are the teeth which have been described as syphilitic really due to syphilis? are those which are assigned to stomatitis really in association with that cause? The evidence on the latter point is of a far less conclusive character than that as to the former. I have seen many sets of so-called mercurial or stomatitis teeth in which I could get no evidence that mercury had been given, or that any form of infantile illness had been experienced likely to be attended by stomatitis. Still, although not uncommon, this class of facts has been, in my experience, exceptional, and those which have illustrated what I believe to be the rule have been far more abundant. About the time that we were first noticing syphilitic teeth, and were busy examining the mouths of our young patients, the observation was made that in most cases of infantile cataract the teeth were very defective. I do not know whether Professor Horner of Zurich, Mr. Teale of Leeds, or myself first noted this point, but we were all at work on it together. To Professor Horner, I believe, belongs the credit of having drawn attention to the fact that the kind of infantile cataract usually so attended was almost always "lamellar," and that there was usually also a history of convulsions in infancy. Then comes the problem as to the mutual relationship between these three conditions—convulsions in infancy, lamellar cataract, and defectively

developed teeth. Upon this I wrote a paper which was published in the Pathological Society's *Transactions* in 1875.* I must not weary you by recapitulating the arguments of that paper, more especially as it is accessible to all who feel sufficient interest in the subject to refer to it. Briefly, my conclusion was, that the cataract was due to the convulsions, and the dental defect to the mercury given for the convulsions. It may easily be seen that I was to some extent wrong on this latter point, and I am quite willing to join with M. Magitot in the suspicion that the illness coincident with the convulsions may, in some cases without the help of mercury, produce defects in the enamel, &c. It is clear that we have here an interesting field for further observation. Let those who have the opportunities for knowing the facts accurately, that is, without trusting to hearsay evidence, tell us whether convulsions without mercurial treatment and without any form of stomatitis are often followed by defective teeth. Conversely let those who possess facts as to children who, during the first year, have taken much mercury (as "teething powders" or otherwise), tell us whether in such defective development of the enamel is ever absent in the permanent teeth. There must be plenty of facts of both classes extant if we could only get at them. That mercury given in infancy is one of the causes of eroded teeth I have not the least doubt. Many very convincing facts have occurred within my own observations. My friend Mr. Moon and others have also carefully investigated cases of this kind, and have arrived at the same conclusion. Here, then, I leave this part of our subject; it requires further work, but the main conclusions which I have formerly published are, I still believe, well sustained. Respecting the so-called "syphilitic" teeth, I am not prepared to admit the slightest doubt or hesitation. It is now more than twenty years since I submitted to the Pathological Society, and through it to the profession, the evidence which I had obtained on this point. Since then I have made daily use of these conditions as an aid in diagnosis, and their value has been acknowledged by a host of independent observers in all parts of the world. Indeed I may, I think, without presumption, claim that considerable advances in clinical knowledge have been made in various departments by their aid. Almost simultaneously with their recognition came the observation that "interstitial keratitis," when well marked and symmetrical, may be regarded as certainly syphilitic. It had formerly been always called "scrofulous." Choroiditis disseminata, a peculiar form of deafness, and a special form of rapidly destructive "lupus," were also soon afterwards assigned with confidence to their proper category. More recently, partly by the evidence of such affections as those just mentioned, but more particularly by that given by the teeth, various observers—Dr. Hughlings Jackson, Dr. Barlow, Dr. Allen Sturge, Dr. Down, and others—have recognised rare forms of disease of the nervous system as certainly due to inherited taint. A large group of bone diseases have in like manner, and on like evidence, been transferred from struma and rickets to syphilis. I produced yesterday, at the London Hospital, for M. Magitot's inspection, about a dozen examples of well-marked syphilitic teeth, occurring in association with one or other, or with several of the maladies I have enumerated. Neither these maladies nor the teeth which have helped us to their recognition are, in certain departments of practice at all uncommon; and I repeat, our clinical knowledge of the one rests to a large extent upon the trustworthiness of the other. Such being the case, and these facts having been in the main recognised, not only in England but over the Continent and in America, I submit that Dr. Magitot is a little late in the day when he comes to tell us that these malformations are not indications of inherited syphilis but of convulsions. I hope I shall not be

* *Transactions of the Pathological Society of London*, vol. xxvi. p. 235.

considered over-sensitive if I admit to a feeling that it is a little hard to be called on now, after my conclusions have been so long admitted, to again go over the old ground of proof. I should have no right to resent this if it came from one who had studied my views, and was prepared with facts to refute them. Before I sit down, sir, let me fully admit that it is not always that the teeth of the subjects of inherited syphilis reveal the diathesis. In very many cases they are not malformed at all. In many others the malformations are too slight or too much mixed to be worthy of confidence, though still possibly very valuable by way of corroboration. The frequent coincidence of syphilitic with mercurial peculiarities has made it much more difficult for learners to obtain clear perceptions respecting them than would otherwise have been the case. The neglect to discriminate between the two has led to innumerable errors. Into this error both Professor Parrot and the author of this paper have, I hold, fallen, and hence the doubts expressed by the latter.

Dr. QUINET, Brussels: There is no doubt that M. Parrot and Mr. Hutchinson hold different views as regards the influence of hereditary syphilis on the temporary teeth. It is proved by the facts of surgery that this diathesis, which so often kills the child in its mother's womb, and leaves its traces on the thymus gland, on the lungs, and on the liver, must also necessarily leave its mark upon the temporary teeth. Animals also sometimes present in their teeth the marks of blind erosion, as in the oturia gulata and the oturia stellaris in the Museum of the Royal College of Surgeons. Lastly, I recognise a general diathesis—rachitis scrofula—which has the same influence on the dental organs, an influence slow and prolonged in its action, and leaving its traces in the entire organs, and especially characterised by uniformity.

Professor PARROT, Paris: I shall answer the argument of M. Magitot, who rejects the syphilitic origin of dental atrophy, and adopts the "convulsive" theory, by three remarks. Firstly, I would call attention to the *systematic* nature of the dental change, certain teeth being affected, and others never. Thus all the teeth of the first dentition may be affected, and among those of the second dentition, the incisors, the canines, and the first molars; while the second and third molars never suffer, and the premolars but rarely, as I have never seen an example. Why should certain teeth have such an immunity? Convulsions take place at all times of infantile life, so all the teeth ought to be affected. Do convulsions or eclampsia take place in intra-uterine life? We do not know anything on this matter, but it is not very probable. But it is during intra-uterine life that the development of the caps of dentine of the first dentition takes place, and indeed also of several of those of the second dentition. It would therefore follow that during this time there must be numerous convulsions. And, lastly, the extent of the affected dentine and enamel cannot be regarded as agreeing with the relatively short duration of the convulsive attacks.

M. MAGITOT, Paris: Le premier orateur qui a pris la parole après ma communication, M. Moon, attribue les diverses espèces d'érosions à l'action du mercure. C'est une opinion que je ne saurais comprendre. Si nous connaissons bien les effets du mercure, c'est sur les gencives et non sur les dents qu'il porte ses actions et la gengivite est capable dans certains cas de produire sur les dents une certaine altération. C'est là une lésion tout autre et qui n'a rien de commun avec celle-ci. Il nous semble donc qu'il y a ici de la part de M. Moon une confusion entre l'érosion proprement dite qui est congénitale et tératologique et certaines altérations pathologiques de la couronne qui se montrent au niveau même du bord gengival enflammé. Mon excellent ami

M. Ch. Tomes défend encore aujourd'hui, je le vois, son ancienne opinion qui rattacherait l'érosion aux affections diverses de l'enfance. J'ai insisté assez longuement sur cette explication pour n'y point revenir : les conditions essentielles pour qu'une érosion puisse se produire, c'est *l'invasion brusque*, suivie de cessation aussi brusque qui caractérise l'affection, cause possible d'érosion ; c'est ce caractère qui amène l'interruption subite entre la région restée saine de la couronne et l'échancrure ou le sillon. C'est encore la nature même de la maladie intercurrente qui doit être à la fois assez grave pour amener une perturbation ou une suppression dans la formation des tissus, et assez générale pour causer un tel trouble de la nutrition. Or, ces conditions, nous les remontrons au plus haut degré dans l'éclampsie dont le mécanisme, bien qu'encore indéterminé, répond certainement à un état du système nerveux ; état toujours grave, et dont les crises convulsives si brusques et si fugaces sont la manifestation extérieure. Cette objection s'adresse également à M. Dally dont l'opinion se rapproche de celle de M. Tomes. En ce qui concerne M. Coleman qui est le partisan des idées de M. Hutchinson ma réponse se confondra avec celle que j'opposerai au chef de la doctrine, à M. Hutchinson lui-même. Or, le savant médecin de l'hôpital de Londres ne reconnaît pas dans les spécimens des lésions que je présente les altérations caractéristiques, suivant lui, de la syphilis héréditaire, celles qui accompagnent la kératite interstitielle, affection qui serait essentiellement syphilitique. Pour M. Hutchinson la seule forme caractéristique de la syphilis, c'est l'échancrure du bord libre. Il nous paraît cependant impossible de se refuser à admettre que les variétés quelconques d'érosion sont de nature identique et que dès lors si la syphilis héréditaire peut produire certaines formes, elle en produirait aussi bien certaines autres, la seul différence portant sur l'époque d'invasion des accidents et le niveau contemporain de l'érosion. Or je produis ici un nombre considérable de faits d'érosion de toutes les formes, celles de M. Hutchinson, aussi bien que les autres et dans leurs rapports étroits avec l'éclampsie. Si M. Hutchinson persiste à regarder comme syphilitique la disposition spéciale en échancrure, je lui demanderai d'établir deux conditions : la première c'est de prouver que les sujets qui en sont porteurs n'ont pas présenté dans leur enfance et à l'époque contemporaine aucune crise éclamptique ; la seconde c'est de démontrer que les mêmes sujets sont bien et dûment des syphilitiques héréditaires. Or M. Hutchinson n'a pas et ne peut avoir sur beaucoup de ses malades aucun renseignement précis sur la question de l'éclampsie. Ces sont des individus de la consultation externe sans famille sans parent pouvant donner des rensignements sur leur première enfance. J'ai certainement constaté dans plusieurs cas que M. Hutchinson a bien voulu mettre sous mes yeux la co-existence de l'altération dentaire et de la kératite interstitielle, mais de là à la communauté étiologique, il nous semble qu'il y a encore loin. Enfin il est une autre question à juger, celle de la nature syphilitique de la kératite parenchymateuse elle-même, que certains auteurs considèrent comme une manifestation scrofuleuse. Or l'identité des deux diathèses n'est peut-être pas suffisamment démontrée.

Ces diverses observations s'adressent naturellement à M. Parrot à qui je dois, cependant, une réponse sur plusieurs points particuliers :

M. Parrot me demande comment j'explique que les dents temporaires soient si rarement atteintes d'érosion et que certaines dents permanentes (deuxième-prémolaire, deuxième et troisième molaires) ne le sont jamais.

Le première explication est fort simple : Les dents temporaires n'accomplissent pas leur évolution complète avant la naissance, et le tableau que nous avons montré indique pour chaque dent la hauteur de son chapeau de dentine à cette date. Or, que l'intervention morbide survienne dans les premières semaines de la vie, l'érosion occupera sur la couronne des dents temporaires un point

déjà très éloigné du bord libre, et c'est aussi que les dents de lait ne présentent jamais d'érosions en coup d'ongle mais bien des sillons toujours très élevés.

Il est certain toutefois que certaines dents temporaires présentent manifestement des traces d'altérations analogues, si non identiques à l'érosion, et c'est pour ces cas que M. Blache demande si je crois à une influence morbide intra-utérine. Apparement, toute lésion des dents temporaires implique une cause absolument maternelle. Mais qu'elle est cette cause? Ce'st encore, dira-t-on, l'éclampsie? Je n'oserais ici l'affirmer bien que j'ai cru une moment saisir un fait de genre chez une mère morte en couches à huit mois d'un enfant porteur d'une érosion aux canines temporaires. Mais à qu'il est facile de comprendre c'est qu'à cette période fœtale, les influences perturbatrices de l'évolution intra-folliculaire d'une dent sont encore plus facile, que plus tard et que le retentissement de la mère sur le produit soit très admissable.

Nous avons dit d'ailleurs dans le cours de notre communication que beaucoup de cas d'érosion de dents temporaire présentés par M. Parrot nous avaient semblé être des altérations cadavériques. Maintenant, si certaines dents permanentes ne sont jamais frappée d'érosion, c'est ainsi que nous l'avons fait remarquer, que l'état de leur développement ne s'y prête pas. Dans un cas d'érosion en sillon de deux premières prémolaires nous avons retrouvé l'éclampsie à trois ans et demi, époque contemporaire de l'état de développement de la couronne. Enfin, les deux dernières molaires apparaissant encore plus tard, aux dates que nous avons établies, ne peuvent subir l'influence de l'éclampsie, affection dont l'apparition ne dépasse guère la quatrième ou cinquième année. M. Parrot insiste avec raison sur la nécessité, dans cette recherche des causes de l'érosion dentaire, de systematiser les faits qui s'y rattachent. C'est précisement à quoi tendent tous mes efforts : seulement tandisque M. Parrot systématise au point de vue de la syphilis, je systématise au point de vue de l'éclampsie, et c'est en raison de ce principe que je renonce à comprendre comment la syphilis héréditaire, dans son action lente et continue, peut produire, avec cette netteté d'un sillon ou d'une échancrure, l'altération que nous avons décrite.

Les Bossus et la Méthode de M. Sayre.

Dr. A. M. Da Cunha Bellem, Lisbon.

Messieurs et Très-Honorables Confrères,—Lorsque nous avons été à la sixième session du Congrès International des Sciences Médicales, réuni à Amsterdam en 1879, nous avons eu l'avantage d'y entendre l'exposition faite par M. le professeur Sayre, de New York, à l'égard de sa méthode pour le traitement des sujets atteints du mal de Pott et de scoliose, et d'y voir l'application du bandage plâtré à un jeune bossu, qui portait depuis long temps et sans aucun résultat un appareil orthopédique. La simplicité de la théorie, ainsi que celle de la méthode même nous a séduit, et nous nous sommes empressé, dès notre retour à Lisbonne, de faire venir de Londres un trépied, d'après le modèle du professeur américain, pour faire des essais sur sa méthode.

Nous avons eu l'honneur d'être le premier et le seul jusqu'à ce moment qui avons fait en Portugal l'application du bandage plâtré dans le but de maintenir le redressement da la colonne vertébrale, obtenu par la suspension, selon les renseignements du très-illustre praticien du Bellevue-Hôpital.

Nos observations n'ont pas été nombreuses, mais elles suffisent pour nous mettre à même d'avoir des idées arrêtées sur les avantages de la méthode de M. Sayre, tour à tour vantée outre mesure, et blâmée d'une façon impitoyable.

Avant de rapporter devant cette respectable assemblée ce que nous avons recueilli de notre expérience à cet égard, il nous faut avouer que nous n'avons reconnu ni les torts que l'on s'est plu à attribuer à la méthode ni les merveilleux résultats que l'inventeur en a prônés.

D'abord on blâma le chirurgien américain de ne pas avoir été original dans les ressources qu'il met à profit pour le redressement de la colonne vertébrale, et d'avoir caché les noms de ceux qui l'ont devancé en des démarches analogues à celles sur lesquelles reposent la théorie et la pratique de la méthode.

Nous ne voulons pas aborder cette question de priorité, mais nous trouvons bien remarquable que ceux-là mêmes qui blâment la méthode, qui lui font l'accusation formelle d'être erronée, réclament avec autant d'empressement pour leurs compatriotes la priorité de l'erreur.

L'idée de la suspension des bossus, le redressement des courbures de la colonne vertébrale par l'extension et par la contre-extension est d'ailleurs tellement simple, tellement intuitive, qu'il ne faut pas nous étonner de ce qu'elle se soit présentée à plusieurs reprises à tous ceux qui cherchent le moyen de guérir une difformité aussi affligeante que celle qui frappe les bossus.

Eloigner les épaules des hanches, voilà l'idéal de tous ceux qui tiennent à guérir ou du moins à améliorer les courbures du rachis, qui racourcissent l'espace entre les omoplates et les os du bassin.

Que les efforts de ce redressement soient faits, le patient couché ou debout, à la position horizontale ou à la position verticale, tiré des deux bouts de son corps, d'une façon ou d'une autre, le soutenant par la tête ou par les bras, afin que le poids même du tronc et des membres inférieurs fasse la contre-extension, ou le faisant s'appuier sur le point saillant de sa gibbosité, de manière à se procurer le redressement de la colonne vertébrale par l'action des deux leviers représentés par les deux parties du corps, depuis la saillie du rachis jusqu'au synciput, et jusqu'aux talons, ce sont là des détails très-secondaires de n'importe quelle méthode de traitement des difformités de l'épine.

Les négres d'une des possessions de l'Afrique portugaise suspendent par les bras les jeunes bossus et leur enveloppent ensuite le thorax d'un entourage résineux.

Pourrait-on blâmer M. Sayre d'avoir emprunté l'idée fondamentale de sa méthode aux manœuvres brutales des tribus africaines soumises à la domination du Portugal?

Pourrait-on même reprocher à M. Cocking d'y avoir puisé l'idée de son gilet de feutre, imprégné de résine, dont M. Busch, de Bonn, a fait usage, en remplaçant les bandages plâtrés?

Qu'est-ce que cela prouve? Que le principe sur lequel repose le traitement des courbures de la colonne vertébrale par l'extension et la contre-extension, ainsi que celui de l'immobilisation du rachis, le redressement, plus ou moins complet obtenu, sont des principes intuitifs, qui se sont présentés à l'esprit des savants, et à l'esprit des ignorants, à Bouvier, à Gillebert d'Hercourt, à Shaw, à Glisson, à Nucks aussi bien qu aux négres de l'Afrique portugaise, à M. Sayre, ainsi qu'à ses devanciers.

Quoiqu'il en soit, la méthode du chirurgien de New York a fait du bruit dans le monde médical, elle a fixé l'attention de plusieurs chirurgiens de l'Europe et elle y a fait ses preuves, les opinions se partageant à l'égard de ses avantages.

Pour ce qui nous concerne, nous nous maintenons au juste milieu entre les détracteurs et les prôneurs de la méthode, notre expérience nous apprennant que l'on échoue autant que l'on réussit.

Les vues particulières, que M. Sayre a sur le mal du Pott, sur le processus pathologique d'après lesquel la courbure de la colonne advient, sont peut-être trop exclusives, le traumatisme pouvant être invoqué assez souvent, mais pas

toujours, les diathèses ayant quelque fois une influence rien que secondaire, pour en avoir la principale dans plusieurs cas.

S'il y a des courbures, le point de départ gisant à la carie d'une vertèbre, qui fait l'angle saillant avec la vertèbre inférieure et les autres, en dessus et en dessous, restant à peu près régulières ou tout-à-fait régulières, il y en a d'autres, où la série des vertèbres supérieures et celle des vertèbres inférieures, au point anguleux qui fait la saillie de la gibbosité, sont elles-mêmes déformées dans leur rapports réciproques, de sorte à former non une ligne à peu près droite de corps superposés, mais la ligne courbe des étéments d'une voûte, la difformité s'élargissant pour ainsi dire sur et sous son point de départ.

Il est évident que les cartilages intervertébraux, si ce n'est pas les corps des vertèbres mêmes, ont souffert un aplatissement à leur partie antérieure, en conséquence du poids qu'elles supportent, et dont le centre de gravité va tomber en dehors de la base de sustentation par effet de la courbure même.

Si la suspension peut amener, tant peu que ce soit, le redressement de l'angle formé par les corps des deux vertèbres où git la difformité, elle peut bien mieux redresser les courbures supérieure et inférieure, qui ne sont que le résultat de la fatigue éprouvée par la colonne, lorsqu'elle soutient la tête d'une façon anormale, et hors des conditions d'équilibre.

Or, lorsqu'on fait la suspension du malade suivant le diamètre occipito-mentonnier, le rachis étant tout-à-fait soulagé, cède lui-même, aux lois de la pesanteur, obéit à la contre-extension faite d'elle même par le poids du tronc et des jambes, et se redresse à merveille.

C'est ce résultat qui représente, selon nous, le plus grand avantage de la méthode de M. Sayre, le redressement de l'angle lui-même devant être proscript à l'état aigu du mal de Pott, ou, à la période de non-réparation ; devenant à peu prés impratiquable ou du moins n'offrant, que de très-faibles chances de réussité, à la période de consolidation absolue, et le chirurgien n'ayant affaire que par exception à des malades dont l'état soit intermédiare entre la réparation incomplète et la consolidation tout-à-fait complète.

Nous avons vu assez souvent échouer la tentative de redressement de l'angle formé par les deux vertèbres malades, tandis que celui de la courbure secondaire des vertèbres supérieures et inférieures réussit parfaitement. Il est d'ailleurs très-facile à comprendre que si l'on a affaire à deux os ankylosés à la suite d'un processus inflammatoire dont ils ont été le siège, on ne parviendra à vaincre leur position vicieuse qu'en risquant de rompre les adhérences déjà formées, d'amener la luxation des vertèbres en conséquence des violents efforts qu'il faudrait employer, de parvenir même à la rupture de la moelle.

Néanmoins, si on parvient à surprendre la maladie à l'état intermédiare entre la période aigüe et l'ankylose formée, on peut s'attendre à voir, par effet de la suspension, le redressement de la position vicieuse des deux vertèbres qui font saillie et, en tous les cas, on redressera la difformité qui résulte de la position des autres vertèbres suivant un arc de cercle.

Toutes les autres ressources orthopédiques, depuis les béquilles jusqu'aux lits mécaniques, échouant toujours pour ce qui concerne le redressement de la colonne, la méthode de M. Sayre, c'est-à-dire, l'application de l'extension et de la contre-extension par la suspension occipito-mentonnière et par le poids du corps, est très-rationnelle, selon M. Dally. Le détracteur le plus acharné de la méthode américaine se plaît à l'avouer.

L'immobilisation du rachis par le bandage plâtré est le complément de la méthode de M. Sayre, et elle est aussi la partie la plus rudement blâmée des manœuvres mises en jeu pour parvenir au redressement.

Mais tandis que d'un côté on reproche à la méthode de ne trouver, ni sur le bassin, ni aux épaules, des points absolument fixes pour amener l'immobi-

lisation absolue de la colonne vertébrale, d'un autre côté, on lui reproche de trop immobiliser, de ne pas laisser libres, les mouvements des vertèbres saines, qui doivent par leur activité suppléer au défaut des mouvements des vertèbres malades de se procurer l'ankylose, ce qui est une erreur.

A quoi donc devrons-nous nous en tenir en fait d'immobilisation ?

La critique des uns répond à la critique des autres.

Il n'y a point d'immobilisation absolue. Donc, les dangers de l'immobilisation absolue ne sont qu'imaginaires.

Les résultats, obtenus par l'extension et la contre-extension, ne pourraient se maintenir qu'à l'aide d'une immobilisation, quand même incomplète et imparfaite. Donc, il faut mettre à profit cette immobilisation obtenue par le bandage plâtré.

Mais la cuirasse dure et légère que l'on obtient par les rouleaux plâtrés n'agit pas seulement par l'immobilisation qu'elle amène, cette immobilisation étant imparfaite, comme on vient de le dire. Son rôle principal est de maintenir le résultat de la suspension, c'est-à-dire, l'éloignement autant que possible entre les os du bassin et ceux des épaules, et, en conséquence, le redressement de la colonne obtenu par la suspension même.

Pour cela il suffit qu'elle s'appuie sur les os du bassin, malgré leur mobilité et que les aisselles viennent s'appuyer sur son bord supérieur. La cuirasse ne peut pas descendre, la saillie des os du bassin l'empêchant de se porter en bas, et les omoplates ne peuvent non plus vaincre la résistence qu'elle oppose à leur descente, les deux bouts du tronc ne peuvent plus se rapprocher, les épaules et les hanches resteront éloignées, autant qu'au moment de la suspension.

On a surpris alors les aisselles éloignées des os iliaques, le rachis distendu, et le redressement plus ou moins complet de la colonne obtenu ; on maintient ces résultats en prolongeant la suspension. Et si celle-ci rend des services, comme M. Dally le reconnaît lui-même, le bandage plâtré ne fait que devenir permanents ces avantages que l'on ne refuse pas de reconnaître à la suspension.

Si l'immobilisation parfaite et complète n'est qu'une pure illusion, si les vertèbres, dans leur cuirasse, peuvent se mouvoir, plus ou moins, au gré des mouvements du bassin et de ceux des épaules, comme chez tous les bossus, le centre d'action des mouvements du rachis se déplace, ou en dessus ou en dessous le point saillant, ou n'empêchera pas ces mouvements quand même on parviendrait à les amoindrir.

Mais s'il en est ainsi, la prescription des exercices gymnastiques, y compris la suspension journalière, est une puérilité inconcevable.

A quoi peut-il faire tort ce mouvement limité des vertèbres ? A quoi peut-il nuire, lorsqu'on blâme l'immobilisation absolue ? Comment pense-t-on obtenir la guérison ? Par l'ankylose ? Dieu nous en défende. Donc, ce ne sera que par la réparation des cartilages intervertébraux, ou des corps des vertèbres mêmes. Eh bien ! Le point du rachis où siège la lésion osseuse ne peut jamais devenir le centre de mouvement ; et en conséquence si l'on parvient à rompre les adhérences déjà formées, ou si elles ne sont pas encore formées, le redressement de la saillie peut s'obtenir, les stalactytes osseuses entre les deux vertèbres en angle peuvent aisément s'établir, tandis que toutes les autres vertèbres, en dessus et en dessous de la gibbosité, libres de se mouvoir, débarrassées du poids de la tête, peuvent suffire à leur réparation, et contribuer en conséquence au redressement de la colonne.

D'ailleurs, M. Dally, partant de ce fait que toute déformation chronique du rachis offre, comme indication première, une élongation de la portion de ligaments vertébraux raccourcie par adaptation à une attitude vicieuse, fait lui-même l'éloge de la suspension et de la contention par le bandage plâtré.

Pour ce qui concerne la suspension, M. Dally fait remarquer qu'il suffira

souvent d'allonger ces ligaments pour favoriser le redressement ; et la suspension y contribue puissamment.

A l'égard de l'application du bandage plâtré, lorsqu'on reconnaît qu'elle ne peut amener l'immobilisation complète, et en conséquence le danger de l'ankylose, on sera forcé d'admettre qu'elle favorise cette alongation des ligaments vertébraux raccourcis par adaptation à une attitude vicieuse.

Nous avons donc accepté la suspension préalable, le bandage plâtré et des exercices journaliers de suspension après l'application du bandage, et nous ne nous sommes nullement repentis de ces procédés.

Seulement nous avons reconnu l'inutilité de la suspension axillaire, qui rehausse les épaules sans rien apporter de profitable au redressement, surtout lorsque la gibbosité siège à la partie supérieure des vertèbres dorsales. D'ailleurs si l'on cherche à faire la suspension cervico-axillaire, comme les bras des malades sont tenus de se lever au-dessus de la tête, les mains saisissant la corde en cette position, il arrive que l'angle des aisselles s'efface tout-à-fait, et que les lacs de la suspension axillaire deviennent absolument inutiles.

La suspension mento-occipitale, lorsqu'elle est pratiquée avec tous les ménagements, suffit, et elle est la seule profitable, pour obtenir le redressement de la colonne.

Mais nous ne croyons pas qu'il soit nullement indifférent, de saisir un malade dont la colonne est courbée depuis quelque temps, de lui faire subir un redressement forcé en vertu de la suspension, et de maintenir tout de suite ce redressement par l'application du bandage plâtré. Il y a des relations vicieuses établies à la longue, parmi les organes de l'économie en conséquence de la courbure anormale de la colonne ; et il faut ménager ces rapports et ne pas vouloir, tout d'un coup, les contraindre à se transformer sous l'influence du redressement subit du rachis. Nous croyons même que la mort survenue à la suite des vomissements persistants, après l'application du bandage de M. Sayre, chez un malade, dont ce fâcheux résultat fut rapporté par M. Alfred Willett et arrivé au St. Bartholomew's Hospital, a été peut-être la conséquence de ce redressement violent et subit qui amena la dilatation excessive de l'estomac, cet organe ayant acquis l'habitude vicieuse d'être plié sur lui-même selon l'arrangement anormal des organes du thorax et de l'abdomen, déterminé par la courbure du rachis.

La prudence nous conseilla donc, et avec le plus heureux succès, de faire faire aux malades plusieurs exercices de suspension méthodique et journalière avant l'application du bandage plâtré.

Pendant une semaine à peu près nous ne faisions que suspendre nos malades par le menton et par l'occiput, d'abord rien que pendant quatre ou cinq minutes, ensuite successivement pendant dix, quinze, vingt minutes, jusqu'à ce que le patient pût se maintenir suspendu, sans être nullement gêné durant une demi-heure.

En agissant de la sorte nous avons l'avantage d'habituer tout doucement les organes à la position qu'on va leur faire prendre par l'effet du bandage, nous corrigeons peu à peu les relations vicieuses que les dits organes ont contractées, nous les ramenons sans violence et sans contrainte à la normalité qu'ils avaient perdue. En outre, nous parvenons à familiariser les malades avec les exercices de la suspension, dont ils s'effarouchent, de prime abord, et nous obtenons ainsi qu'ils puissent se maintenir dans la position convenable pendant l'application des rouleaux plâtrés, ce qui est un avantage qui n'est pas à dédaigner.

En même temps que les malades, surtout les jeunes gens, deviennent de cette façon, bien plus dociles, qu'ils se prêtent bien mieux à se laisser suspendre, qu'ils s'habituent à l'auto-suspension, en tirant eux-mêmes la corde et en levant leurs bras au-dessus de la tête, on obtient des garanties très-sérieuses contre la rupture

des adhérences déjà formées, contre les luxations incomplètes des vertèbres, voire même contre la rupture de la moelle.

Les malades préparés d'avance avec ces exercices gradués et préalables, nous leur appliquons l'appareil de M. Sayre, selon les renseignements précis de ce professeur, et d'après ce que nous avons appris nous-mêmes en le voyant faire l'application d'un appareil à un jeune malade, à Amsterdam.

Il va sans dire que nous repoussons absolument l'anesthésie, par n'importe quel agent. On ne pourrait, par de telles démarches, beaucoup trop imprudentes, que s'exposer à étendre par trop la colonne vertébrale et dépasser le but ou se risquer de produire la syncope respiratoire et cardiaque, comme M. le professeur Boyer l'a fait remarquer.

Et que pourrait-on obtenir de bon par l'anesthésie des patients? La souplesse des ligaments vertébraux? La flexibilité des éléments qui forment la colonne vertébrale?

On obtient tout cela par les exercices méthodiques mis en usage une semaine d'avance,

S'agit-il d'une opération douloureuse? Point du tout.

Donc, à quoi bon l'anesthésie?

Pour obtenir la docilité de l'enfant ou pour vaincre la peur de l'adolescent!

Nous en venons à bout bien autrement par les exercices préalables, et il n'y a point de malade qui, au bout d'une semaine tout au plus, ne se soit familiarisé avec le trépied, avec les courroies qui lui tiennent la tête par l'occiput et par le menton, avec la corde qu'il tire à merveille. Et tout cet attirail qui l'effarouche au premier jour, dont l'aspect seul le fait fondre en larmes devient pour lui à bref délai un jeu qui l'amuse, auquel il s'adonne du meilleur cœur.

L'exemple donné par un autre jeune malade, instruit déjà aux exercices de l'auto-suspension, et la familiarité avec le médecin traitant, sont les moyens les plus sûrs de porter la conviction à l'esprit des jeunes gens, et de vaincre toute leur résistence à s'y soumettre.

Du reste les jeunes gens sont très-dociles aux exercices de la suspension, quelques jours passés; et la mise du bandage plâtré ne les effraie nullement. Il faut néanmoins les caresser et leur distraire l'esprit, pendant qu'ils sont tenus d'être couchés sur un matelas dur et uni, en attendant que la cuirasse vienne à désécher, pour se rendre assez solide.

Aussitôt qu'on les met debout, comme leur conditions d'équilibre se sont transformées, ils éprouvent quelquefois de la gêne à se maintenir en position verticale, de la difficulté pour marcher; et cela arrive surtout chez les jeunes malades, qui avaient contracté l'habitude d'appuyer une de leur mains sur leur genou pour pouvoir marcher, ou bien mieux chez ceux qui s'appuyaient de leurs deux mains, ce qui n'est pas assez vulgaire d'ailleurs.

La cuirasse les empêche de porter la main sur le genou et ils s'épouvantent en se voyant debout et sans l'appui habituel, de telle façon qu'ils croient même qu'ils vont tomber en avant, et ils ne veulent point quitter ceux qui les soutiennent, de peur de se faire du mal dans la chute.

Le bandage gêne aussi, tant peu que ce soit, le mouvement des cuisses, et il presse sur les hanches de tout le poids provenant de l'appui des aisselles sur le soubord supérieur.

Mais dans peu de jours les malades s'habituent aisément à toutes ces petites contraintes, et, une semaine passée, tout au plus, on les voit courir, s'amuser sans gêne et sans difficulté dans leurs mouvements, ayant tout-à-fait oublié leur ancienne habitude de chercher un point d'appui sur le genou.

Dans un seul cas nous avons reconnu que le bandage plâtré nuisait à la dilatation de l'estomac après l'ingestion des aliments malgré la précaution de

leriver-pad, bien que nous ayons l'usage de le mettre sur le sommet de l'estomac de tous nos jeunes malades.

Mais il s'agissait d'une jeune fille d'une constitution très-chétive, qui avait souffert des fièvres d'Afrique, et qui portait en conséquence la physchonie de l'estomac, s'accompagnant d'hypertrophie du foie et de la rate.

Cela non obstant, l'habitude a vaincu la contrainte première et une semaine après, la jeune malade pouvait faire l'ingestion des aliments sans gêne, et les digestions se faisaient régulièrement comme elles s'étaient toujours faites.

Pour ce qui concerne la respiration et la circulation, elles deviennent plus libres et plus régulières après l'usage de la cuirasse plâtrée, et ce n'est pas là un des moindres bienfaits de la méthode de M. Sayre.

Les jeunes gens se trouvent tellement bien à l'abri de cet corset dur et résistant, qu'ils le réclament, comme un soulagement reconnu, lorsqu'on vient à les en débarrasser pendant quelques jours pour leur donner des soins de propreté.

Voyons maintenant les désavantages inhérents à l'usage de la cuirasse.

D'abord, le bandage plâtré ne peut se maintenir en place, du moins dans les climats chauds, que pendant un mois tout au plus,

Nous avons essayé de le maintenir pendant un plus long délai, et nous nous en sommes repenti. L'accumulation de la sueur, la poussière du plâtre même ont produit des démangeaisons très-désagréables chez les jeunes malades, qui commençaient de maigrir et de devenir tristes et impatients à cause de ce désagrément. Ensuite les conséquences qui dérivent de la malpropreté du corps se prononçaient de plus en plus, une odeur fétide et insupportable même sortant de dessous l'appareil, ce qui était une atmosphère nuisible qui venait entourer les malades.

Leur odorat, leur gôut même en souffraient; ils perdaient l'appétit, leurs bonnes couleurs s'effaçaient, ainsi que leur joie; ils vivaient plongés dans la tristesse, cherchant en vain à se gratter, se dégoûtant de tout, sous l'influence d'un martyre perpétuel que leur procuraient les démangeaisons et la puanteur dont ils étaient entourés.

Nous avons donc reconnu le besoin de remplacer le bandage plâtré à de courts intervalles, d'un mois à peu près.

L'excoriation du point saillant de la colonne devient inévitable aussi, lorsqu'on n'a pas le soin d'y obvenir par le remplacement très-fréquent du bandage plâtré. Malgré la précaution prise d'éviter le contact du bandage sur la saillie vertébrale, en y formant une petite cage à l'aide de deux rouleaux de bandage placés à côté l'un de l'autre, de telle façon qu'ils fassent une saillie plus grande encore que celle de l'angle des vertèbres mêmes, on ne parvient pas à obvenir tout-à-fait au frottement des tours du bandage qui passent sur ce point saillant, ni à celui du gilet de fils de laine, quoiqu'il n'ait ni contures, ni boutons, ni plis; et l'excoriation survient, cette excoriation étant d'autant plus difficile à guérir que les malades sont en général des gens chez lesquels le lymphatisme, voire même l'escrofulisme prédominent d'une façon assez remarquable.

Lorsqu'on s'obstine à maintenir le bandage, l'excoriation peut aller jusqu'à l'ulcération, et, alors, aux fâcheuses conséquences de la malpropreté, de l'accumulation des produits de la perspiration cutanée, des incrustations de la poussière du plâtre, on peut ajouter celles d'une suppuration, déguisée sous le bandage, sans qu'on y puisse porter les soins d'un pansement régulièr.

Si l'excoriation vient à paraître, et bien mieux encore si l'ulcération éclate, on est tenu de soulever l'appareil, et d'en débarrasser le jeune malade pendant plusieurs jours de suite, pour les applications convenables. C'est donc un temps perdu que celui qu'on croit épargner en maintenant trop longtemps la cuirasse plâtrée sans la faire substituer. Mais nous sommes parvenu à éviter

le désagrément de l'excoriation, en plaçant sur la saillie du rachis une pièce de sparadrap carrée, et taillée en croix de Malte, afin qu'elle puisse s'adapter parfaitement sur l'angle saillant de la colonne. Néanmoins et malgré même cette précaution il nous a fallu toujours remplacer le bandage à de très-courts intervalles, d'un mois tout ou plus.

Et il y avait là encore un autre avantage. Les bords de la cuirasse viennent à s'user par le frottement, quelle que soit la dureté que le plâtre ait prise, elle se ramollit; destinée à maintenir l'écartement entre les hanches et les épaules, elle s'élargit d'elle-même, ou en conséquence de ce que le jeune malade vient à maigrir; et alors elle ne serre pas assez bien le tronc pour assurer le maintien du redressement obtenu; ou, par contre, elle peut devenir trop serrée jusqu'à produire de la gêne, si le malade devient plus gras.

La substitution fréquente du bandage y obvie à merveille.

Nous avons essayé de faire glisser du glycérolé phénique sous l'appareil, le portant aussi profondément que possible à l'aide d'une plume d'oie, dans le double but de corriger l'odeur désagréable et de guérir l'excoriation, mais nous n'avons obtenu rien d'assez avantageux de cette pratique, le remplacement de l'appareil étant la seule démarche qui nous ait fourni de véritables avantages.

Chez nous l'usage est admis d'envoyer aux bains de mer tous les jeunes gens chétifs, souffrants de lymphatisme, de scrofulisme, d'anémie, et il faut que le médecin traitant fasse des concessions à cette habitude populaire.

Il ne faut pas discuter ici ce que l'on peut obtenir de bon de l'usage des bains de mer comme ressource thérapeutique. Il suffit de constater le fait de l'habitude inveterée chez les familles de nos jeunes malades; et quand même on viendrait à reconnaître que le bain de mer ne vaut guère que comme ressource hygiènique, que comme un moyen de faire respirer aux jeunes malades l'atmosphère saine des plages surchargées de brome et d'iode qui se dégagent des eaux, il y a raison pour avoir des raccommodements avec ce culte à peu près superstitieux pour la vertu des bains de mer.

Or, l'appareil plâtré entourant le tronc des jeunes malades, leur empêche l'usage de n'importe quels bains, puisqu'il devient impossible d'essuyer la peau, puisque les bandages mouillés pouvent prédisposer aux bronchites, aux pleurésies, aux pneumonies.

Mais lorsqu'on a obtenu quelque résultat de l'usage de l'appareil, c'est dommage d'en débarrasser les malades, pour leur permettre de se baigner, car si l'on n'a pas encore obtenu la consolation, si les cartilages intervertébraux ne sont pas devenus encore assez solides et assez résistants pour supporter le poids de la tête, la courbure revient sous peu.

Nous nous sommes tiré de notre mieux de cet embarras en commençant le traitement pendant les mois d'octobre ou de novembre après que les malades avaient pris leurs bains, car en agissant de la sorte nous avons devant nous dix mois pour amener la guérison de la gibbosité.

Nous avons dit que les jeunes malades étaient très-dociles à l'usage du bandage plâtré. Il n'en est pas de même pour ce qui concerne l'usage de la tige d'acier ou de fer se levant derrière l'appareil, se courbant au-dessus de la tête pour la maintenir en suspension occipito-mentonnière perpétuelle.

Lorsque la gibbosité se présente à la partie supérieure de la région dorsale, lorsque les vertèbres cervicales ont pris une position anormale, lorsque la tête est enfoncée entre les épaules, le bandage plâtré ne suffisant plus pour maintenir le redressement obtenu par la suspension, il faut avoir recours à la tige d'acier qui joue le rôle d'une colonne vertébrale supplementaire destinée à débarrasser les vertèbres de la besogne de supporter le poids de la tête.

Mais les jeunes malades sont on ne peut plus indociles et rebelles à l'emploi de ce moyen. Ils se plaignent de bonne heure, ils deviennent tristes, ils ne

veulent plus bouger, ils ne jouent plus, et on est forcé de supprimer cette partie de l'appareil qui d'ailleurs est indispensable lorsqu'il s'agit des courbures siégeant très-haut ou donnant à la colonne vertébrale la forme d'une S.

La méthode de M. Sayre nous est donc restée impuissante dans ces cas, et nous n'y sommes parvenu qu'à redresser un petit peu l'atitude vicieuse du thorax, sans que nous ayons pu redresser la courbure même, ou vaincre la saillie des vertèbres dorsales, pas plus qu'à faire mettre en ligne les corps des vertèbres cervicales se présentant en concavité tournée en arrière.

La méthode de M. Sayre améliore tous les malades souffrant du mal de Pott; néanmoins elle ne les guérit pas en tous les cas, le redressement parfait de la colonne vertébrale ne pouvant s'obtenir chez plusieurs malades, pas même à l'aide de la suspension régulière exercée tous les jours.

A la période de consolidation des vertèbres il devient impossible de rompre les adhérences déjà formées, l'ankylose même, à n'importe quelle période; elle est impuissante pour agir sur les difformités du rachis siégeant très-haut, à la region dorsale, ou à plus forte raison, à la région cervicale.

Cependent avec l'application de la cuirasse plâtrée selon cette méthode, on parvient assez souvent, presque toujours même, à transformer les bossus difformes en des bossus passablement élégants, car si la saillie reste, la courbure s'efface et la colonne devient, de la sorte, redressée en dessus et en dessous l'angle des vertébres, et en conséquence les pauvres malades peuvent maintenir la position verticale et ils n'ont plus besoin de s'appuyer de leur main sur leur genou.

Il résulte de nos observations que l'usage du bandage plâtré pendant six mois ne donne pas toujours des garanties suffisantes de ce que la colonne redressée ne vienne à reprendre sa courbure vicieuse.

Cela se comprend aisément. La saillie est le résultat du processus morbide qui a eu lieu au corps d'une vertébre; si l'ankylose est formée et si l'on ne parvient à rompre cette ankylose, l'amélioration n'a lieu qu'en conséquence du redressement des autres vertèbres, siégeant en dessus et en dessous le mal. On ne peut donc s'attendre à une consolidation semblable à celle que l'on se procure à la guérison d'une fracture osseuse. Ce serait ankyloser toute la colonne vertébrale. Ces vertèbres ayant pris une position vicieuse en conséquence de l'applatissement des cartilages intervertébraux ou du corps des vertébres mêmes, on ne peut pas s'attendre à les voir assez solides pour résister au poids de la tête sans que la nutrition y ait amené de nouveaux éléments, et sans que le long repos de toute fatigue de soutenir le crâne ne leur permette de se restaurer en effet de ce travail de nutrition.

Or, ce n'est pas là un travail qui puisse s'accomplir en des semaines, en des mois même, si la créature est chétive, si ses os sont mous et très-pauvres en phosphate calcaire. C'est ainsi que lorsqu'on supprime trop de bonne heure l'usage de l'appareil, la tête pesant tout de suite sur les vertébres mal affermies, les force à se courber tout comme auparavant; et il faut qu'elles soient assez solides pour ne pas céder au poids et pour ne pas reprendre l'attitude vicieuse qui leur est donnée par le besoin de chercher des conditions d'équilibre en compensation du deséquilibre qui leur vient de la saillie de la vertèbre où siègea la carie.

Si, par contre, les adhérences vicieuses sont rompues, ou s'il n'y a pas encore d'adhérences formées, il faut s'attendre à ce qu'elles se forment, à ce que l'ankylose s'établisse parmi les vertèbres saillantes, et dans ces cas mêmes, qui sont les moins vulgaires, l'ankylose ne se fait pas aussi rapidement que pourrait se faire la consolidation des deux bouts d'un os ayant souffert une fracture.

Des jeunes gens que nous avons débarrassés de leurs bandages six mois après les avoir portés nous les avons vus, se courber encore, redevenir, à peu près, à la position primitive et chercher le point d'appui sur leurs genoux.

Mais à la longue les vertèbres deviennent plus solides et elles sont à même de supporter le poids de la tête et la verticalité du tronc.

L'usage du sirop au lacto-phosphate de chaux de Dusart et de l'huile de foie de morue contribue puissament à la réussite.

Pour ce qui concerne les cas de scoliose, nous ne pouvons nous prononcer d'une façon très-positive, les observations nous faisant défaut, sur les avantages de la méthode de M. Sayre à leur égard ; mais nous croyons que lorsqu'il existe un développement inégal des côtes de chaque côté du thorax, la suspension ne peut y porter remède, et la compression circulaire du bandage entourant le tronc est insuffisante pour corriger cette inégalité de développement ; et en conséquence nous croyons bien moindres les avantages de la méthode de M. Sayre dans les cas de scoliose que dans ceux de courbure anormale provenant du mal de Pott.

Nous nous sommes imaginé de produire la suspension permanente en maintenant les jeunes malades, pendant toute la journée, sur une sorte de vélocipède, qu'ils feraient mouvoir de leurs mains et de leurs pieds, se tenant appuyés sur leur séant, presque dans la position verticale. De l'arrière de ce vélocipède se lèverait une tige de fer courbée en haut pour soutenir les courroies destinées à faire la suspension tout comme on la fait au trépied. Et le soir on ferait coucher les jeunes gens soumis à ces manœuvres sur des lits appropriés où l'on pourrait maintenir l'extension et la contre-extension par des lacs élastiques.

Mais, pour en venir à bout il faudrait monter un établissement destiné à soigner les jeunes bossus, et y avoir des institutrices assez intelligentes et assez dévouées pour soutenir le moral des malades, pour les amuser toujours, pour les instruire tout doucement, pour leur égayer l'esprit, pour leur faire subir sans peine et sans contrainte le désagrément d'une position constante et d'une suspension permanente.

Les bossus ne sont pas assez nombreux dans notre pays, pour que l'on puisse se hasarder à une telle entreprise ; et les pratiques de l'orthopédie vulgaire y dominent encore tellement qu'on s'attendrait en vain à voir les familles se séparer de leurs enfants pour les faire entrer en pension à l'établissement où l'on essayerait une nouvelle méthode.

En conclusion : La méthode prônée par M. Sayre, qu'elle soit de son invention, ou qu'elle soit copiée d'anciennes méthodes, est digne d'être prise en considération, lorsqu'il s'agit de guérir ou tout au moins d'améliorer les malades souffrant de courbure vicieuse de la colonne vertébrale.

L'orthopédie classique y échoue dans la plupart des cas ; la méthode du professeur américain, quand même elle n'ait pas un plus grand succès, a l'avantage de rendre la respiration et la circulation plus libres.

Les inconvénients dont on l'accuse sont très-faciles à corriger, et de cette façon la méthode devient une ressource précieuse.

Spinal Curvature in Children, with Special Reference to Sayre's Method.

Mr. C. H. Golding-Bird, London.

While the mechanical method of treating spinal disease introduced by Sayre is applicable both to adults and children, it is in these latter more especially, I think, that the value of it is seen.

Of all physical means of restraining the movement of the trunk in children,

none surpasses the plaster jacket, while of all it is the least irksome—less so even than bed itself.

Of the four deforming diseases of the spine—excepting that from cicatrices or empyæma—three are found only in children—viz., caries, rachitis, "general curvature;" the fourth, mollities osseum or osteomalacia, does not concern us now, being an affection of adult life.

A rational treatment is arrived at only by a study of the etiology and pathology of these diseases; and though our knowledge on these points is imperfect, yet we know enough to be able to formulate some general therapeutic rules.

In past years too much attention has been given to the deformity of the spine, too little to its causes and accompaniments; while this fundamental fact must be admitted at the outset, that there is nothing special in these diseases when affecting the spinal column: they are the same as are found in other parts of the osseous system, and they essentially demand the same lines of treatment.

Therapeutically the diseases of the spinal column in children belong to one of two classes:—

a. The inflammatory, or caries—the same disease as is often seen in the cuboid bones of the body.

b. The non-inflammatory, or (i.) "general curvature," a weakening of ligaments and a yielding of bones, the pathology of which is not clearly understood; and (ii.) rachitis, which is a perverted osseous development, the bones being preternaturally weak.

Mechanical treatment, however perfect, is but half the surgeon's duty; due regard must of course be paid to medicaments and hygienic conditions, but these do not belong to my subject.

Inflammatory disease in the bones, or caries, wherever situated, requires that the bones affected should be immovable, free from pressure, in a state of "physiological" rest, and that pus should have ready exit if formed.

Some, but not all, of these requirements rest in bed will afford; but all, without exception, are obtained by the plaster of Paris jacket, with the additional advantage to the child of being able to take exercise in the open air.

The jacket referred to, as you all are aware, consists of muslin bandages steeped in plaster of Paris, moistened, and then applied closely over a skin-fitting merino shirt from the armpits to one inch below the anterior iliac spines.

The patient must be in a position, however, which shall ensure (*a.*) the chest being in a state of full inspiration—*i.e.*, the ribs raised; (*b.*) the inflamed vertebræ free from superincumbent weight.

Such conditions are best obtained either in "Sayre's swing" or by holding up the child under the arms whilst the bandages are applied. Some surgeons prefer a horizontal position, but the maximum benefit is not thus attained.

The various pads of wool employed to prevent chafing must be used according to the surgeon's experience.

If well and closely applied, the jacket is borne by very young children; I have used it in the infant under a year old. We all know the difference between an ill-fitting or good-fitting article of dress: when moulded to the form of the body it ceases to be felt, and only becomes uncomfortable and troublesome when from any cause it begins to press unduly on one part or the other.

It is the same here. Children cannot wear a loose jacket that shifts, however little; and this, in inexperienced hands, is often an unsuspected source of failure and annoyance. We all know how irksome were the old spinal supports of steel and leather to wear, however lightly made: they never formed an integral part of the patient's body, like the plaster jacket; and hence their dead weight, though perhaps not in excess of that of the jacket, told upon the wearer.

The jacket should be worn as long as it is comfortable; and obviously the rate of growth of the child is the chief cause for a change. In children two to three months is an average time for one jacket to last, but this may be extended in patients of ten years or upwards. The presence of a sinus necessitates a window in the jacket; but with an unopen "cold" abscess, no change beyond a protection of cotton wool under the jacket is to be made. I have seen an iliopsoas abscess the size of the patient's head disappear without discharging while using the jacket in a girl of about nine years of age.

If the disease is in the cervical or upper dorsal regions, then the "jurymast" is to be employed if the child is old enough to walk; if not, or if for the time too ill to do so, then rest in bed, with sandbags, is undoubtedly preferable.

Also, if acutely ill, whether the disease be in the neck or back, rest in bed, according to ordinary surgical rules, must be first adopted. Sayre's method only becomes applicable when the acute symptoms have subsided.

To express *statistically* results of the jacket in children with carious spines is impossible—all the concurrent conditions cannot be reckoned;—but I can truly say that I have never known a case fail in showing results, the outcome of the "physiological rest" of the jacket, which were not incomparably better than any obtainable by any other mechanical means. All my results with the jurymast have been most satisfactory.

Non-inflammatory disease of the spine.—The full value of Sayrism in "general curvature" cannot be appreciated in the case of children only. The period of puberty and of young adult life furnish the most illustrative cases of all degrees.

Strictly, in childhood some gain may be hoped for as a rule in all non-inflammatory cases, inasmuch as they are of necessity, with one exception, in an early stage.

The worst non-inflammatory deformities in children are from rickets. This disease, if it affects the vertebræ, has ample time by, say, six years of age, to cripple as much as an old-standing case of general curvature later on in life; and such cases, with marked deformity, a "hump," from protrusion of the angles of the ribs backward and lateral curvature, can be benefited but slightly by any treatment.

Rickets seen early may be at once treated by the application of the plaster bandage, and a favourable prognosis may be given.

General curvature in children requires one golden rule to be followed out—"Never over-support."

Disappointment is met with, and Sayrism is abused, by the surgeon at once putting *all* his cases of general spinal curvature into jackets.

As in flat-foot we modify the support to the requirements of each case, so as not to take all the work off the adductor muscles of the ankle, so must the support be regulated in general curvature of the spine. Indeed, it is not uncommon to find curvature and flat-foot concurrent in the same patient, not, I believe, as cause and effect, but both as the expression of one constitutional condition.

In caries the keynote of treatment is "rest;" in general curvature or in rickets it is "restoration." The former cannot be done without mechanical appliance—*i.e.*, the jacket—unless the patient is to keep his bed. The latter is done primarily, *not* with the jacket, but with the extension of the tripod. The jacket is added, not according to the age of the patient, but the age and degree of the disease.

In cases of early lateral curvature, when the outgrowing blade-bone and elevation of the shoulder are the prominent symptoms—*i.e.*, where lumbar deformity has not become marked, and where rotation is not obvious, the jacket is not needed, such patients are cured by the use of the tripod exercise alone. The earliest age at which children show signs of lateral curvature, say six to seven years, is

1.—The suitability of this form of treatment in cases of angular curvature.

2.—At what stage in the treatment of the curvature it should be made use of.

3.—Its suitability in cases of lateral curvature.

4.—Whether the whole method of treatment should be carried out, or only a part.

In the first place, with regard to angular curvature—is it the best form of treatment to suspend the patient, and to encase his body in the plaster jacket?

If the disease of the vertebræ is diagnosed by the surgeon before deformity takes place, I suppose that even Dr. Sayre would hardly recommend suspension, as it could obviously be of no service. If, however, the vertebræ have begun to fall together and cause the characteristic deformity, is it advisable, or, indeed, justifiable, to attempt at once to rectify the accident and bring them apart again?

This mode of treating caries of the spine by extension has been in use from the earliest ages, although not in the exact way suggested by Dr. Sayre, and it has been generally discontinued and condemned by modern surgeons, for the simple reason that in the acute stage of the disease, before anchylosis takes place, it is attended with danger, and also that in the more advanced cases it is of very questionable advantage. This is well shown in the case mentioned by Mr. Alexander Shaw in his article on this subject in Holmes' "System of Surgery."

In the great majority of these cases of angular curvature in which more or less anchylosis has taken place, suspending the patient, although harmless, is perfectly useless, and the increase in the patient's height, which undoubtedly takes place, is due, not to the straightening of the diseased curve, but to the extension of the compensatory curves, and also to the stretching of the healthy intervertebral cartilages, and its practice can only engender false hopes in the minds of the friends of the patient. If these views then be correct, extension, whether by suspension or by other means, should never be practised in cases of caries of the spine.

With reference to the use of the plaster jacket in cases of angular curvature, all surgeons, including Dr. Sayre, I believe, hold the opinion that the one essential point to attain in the treatment of this disease, before anchylosis takes place, is *rest*. If this desirable condition of rest can be accomplished by the plaster jacket, while the patient walks about or follows his usual employment, and, at the same time, if it does not interfere with the healthy development of the chest by compression, I think every one must agree that this is the thing that has been so long wished for—this is of all forms of treatment the best.

If, however, the structure of the spinal column be considered, as well as its relations to adjacent parts—viz., the chest and abdomen, and the duties these are constantly called upon to perform, it appears incredible that a bandage can be placed around them so as to obtain the required rest for the spine; and, indeed, according to my experience, it does not do so.

Again, if this bandage is kept on, as has been advised, "from one to two or three months," it must of necessity prevent the proper development of the chest, especially in weakly and sickly children, and in the case of adults it is difficult to understand how the pressure caused by it is less hurtful than that of ordinary stays, the ill effects of which are sufficiently recognised.

The idea, then, that *rest* can be obtained for the diseased vertebræ by means of the plaster jacket, or, indeed, any other contrivance, at the same time that the patient is allowed to walk about, is, in my opinion, an error. I venture to think that the only way to obtain rest is to keep the whole body in a state of recumbency.

In those cases in which anchylosis has begun to take place, although sus-

pending the patient can be of no use, I have seen cases in which the plaster jacket has given satisfactory support, and, without doubt, has helped to counteract the weakness that is always felt by patients for some time, even after the diseased vertebræ are anchylosed together. As this is the case, there are probably some cases amongst the very poor, and also in out-of-the-way districts, where its use may be indicated on account of its cheapness and the ease with which it can be applied. As it appears to me, however, that it cannot be kept on for any length of time without causing injurious pressure by preventing expansion of the chest, especially in the case of children, I consider that other forms of support, made either of steel or felt, which can be removed at will and enlarged as the child grows, are far better.

In the consideration of this subject in relation to lateral curvature of the spine, it appears strange that the same form of treatment that is advised for the cure of caries of the vertebræ should have been so confidently recommended for the treatment of lateral curvature, as the two affections are so entirely different in their pathology and progress. Angular curvature is always the result of one cause—viz., caries of the vertebræ, so that it is rational to treat all cases in a similar manner; on the other hand, lateral curvature is due to a variety of causes, and it appears somewhat irrational to suggest one form of treatment for all cases, and for all the different stages through which the deformity passes. For instance, in a great number of cases, in which a lateral deviation of the spinal column is observed, the cause will be found to be a tilting of the pelvis, the result of an inequality in the length of the lower limbs. If this deformity is observed soon after it takes place, the treatment is self-evident, and no spinal support of any kind is needed, and the encasing of the body in plaster or anything else must be useless and hurtful. In all cases of lateral curvature of the spine, the cause must be discovered before we can hope to carry out a successful course of treatment.

In cases of simple curvature of the spine without disease, extension by means of suspension or otherwise is useful, but in those cases in which a support for the spine is absolutely necessary, the plaster jacket is not so efficient as a properly adjusted steel support, by means of which the pressure can be regulated at any time by the surgeon attending the case.

I should not have ventured to express my opinion so positively in opposition to what I am aware is held at the present time by so many able surgeons, were it not the result of a considerable practical experience of these cases during a residence of five years at the Royal Orthopædic Hospital, upwards of 700 cases of different curvatures of the spine having come under my notice during that time. I may also mention the fact that I have myself applied the plaster jacket forty or fifty times, in cases both of angular and lateral curvature, during the period in which experiments were made with it at the Orthopædic Hospital.

Note on Some of the Abuses of the Jacket Treatment of Spinal Disease.

Mr. Walter Pye, London.

Although the time at my disposal will compel me to keep very closely to the subject I have in hand, I must in the first place guard myself against any suspicion of undervaluing the very great step which has recently been taken in treatment of the various forms of spinal lesion, and especially in the cases of

carious necrosis of the vertebræ, usually known as Pott's disease. And further, it is only right to acknowledge in the fullest possible way that it is due to Dr. Sayre's eloquent and forcible advocacy of the "jacket," especially of the plaster of Paris jacket, that, in this country at any rate, the general adoption of this method of treatment is due.

It cannot, at least, be said of us English surgeons that we were prejudiced against, or slow to adopt, a method of treatment, new or revised, because it came from the other side of the Atlantic, when we reflect how universal, in the course of four years only, the plaster of Paris jacket treatment has become throughout the country, and how on certain days in some of our children's hospitals we may see the floors of a ward strewn with small patients of various ages, laid out to dry in their shells, like so many half-finished mummies!

On the contrary, it appears to me that we have been, and are, rather too ready to box up a spine in a jacket as the only panacea for all the ailments and diseases it is heir to.

It is for this reason that I have been tempted to very briefly call the attention of this meeting to some points which in certain cases contra-indicate its employment, and to cases in which its use has been, I believe, not only useless, but actively harmful. I propose also to call attention to the principles which should guide us in constructing a spinal jacket, some of which, so far as my experience goes, are frequently violated, not without danger to the patient.

In the first place, then, I will speak of the discrimination of those cases which are proper for the application of a stiff jacket (whatever material may be used), and of those which are unfit; and in the second, of the modes of application of the jackets, and the risks attending this application if it be improperly or carelessly performed.

It would be mere waste of time and almost an impertinence to my audience if I were to do more than allude to what is, however, in my experience, the commonest error committed in selecting cases for jackets—namely, in mistaking merely rickety spines for cases of commencing serious disease. In some cases, no doubt, the diagnosis is obscure, but it may always be cleared up by watching for a short time. In most, the contrast between the careful rigidity of the true disease and the helpless flexibility and general slackness of the whole spinal column in the rickety child, is so strong, that even if tenderness along the spine be present, it is difficult to account for the error being so often committed.

However, it *is* made, and the consequences are serious enough sometimes. The yielding legs and pelvic bones, already overtaxed by the weight of the tumid belly and large heavy head, are made to bear no slight additional burden, with the result of increased permanent deformity, and very likely a permanent yielding or slackness of the vertebral ligaments and muscles.

The application of a rigid supporting jacket to cases of simple lateral curvature again appears to me to be not infrequent. Yet surely, if it should ever be done, its use should be confined strictly to those cases in which the deformity is so great, that the general health of the patient suffers from the altered anatomical relations and diminished capacity of the abdominal and chest cavities, and where, too, all chance of bracing up the spine by exercise and other means of improving its nutrition have failed.

In these extreme cases of simple curvature I believe a felt or plaster jacket to be better than any apparatus designed by an instrument-maker, but in these only. For while a chance remains of improvement of the muscular and ligamental nutrition, it can only be harmful to bar the door of hope in this direction by preventing all possibility of movement or exercise. Putting aside these two classes, about which I do not suppose there can be much difference of opinion

as to the unwisdom of encasing, I come to a class about which the sense of such a meeting as this may well be taken.

In cases of true Pott's disease, detected at or about its commencement, and before any noteworthy deformity has occurred, should a stiff and immovable support be at once applied or no? Probably the answer will differ with the age of the patient. It must be recollected that we are only here discussing the treatment of the illnesses of childhood; still there is distance enough between a child of nine months and one of say nine years to allow of a different answer being given in the two cases.

Probably the answer for cases which commence after the sixth or seventh year of life will be, that the sooner the support is applied the better, both for the patient's present comfort and for the probability of its ultimate recovery with the least possible deformity.

But what should be the answer in very weakly children, or in those of very tender age? Three lines of treatment may be adopted.

1.—The absolute horizontal position, prone or supine, with occasional counter-irritation, and endeavours to mitigate as far as possible the evils of a prolonged confinement in one position.

2.—A spinal support, presumably some form of jacket combined with the horizontal position.

3.—The jacket, without any confinement of position, or restriction of movement.

Of these three lines, for infants, I believe the last to be the worst. It is tempting, of course, to indulge in the hope of being able to see a patient through the whole course of an illness, such as spinal caries, avoiding the manifold ills of confinement to bed. But can any supporting jacket, put on such young and frail children, be expected to secure even tolerable immobility, and yet not be hurtful in the way of confining the chest and abdominal cavities, and of being by its weight a distressing cause of fatigue, if the patient is allowed to move about?

When we bear in mind that spinal disease (with most others) is not a mere local condition, but is the expression of a generally vicious nutrition; and remember, too, the enormous importance of the due development in infancy of the cavities of the trunk, for the future maintenance of even tolerable health, we may well doubt if it will not be best to tread the old paths in which our surgical fathers walked, and in the early stages of Pott's disease in infants and very young children, simply let the confinement be to the bed or couch, but to secure that the recumbency be absolute and long continued.

For the same reason, *i.e.*, the danger of arrest of development, I am inclined to think the second plan—confinement both to bed and jacket—to be unwise too, though here the fatigue of the apparatus is avoided to a great extent.

These suggestions apply only, as I have said, to *commencing* disease in very young children. Undoubtedly in them, as in other cases of Pott's disease, we may shorten the period of recumbency very greatly by the application of the jacket as soon as consolidation has begun to be established.

I come to another class of cases in which I believe the jacket, if applied at all, should be so with the greatest possible care; and its application followed by careful watching. These cases are the ones in which there exists any noteworthy amount of bronchitis, or in which there is evidence of tubercular deposition in the lungs, active or quiescent; or in which there is cardiac disease.

I can call to mind, and I know my experience is not singular, at least five or six cases in which exceedingly alarming and urgent respiratory difficulty has followed the putting on of the jacket; and two, in which death actually occurred.

In some of these cases, of course, the distress or disaster may have been caused by the jacket having been put on badly, a point I will refer to directly. This was not the case, however, in all, or most of them; and we should, I am very sure, bear in mind in similar cases, that even if there be no apparent respiratory distress before the jacket is applied, still if there be lung or heart disease present, the child may very possibly be without any reserve breathing capacity at all, and that a very slight embarrassment of respiratory movement may produce dangerous dyspnœa.

The last class of cases I will mention in which the jacket often I think does harm, and in which its use must be attended with great caution, are those which are complicated by any high degree of paralysis. Apart from the common sense difficulty of keeping the jacket and parts beneath it clean and sweet when there is dribbling of urine or incontinence of fæces, these cases are very liable to the extremely rapid formation of sores, somewhat akin to the acute decubitus of Charcot; and as these are very insidious in their origin, and often at first nearly painless, great destruction of tissue may take place before the jacket is removed on account of the offensive discharge, and the mischief is discovered.

With these I end the list of cases in which I believe that the employment of a jacket, be it of plaster of Paris or felt, or other material, is either directly harmful, or is only to be decided on and its effects watched with care.

It remains for me now to notice one or two of the more important ways in which the jacket is harmful, not because it should not have been put on, but because it was put on in the wrong way. I will not waste your time by discussing questions of mere detail in which errors are due either to want of common sense, or want of practice, but will consider those cases only, where the principle of the treatment appears to me to be wrongly conceived.

In the first place, plaster of Paris jackets are very often far too heavy. I have seen some on quite young children weighing as much as five and six pounds. No argument is needed to point out that the weight of the structure very seriously diminishes its utility. And it is, moreover, quite unnecessary. If care be taken that the jacket is really fitting, and that the bandages lie in the direction of intersecting loops or figures of 8, a very slight thickness will serve as an abundantly strong support. A more important question to settle, however, is that of the employment of the swing and pulley, and I should be glad to elicit some expression of opinion on this subject. I myself hold that in children the use of the swing is bad in practice, and that, moreover, it is founded on erroneous surgical principles.

The question resolves itself into one of the justifiability of forcible extension of the spine. I know that it is held out as one of the great merits of the jacket treatment that by it the angular curvature can be cured, and it is not at all unreasonable to suppose that complete suspension may sometimes succeed in breaking down adhesions between consolidating vertebræ, and in untwisting the rotated axis of the spine. But is this an end we are surgically justified in trying for? Surely if we consider what consolidation means, and what the destruction has been which has thus happily ended, we may rest satisfied with so applying the support, that there can be no increase of the deformity, and that the settling together of the vertebræ may be as thorough as possible.

Again, in children, the swing has always appeared to me to fail in one very important point—namely, in securing that during the whole time that the jacket is being put on, the walls of the chest be in a condition of extreme inspiration. I believe that in those cases in which this point has been neglected the respiratory trouble often occurs. The swing, with stirrups under the armpits, seems to me, through the yieldingness of a child's shoulder girdle, to pull up the

shoulders and displace the scapulæ, so that this inspiratory position is not attained. The swing also is an object of great terror and awe to most children, and although this point would not be worth considering if there were no alternative, still it may be borne in mind. If the idea of forcible extension be abandoned, the points to be aimed at in considering the position of the child at the time of the application of the jacket are—*firstly*, that the body shall be as far as possible in a straight line—all departures from this, which are due merely to weakness or slackness of ligaments, being remedied; *secondly*, that the chest shall be in a condition of extreme inspiration. These conditions can obviously be most easily fulfilled by the use of a single inclined plane, on which the patient may lie with his arms extended over his head, and there grasping something, say a rod. In this position a roller plaster of Paris bandage cannot be well applied, but if mucilage and plaster be used, and the bandage cut in strips in the way advocated by Mr. Walker, it will be found a very comfortable way of putting on a jacket, and it is, I believe, certainly the best position in which to apply any of the resinous felt jackets, now often employed.

For the ordinary roller plaster of Paris bandage I believe the best method is simply to hold the child up by the arms close to the shoulder, allowing so much weight to be borne by the feet as will prevent any undue extension of the spine. In this way the proper position of the chest walls and body is secured.

Of Mr. Davy's method of extension by swinging in a hammock in a prone position, I am sorry to have had no experience. It seems, however, hardly suitable to young children.

Lastly, it is difficult to insist too strongly on the importance of the jacket clasping the pelvic bones below, and following accurately whatever curve the spine may take. It is very common to see jackets looking, and being, in fact, just like so many tubs with their ends knocked out encircling the patient. Jackets of this kind can never fix the spine—indeed, they never can fit properly at all—and working up and down as they do with every change of the patient's position, they are a very fruitful source of sores.

I am very conscious that I have laid myself open to the charge of unduly insisting on petty details. My defence is, that they are all suggested by faults I have seen committed, or have committed myself. Moreover, the details of the surgery of adults may perhaps in children be regarded as having a greater importance when we recollect that we have in them to deal with a balance in which the question of life or death, of recovery or permanent hurt, are weighed in a far more sensitive way, and which is far more readily affected for good or ill, than is the case with grown-up people. "De minimis non curat lex" may be good law, but "De minimis non curat chirurgicus" is bad surgery in whatever sense we may prefer to translate it.

DISCUSSION.

Mr. A. E. Barker, London: The purpose of these remarks is as follows:—To give an entire allegiance to the principles of treatment of caries of the spine formulated by Professor Sayre; to express further the belief that the method of treatment designed by the same surgeon is the best of any yet devised for carrying out those principles; to submit that when the application of the jacket has been followed by no benefit, it is owing to the fact that the details of application developed and insisted on by Professor Sayre have not been carefully carried out. Two or three conclusions have, however, forced themselves

upon me as the result of experience, which are at variance with those expressed in some of the papers read to-day:—(1.) That the corset is not suited to caries in very small children. (2.) That an actual improvement in the actual angle by suspension is not to be hoped for, or indeed aimed at, in view of dangers observed. (3.) That there are cases quite consolidated where the corset is often applied unnecessarily.

Dr. Martin Oxley, Liverpool: I consider Sayre's treatment is most satisfactory, and having had a large experience at the Liverpool Infirmary for Children, where every case of Pott's disease is put up in a plaster jacket, I have much pleasure in stating that I consider it to be the only treatment for this disease. When I first began to put on the jacket, I have no doubt that I frequently over-extended my patients, and it may be possible that I did harm, not good. For this I must say, with all due deference, Dr. Sayre is in some measure to blame, owing to his having demonstrated the extent of curve before and after extension. The points in the treatment which require careful attention are as follows:—All prominences ought to be covered with a pad of cotton-wool in the form of a ring. We must not over-suspend, as the object of the suspension is simply to take the weight off the diseased vertebræ, not to straighten the deformity, although, as a matter of fact, we do straighten the spinal column as a whole; for the patient, instead of bending forward, as is usual, and leaning the hand on the knees, or on some object near, such as the back of a chair or table, has the whole body above the disease supported by the articular processes and the pully. In conclusion, I would say that so far I have not seen any case of disease of the spine that I would not treat with a plaster jacket. To my mind, the most important point in the treatment is the long-continued rest which a jacket, worn for half a year or more, gives to the diseased vertebræ. On this account I would strongly insist upon keeping the jacket whole, not slitting it up the front, as is too often done; for when a jacket is cut, we must, in order to make it a support, bind it firmly on the body, compressing the chest, and doing away with the space in front left by the removal of the inner pad. Patients may be kept perfectly clean for months by changing the Sayre's singlet every week, or oftener, in the following manner. Before applying the plaster jacket, put on two of Sayre's singlets instead of one; when the change is necessary, tack a clean singlet to the lower edge of the singlet next the skin, then, by drawing the latter (the soiled one) up over the head, the clean one easily follows and takes its place.

Mr. Edmund Owen, London: The position which a not inconsiderable experience with the plaster of Paris treatment in the out-patient department of the Children's Hospital has led me to assume may at first sight appear anomalous; for though with certain modifications I have carried it out for some years, and though I hope still to employ it, yet I do not consider it to be the best method of dealing with cases of spinal caries. I welcomed the Sayre treatment because I saw that it would afford us the means of dealing with spinal caries on the same principles as those which influence us in the case of caries of the tarsus—viz., of ensuring absolute physiological rest; also I read in the plaster scheme the death-knell of the expensive, heavy, and often useless mechanical supports. Certainly, since Professor Sayre's demonstrations my own cases of spinal disease have had a much happier time; but I think that the introduction of the method has suffered from the too great enthusiasm with which it was surrounded. One was led to suppose that there were to be no more angular deformities, and that those which had occurred were to be straightened out, "unless nature had already thrown out ossific matter, and adhesions were

beginning to form; that then they were not to be broken up by too severe extension." ("No, no," from Professor Sayre.) Those were the Professor's own words, and surely they implied that a straightening out of many angles might be expected. I have given up all suspension, and I am now relieved from that anxiety which must attend the suspension of a child with an ulcerated spine. I am glad to hear gentlemen confessing that such accidents were within their knowledge. Had I a child of my own the subject of commencing spinal caries, I would not put him in a plaster of Paris jacket, but I would keep him flat and absolutely quiet in bed for months or years, until the disease was quiescent; only then would I encase him in a firm support, and have him moved about. I gratefully accept Dr. Sayre's method (without suspension) for my hospital out-patients, because it affords them the nearest approach to physiological rest which their domestic surroundings permit.

M. DALLY, Paris: According to my experience, the suspension acts in a very limited manner, and only when the patients are not entirely suspended. If they are raised from the ground, it is the muscles of the neck that are elongated, and not those of the back proper. If patients are not entirely suspended, and if they are directed not to make any muscular resistance, no doubt then some elongation can be obtained by the suspension. In cases of rotatory deformity, in cases of anchylosis between the vertebræ, and in the costo-vertebral articulations, suspension cannot effect elongation. In the latter cases it is only by proper training that any modification in the shape of the spinal column can be effected. In the early stage of vertebral caries, vertical extension may certainly be of great service by removing the weight of the upper part of the body. But when the torsion is confirmed, how can one reasonably hope that vertical extension can bring about a cure. It is impossible both on mechanical and physiological grounds. If stretching *were* possible in cases of caries, death would result, and in a few cases has occurred. Fortunately suspension is limited by the muscles of the neck. I am pleased that extension is not practised so much as formerly, and that the best part of Sayre's treatment is being retained.

Mr. BERNARD ROTH, London: I cordially agree with everything that has been said in favour of Dr. Sayre's plaster jacket for the treatment of spinal caries, and I have never had any sores or other discomforts resulting from it in the forty or fifty private patients I have thus treated. In almost every case, without exception, the patient has assured me how very much more comfortable he or she felt the moment the tripod was left with the jacket on. I beg to thank Dr. Oxley most heartily for the very useful hints he gave us yesterday about changing the vests under the jacket, which completely removes all objection to Sayre's jacket on the score of cleanliness. With reference to lateral curvature of the spine, I am sure Dr. Sayre has been led too far by his noble enthusiasm, and that his teaching will do much harm. It is very difficult, from what has been both written and spoken by Dr. Sayre, to understand to which stages of lateral curvature he refers when he speaks of some brilliant cures by his method applied to lateral curvature. I constantly examine cases of lateral curvature which at the first glance appear very badly deformed, with scapulæ very unsymmetrically placed—one hip unduly prominent; yet I have found on careful examination that it is quite possible to restore the apparently deformed spine back into a perfectly normal shape, and for the hip to retain this position for a few seconds; the fact being that there was no real osseous deformity, only a relaxed vicious condition of the bones, ligaments, and muscles. These cases are claimed as *cures* by Sayre's treatment, but there never was any deformity

present. I challenge Dr. Sayre or any of his disciples to exhibit at a Medical Society here a case of lateral curvature with real osseous deformity of the vertebræ and ribs, even to a very moderate amount—but it must be there—and then to exhibit the case again within six or twelve months as being cured by his treatment. Daily suspension by the head in the tripod does not strengthen or exercise the muscles, as maintained by Dr. Sayre; it is against common physiological sense. If you wished to strengthen a weak and flabby biceps muscle, surely you would not put it in a plaster bandage with the elbow extended to its full extent, and then hang the patient repeatedly daily by the hand of the imprisoned limb. I have now seen several patients with osseous lateral curvature who have been daily suspended by the head for months, and if Dr. Sayre's theory were true, at all events the neck muscles should have been strengthened, as they are most severely acted upon by the extension from the weight of the body; but nearly all of them poked their head as much as ever they did, I was informed by the friends. If you really wish to strengthen the muscles of the spine by exercise, you must put the patient in such a position that these muscles are called into play. Let the patient lie on his stomach (prone) on the ground, and while the pelvis is held, request him to raise the head and upper part of the trunk a few inches from the ground without the aid of his arms, and the whole mass of the erectores spinæ are bound to come into action. Sayre's jacket is only of use in lateral curvature in cases of extreme osseous deformity in supporting and giving relief to the patient, and in preventing the deformity becoming worse, also in cases of paralytic lateral curvature; short of this I am convinced it only does harm, like the ordinary steel spinal supports with crutches, which I am glad to see are being more and more condemned by most general surgeons, except those specially interested in them.

Mr. Ketley, London: I have suspended patients chiefly for the application of plaster cases, but partly for those of poro-plastic jackets, and to a lesser extent for mere purposes of measurement, over one thousand times, and with no worse result than an occasional tendency to faint on the part of adults. And I have not met with a case of fainting for nearly a year, nor do I expect to again, now that experience has brought greater expedition and a quicker perception of the signs of approaching trouble. Cleanliness may be secured with the use of the plaster jacket by wearing two linen or cotton handkerchiefs beneath the jersey next the skin, one anteriorly, the other posteriorly. These are changed by stitching the dirty one on to the clean one which is to take its place, and then using the former to drag the latter in. This is a similar method to that by which it appears Dr. Oxley changes his inner jersey. The handkerchiefs take up no perceptible room, are too thin to make any uncomfortable wrinkle, exercise no elastic pressure on the mammæ, are smooth, and are always readily obtained. The plaster case is just as removable and reapplicable as any instrument, but—and this is a great advantage—only when the surgeon wishes it. To change the plaster case from a fixed to a movable one, it should be neatly sawed up the middle in front, and then a *triple lace* applied. Holes are bored along each side of the opening, and a separate cord laced up and down so as to make a row of loops on each side. These loops are permanent. The jacket is then reapplied, and a third movable lace is laced in and out the loops of the two fixed laces. Sores and excoriations are best avoided by kneading the case to make it fit accurately all prominences. The plaster jacket can be made at once light and strong by building it up on the principle applied by Fairbairn and Stephenson to the construction of the Menai tubular bridge, and of hollow girders in general. The felt jacket, though intrinsically inferior to the plaster one, is very useful, because it is convenient and economical of time.

Dr. DIVER, Kenley: I have had some opportunities for forming an opinion upon Sayre's method, though not approaching the large experience of previous speakers. But I have seen enough to lead me to form a distinctly favourable opinion of the process. I have used it in cases where the patient was by it increased in height, made distinctly comfortable, and every one concerned with the patient more happy. Sayre's treatment is not only better but much less costly than the old spinal support plan. For as many pence as these instruments cost pounds a better result can be brought about. With the steel instruments the history of the patient is, that after a few days' wear of them a screw becomes loose or lost, and the apparatus has to be taken off and sent to the machinist for repair and await his convenience to put right. Then in a week or two perhaps a steel support gives way, and another period of time is lost while the apparatus again goes to the maker. So expense and loss of time are brought about. I would ask Professor Sayre one or two questions which were suggested by difficulties in carrying out the treatment. One, whether it is possible to adopt a method of moistening the roller without on the one hand soaking the roller unduly, and on the other of losing much of the plaster by its falling from the bandage? Also what method is best in evenly distributing the plaster over the bandage? Also the best way of opening the apparatus without the dust of unwinding? Whether Dr. Sayre will say within what age his plan is likely to be useful? Also whether there are cases in which his treatment is unadvisable if not inadmissible? In short, if he will help us in eliminating unsuitable cases from the employment of his method? It cannot be that Dr. Sayre's treatment is commonly injurious, as some have said, for it is impossible for men who have had experience of hundreds of cases to speak with almost unrestricted praise of a method, if that method, when properly used, be harmful.

Mr. J. H. MORGAN, London: Nearly all previous speakers have referred to the danger which may attach to suspension, and the necessity of modifying the swing apparatus of Sayre. Why suspend these patients? the speaker ventures to ask. Is the position of the various elements of the spine when the body of a patient is suspended in that condition of physiological rest which it is the object of the plaster of Paris bandage to maintain? Surely the muscles and ligaments whose function it is to keep the various elements of the spine in such a relationship as will support the superjacent weight are in anything but a state of rest when the body is suspended in this way. The only conditions under which all the elements of the spine can be at rest are when the body is recumbent. If, then, the spine is fixed by the plaster of Paris bandage—for the use of which in such cases all credit is to be given to Dr. Sayre—the spine is immovably maintained in a position of rest, and weight is removed from it and transferred to the bandage.

Professor SAYRE, New York: My whole mission this year is to correct, if possible, some of the errors which many distinguished gentlemen have made in my treatment. I thought that my directions were so simple and plain that they could not be misunderstood; but I find this is not the case, as I see that even so distinguished a man as Professor von Langenbeck has used extension (in my judgment) to a rather dangerous extent, and even recommends the use of an anæsthetic during the suspension. With all due deference to the opinion of so distinguished a gentleman, I must strongly protest against the use of any anæsthetic while the suspension is being made; and I beg any gentleman, if he does use chloroform or any other anæsthetic, not to call the treatment by my name, as I do not approve of its use, and am unwilling to be held respon-

sible for its employment. I make these remarks thus strongly because I notice in various medical journals a report of the proceedings of the German Surgical Society, in which Professor von Langenbeck reported a fatal case occurring in his clinic, and some of these journals have headed the article with leaded type as "*Fatal Result from the Application of Sayre's Jacket.*" Now the fatal result was not from the application of the jacket, but from rupturing of an abscess by the suspension of the patient, the suspension being made under chloroform, in direct violation of my teachings; and therefore I do not think my method of treatment should be held responsible for such a result. In my work on "Spinal Curvature" (Smith, Elder, & Co: London, 1877), pp. 21, 22, will be found the following definite instructions. "Before narrating the cases illustrative of the principles of treatment here advocated, I wish to give, or rather repeat, a word of caution, fearing that I may not have enforced it with sufficient distinctness already. It is this: *Do not attempt the impossible*—do not try to straighten curved spines, the result of caries, that have become partially or completely consolidated. If nature has already thrown out ossific matter and adhesions are beginning to take place, *do not break them up* by too severe extension, but simply extend the patient very slowly, so that the contracted muscles alone will yield, *until the patient says he feels comfortable, and never extend the patient beyond that point.* If it is a child who cannot talk, watch his countenance, and as soon as the expression of *pain* is changed to one of *pleasure, there stop*, and *secure* your patient by the plaster bandages, keeping him in that position until the plaster has set; he will then retain this sense of comfort so long as the bandage is properly adjusted." There, gentlemen, I don't know how to write any more distinctly than that, and yet I have been compelled to cross the Atlantic this year in order to impress this principle of treatment upon the professional mind—namely, giving your patient perfect comfort and freedom from all pain. This is to be done by making extension just sufficient to take off all pressure from the inflamed parts, and then retaining the parts in the position by the plaster bandage. If the patient is under the influence of an anæsthetic, it is impossible for him to tell you when you have extended him to the exact place desired, and no one else can; it should therefore never be used in the suspension of a case suffering from Pott's disease under any possible circumstances. There is no pain whatever given by the *proper* application of the suspension, and therefore no anæsthetic is required to relieve it. On the contrary, the proper application of extension gives immediate relief to the patient, and this is the invariable testimony of the many hundred cases in which I have employed it. I prefer to make the extension in the vertical position by suspension because it is more easily managed, and while in this position I can apply the plaster bandages better than when the patient is in the recumbent position. But suspension is not absolutely necessary to carry out my principles of treatment, for extension can be applied in the horizontal posture, as advised by Mr. Walker, or in the hammock, as advised by Mr. Davy; but neither of these methods is so convenient or so easily managed for both surgeon and patient as the extension by means of the pulleys with head and axillary straps. The objection to Mr. Davy's plan has been very well pointed out by Dr. Oxley of Liverpool in the July number of the *Liverpool Medical Journal*—viz., the danger of over-distension by curving the body too much backward; and he therefore suggests cutting holes through the hammock for the legs to protrude. I have never had any difficulty in applying suspension, even in the youngest cases. They often cry while being undressed for the first time, and having the skin-fitting shirt, dinner pad, &c., properly adjusted, particularly if they have already been in the hands of other surgeons, and have had instruments applied, as they think some new torture is to be inflicted; but so soon

as the head and axillary straps are properly adjusted and the extension properly made, they cease crying almost on the instant, and give a deep, full, diaphragmatic respiration in place of the short, grunting, catching breathing they had before the extension was applied. This was very markedly the case in the little patient which Mr. Davy saw in my office, and which he has described as suffering such torture from the suspension, whereas the instant the extension had reached a certain point the child ceased crying, and her face was entirely relieved from all expression of pain. But Mr. Davy's feelings were so touched by the previous crying of the child that he turned from it to examine some pictures in the office, and thus lost the important moment of observation, when the "countenance changed from pain to pleasure," and he has thus innocently represented the tortures and pain of suspension, not having observed the instant relief to the pain when the suspension had reached the proper point. In a very few minutes the jacket was applied by my son, and the child, almost immediately after being laid on the air-bed for the plaster to "set," fell into a sound sleep, such as the mother said it had not enjoyed for months. I have seen a number of cases go to sleep while the jacket was being applied, and with children as a rule they go to sleep while the jacket is hardening or getting "set." The only treatment to be adopted in cases where the jacket is inadmissible—as in very small children or some cases of abscess—is the horizontal position; but even when the horizontal position is assumed, it must in many cases be supplemented by extension, to overcome the tendency to reflex muscular contraction induced by the inflamed vertebræ. I have seen many cases that have been confined to the horizontal position for years, and yet the disease was still progressing, and in most of them there was marked deformity; and I have seen complete recovery with consolidation in a great number of cases in less than a year, and with very slight, and in some cases no, deformity whatever, with extension and the plaster jacket; so that the prophecy of Mr. Furneaux Jordan, that "the days of the humpback were ended," is not, after all, so visionary. If we diagnosticate the case early, and treat it *before* the deformity is commenced, we can in the majority of cases effect a cure in from eighteen months to two years, and with very little or no deformity perceptible. If, however, the deformity is allowed to occur, it will remain permanent; and although you will hasten the consolidation, and thus effect a cure more rapidly and with more comfort to the patient by the method I have suggested than by any other mode of treatment, still the deformity will remain just in proportion to the extent of the destructive process that has taken place in the bodies of the vertebræ before the treatment was commenced. It has been asked at what age should the treatment be commenced. I answer, at any age. If a person of seventy breaks a limb, you certainly try to repair it the same as in a younger person; and the same rule holds here. In children too young to walk it is unnecessary, as they can be carried outdoors in the wire cuirass; but as soon as they can walk, the plaster jacket is better, as it enables them to take free exercise in the open air, which improves their digestion and powers of nutrition, and this is the essential element of cure. When the disease is in the cervical or upper dorsal vertebræ, the "jurymast," or head-rest, is absolutely essential, and I am satisfied that the suggestion of Dr. Samuel Grey of Philadelphia, that it should be used when the disease is even lower down, is a good one, as it relieves the parts from the pressure of the superincumbent weight of the head. I have applied it in many cases of two years, and in one of sixty, and one—an old Irishwoman at the "Home of Incurables" in New York, paralysed in lower extremities, and confined for nearly three years to bed—by making gentle extension from the feet, while Dr. Jones, the resident physician of the Home, sustained her under the axillæ, she almost

immediately said that she could feel in her toes. This settled the question that sensation was interfered with by pressure, and she was suspended while sitting in the bed, and a jacket and head-rest applied. In three weeks she was so much improved as to be able to stand, when a new jacket was applied, which was much better fitting, as she was erect during the application; and in one month she was able to walk with a stick, in six months walk without any assistance, and is now perfectly well. Objections have been made to the want of cleanliness. This is easily obviated by the suggestion of Dr. Oxley of Liverpool, by using two knitted jackets, the under one being pulled out when necessary, after stitching a clean one to it, which is thus drawn into position. There has been a mistake in putting Pott's disease and lateral curvature together in the same discussion. One is a disease producing deformity the result of inflammatory disintegration, the other is a distortion the result of unequal muscular contraction. Mr. Adams says, "It is as bad for lateral curvature as it is good for Pott's disease." This I am confident is a mistake. Lateral curvature, being the result of debility, requires gymnastic exercise to develop the muscles, and self-suspension, as suggested by Dr. Brig Lee of Philadelphia, is the best. This, with "Massage" electricity and general toning up of the system, is all that is required, and the jacket is unnecessary. But in advanced cases the jacket is requisite to retain the benefit obtained by self-suspension. When the jacket is thus applied, the patient is to continue the self-suspension twice a day, until the body is so straightened out as to receive no support from the casing, and then the projecting hump is to be pulled off from the shirt, and the triangular portion thus cut away is covered over with a fresh roller of plaster bandage after the patient has suspended herself, thus securing again the body in the improved position. This process is to be renewed as often as necessary, until the patient is improved as far as she is capable of being, and then the jacket is made into a corset, and worn as an ordinary dress corset; and being made of plaster bandage *on the body of the patient*, is more accurate in its fit than any other apparatus that can be made. I have now treated eighty-seven cases of lateral curvature by this method since January 1, 1877, thirty-nine of which are cured, and forty-eight are still under treatment. Several of these cases are perfectly cured, and are straight, as will be seen by these photographs, taken before and after the treatment. All of them present better results than I have ever been able to obtain by any other method.

A large number of photographs to prove the accuracy of his statements was then exhibited.

The Surgical Vice-President, Mr. HOLMES, London, at the request of the President, summed up the debate. He said that the following appeared to him to be the main conclusions arrived at:—(1.) Nobody seems seriously to contest the priority of Dr. Sayre as the introducer of the method. What traces there may be of somewhat similar treatment in the hands of former surgeons are not more than the resemblances always met with between our predecessors' ideas and those of our own day. (2.) The discussion had been obviously imperfect in respect to other spinal curvatures than that from caries. In fact, it would have been better to restrict the discussion from the first to angular curvature. (3.) The debate had not enabled us strictly to define the class of caries in which the treatment might be considered as more especially indicated. Most of those who recommend it agree that the earlier it is employed the better; but we are still unable to say whether and how far symptoms of decided spinal irritation or inflammation are to be taken as contra-indicating it. (4.) Only a small minority of the speakers reject the method entirely, and believe that confinement to bed is preferable. The opinion of the

great majority seems to be, that in at any rate a very large proportion of cases of spinal disease, this method offers very large advantages, both local and general. (5.) It results from this discussion that no form of extension, whether by suspension or otherwise, is to be regarded as a necessary feature of the treatment. The jacket can be applied either in the suspended, the erect, or the horizontal position. (6.) There appears no evidence that any actual straightening of the spine has ever been produced. Dr. Sayre has strongly dissuaded the attempt to straighten the spine when there is any reason to think that the bones are at all consolidated together; and although he said that if applied before any deformity existed, his method would prevent its occurrence, he seemed quite uncertain as to the possibility of redressing even slight and commencing deformities. (7.) As to the question whether other plastic materials would not do as well as plaster of Paris, this discussion seems to show that there is no reason why such should not be the case, though Dr. Sayre and most of the speakers seem to prefer the plaster. (8.) An important point had been brought out as to the possibility of changing the inside shirt without removing the plaster case. This is not only convenient for cleanliness' sake, but may in some cases be a matter of serious practical importance. (9.) That there are drawbacks to the method in the shape of ulcers, abscesses, &c., seems not only possible, but inevitable. The extent and nature of such drawbacks should be clearly stated. They form no radical objection to the treatment. (10.) The average length of time required for cure is a matter of very great importance. It will probably be found to be much less than in the treatment by rest in bed. (11.) Finally, the general opinion seems to be that this is a real and a great advance in practical surgery.

On the Nature of the So-called Scarlet Fever after Operations.

Mr. Howard Marsh, London.

In 1864, Sir James Paget gave a clinical lecture on scarlet fever after operations, in which he related the case of a boy, who, on the day after he had undergone lithotomy, had an eruption exactly like that of scarlet fever, and a month later a considerable hæmorrhage from the kidney, followed in two days by sore throat and a return of the rash, and subsequent desquamation.

After stating that in the previous year, when scarlet fever was very prevalent, he had seen six cases following operations in private practice, Sir James Paget expressed the opinion that there is something in the consequences of operations which makes patients peculiarly susceptible to the influence of the scarlet fever poison, and added, that in such cases the disease undergoes certain modifications: the incubation period is shortened; the rash is irregular; in some instances there is no sore throat; and in some no desquamation; while in others sore throat and desquamation alike are absent. Since this lecture was published the subject has been carefully studied, and the opinion of Sir James Paget has been so fully confirmed, that this peculiar liability is established to the satisfaction of a large number of skilled observers.

It may, however, be well on the present occasion to state concisely the evidence on which this conclusion rests. For this purpose I will turn to the Hospital for Sick Children, for in its records convenient illustrations can be found of all the points that I shall have to mention.

1.—In the original building, consisting of two large dwelling-houses, the

wards, containing both surgical and medical cases, were on the first and second floors, while immediately above there was a fever ward, containing six or eight beds.

In order to prevent contagion stringent regulations were in force. Double doors were placed on the lobby at the entrance of the fever ward; the fever nurse had rooms on the same floor, and all intercourse between the fever and the general wards was as far as possible cut off. There was, however, only one staircase for all the wards, which was unavoidably used for the conveyance of the patients and their linen, diet, &c., and by the medical officers, and other officials. The fever ward was rarely empty, and often contained four or six cases of scarlet fever or its sequelæ. In these circumstances a state of things existed equivalent to a series of epidemics of scarlet fever; while the occasional inadvertent admission of a case into the general wards or the outbreak of the disease among the medical patients told in the same direction. Under these conditions the appearance immediately after operations of what all who saw the cases regarded as scarlet fever was frequently observed. Of forty-three cases of lithotomy by Mr. Thomas Smith, seven—that is, nearly 17 per cent.—were attacked, and so were many in whom other operations were performed.

But since the building of the New Hospital, the removal of the scarlet fever ward to an isolated block, and the separation of the medical and surgical cases, the disease in question has become very rare.

2.—Although children attacked with the disease under consideration were placed in the fever ward, in which the poison of scarlet fever must have existed in a concentrated form, they suffered no further infection.

3.—In some instances the features of scarlet fever were clearly marked, and the disease was of so severe a type that it proved fatal, either primarily or through its sequelæ. In all Mr. Smith's cases the rash was present, while in three there was desquamation, and in three severe albuminuria.

4.—The fact that the question has been considered by what I may term a jury of experts, composed of Sir James Paget, all the staff of the Children's Hospital (with whose opinion I am acquainted), Mr. Stirling (who has written a very able paper in the St. George's Hospital Reports), Dr. Goodhart and Mr. Paley of the Evelina Hospital, M. Trélat, and others, and that all have expressed their opinion that the disease is scarlet fever, goes far, I think, to carry conviction with it.

And here I would, in passing, observe that a belief in the peculiar liability of surgical patients to scarlet fever does not in any way clash with the opinion that eruptions of a non-scarlatinal origin occur after operations. Such rashes, whether produced by some form of blood-poisoning or by drugs, are undoubtedly met with, and have been already carefully observed and described. But I shall pass them by, for they lie beyond the range of the present communication.

This peculiar liability to scarlet fever has been manifested after every variety of operation, from the most trivial, such as tenotomy, the aspiration of an abscess, or the removal of a small sequestrum, to such as lithotomy, resection of the large joints, or amputation. Some observers have conceived the liability to be greater after operations on the genito-urinary organs, but facts are, I think, wanting to confirm this view. Assuming, then, that this peculiar liability may be regarded as established, the question now arises, Can this fact be explained? The one constant element in the problem is the existence of a recent wound. Let us, therefore, follow the subject by examining the action of the wound, first, in its purely local character, and, secondly, in reference to any influence it may exert on the general condition of the patient.

1.—It has been suggested that the materies morbi is conveyed by the hand

or instruments of the operator—that the process is one of true inoculation. Such an hypothesis is, at first sight, plausible; inoculation is a method by which poisons are readily introduced, and one which conduces to their rapid action. But the inoculation theory seems to derive no confirmation from the facts of the case. Operating surgeons are so rarely brought into contact with scarlet fever that it may fairly be held they rarely convey it to their patients. And in the Children's Hospital it seems much more likely that contagion was derived from cases occurring either in the same or a neighbouring ward. Even in an instance which occurred some years ago, in which the operator, whose own children had scarlet fever at the time, probably infected the patient, there is no ground for believing that the poison was conveyed by inoculation. It may reasonably be held that it was transmitted by the surgeon, just as it might be carried by a nurse from one child to another in the ordinary way. But though there is no direct inoculation, does not the wound, we may ask, serve as the portal through which the materies morbi enters, for the surface of a recent wound offers conditions very favourable to absorption? The answer seems to be that infection through the wound, at least in many of the cases, is improbable, first, because scarlet fever has been developed in instances which were treated with strict antiseptic precautions—that is, by the free application to the wound of carbolic acid—an agent which is believed to destroy the scarlet fever poison; and, secondly, because the wound has remained healthy, which, as Mr. Stirling remarks, would hardly be the case were it the passage by which the poison found an ingress.

Thus, as to the manner in which the poison is introduced, we may, I think, acquit the wound of any direct or local agency. And yet the fact remains that the presence of a recent wound and the appearance of scarlet fever are intimately associated; and we are led to infer that in consequence of an operation the condition of the patient is in some way changed, so that, if he is exposed to contagion at or near the time of an operation, not only is he in more than the usual degree liable to be affected, but the incubation period is cut short, and the disease almost immediately appears; while, if he has been in contact with the poison some time before, though he has hitherto escaped, he is now apt to be forthwith attacked. That contagion occurring at or near the time of an operation is followed by an abnormally rapid outbreak is a generally acknowledged fact, clearly confirmed by many of the cases at the Hospital for Sick Children, in which the source of infection could be distinctly traced, and in which the incubation period was not more than twenty-four hours.

As to the instances in which the outbreak immediately after an operation appears to result from a long previous infection, some have supposed them to be cases of prolonged incubation. But this seems a very doubtful hypothesis. Though our information as to the incubation of the exanthemata is far from complete, all that is known on the subject points to the conclusion that each has a period that is liable to but little variation, and that the period for scarlet fever is rarely, if ever, more than seven days. Many cases, however, have occurred in which the interval between the infection and outbreak appears to have been so extended, that, if we accept this limit of seven days as even approximatively correct, we cannot with any show of probability regard them as examples of prolonged incubation. They may, I think, be more satisfactorily explained on the theory that the patient had carried the poison in a latent form about him, and that it was only when an operation had (in whatever manner) altered his condition that he became amenable to an attack. If we may hold this view, the cases of remote (infection) concur with cases of recent infection in illustrating the same fact, that, in the words of Sir James Paget, there is something in the consequences of an operation which renders a patient peculiarly

liable to the influence of the scarlet fever poison. No formal proof need be offered of the vitality of the poison of scarlet fever. But how, it may be asked, does it exist, or what is its habitat during those considerable intervals that are sometimes observed between its reception and its outbreak? Is it lodged only in the clothes, or on the external parts of the body of the patient? or having been inhaled, or possibly absorbed from the surface, is it retained in situations where it is in close proximity to the circulation? The latter view would afford an explanation of the sudden way in which the disease (though imbibed long before) makes its appearance after an operation; for the materies morbi is already in the same intimate association with the blood current as it would be were it inhaled or absorbed by the patient at the very time at which the operation is performed.

As to the nature of the change produced by an operation I am unable to express any definite opinion. The subject is one that leads us into a field of inquiry in which at present very little has been discovered; for it has reference not merely to the phenomena and results of diseases, but to their primary and essential nature. We know the syphilitic poison by its results, its eruptions, nodes, and ulcers; and we know that its course and the features which it assumes are influenced by the constitutional tendencies of the patient: that gout and tuberculosis, for example, stamp it with different characters. And we have acquired similar experience in respect to other diseases. We know that some patients are so prone to measles or scarlet fever that they are repeatedly attacked, while others enjoy a complete immunity, however much they may be exposed to infection. And Sir William Jenner has pointed out that some families are peculiarly liable to diphtheria, and others to typhoid fever. Here, however, we are merely observing phenomena, and it is the same with regard to scarlet fever. We see that operations increase the liability of patients to an attack, but of the cause of this liability we are still ignorant.

Whatever the nature of the change may be, the probability is that it acts through the nervous system. The effect produced on the ordinary process of nutrition by nervous exhaustion following prolonged fatigue, loss of sleep, or severe mental distress is well known; and the same condition has often been seen to predispose individuals to cholera, fever, diphtheria, &c.; and Sir James Paget has recorded an instance in which a severe fright was followed in two days by a copious syphilitic eruption.

It is, I think, to some such change—of the nature of nervous depression—that this peculiar liability to scarlet fever after operations may most reasonably be ascribed.

Ueber die Natur des sogenannten chirurgischen Scharlach's.

Dr. Riedinger, Würzburg.

Mit Rücksicht auf die vorgeschrittene Zeit und die soeben gehörten Mittheilungen des Herrn Collegen Marsh werden Sie mir erlauben, dass ich mich so kurz als möglich fasse, und Ihnen meine Erfahrungen über den vorliegenden Punct mittheile. Es ist eine merkwürdige Thatsache, dass bis jetzt nur von wenigen Seiten auf die Complication zwischen Scharlach und Wunden aufmerksam gemacht wurde. Englische Aerzte waren die ersten, welche denselben erwähnten. Paget sah in kurzer Zeit zehn Mal Scharlach nach Operationen; Murchison und May machten dieselbe Beobachtung; ebenso Harrison und Wilks; doch hielten die beiden Letzten das nach Operationen auftretende Exanthem nicht für wirklichen Scharlach, sondern für eine, dem Scharlach

ähnliche Affection. Auch französische Schriftsteller berichten über Exantheme nach Wunden. Verneuil sah solche wiederholt bei septischen Processen auftreten; dieselben hatten eine ungemein schlechte Prognose, alle Kranken gingen zu Grunde. Martinet beobachtete ein solches bei einem an Osteomyelitis femoris leidenden Knaben. Der Ausschlag hatte Aehnlichkeit mit Urticaria, zeigte grosse, rothe, erhabene Flecken, welche bald rund waren, bald nur das Segment eines Kreises darstellten. Auch Legroux sah solche Exantheme bei Puerperalkranken, ohne dass jedoch die Prognose eine absolut lethale war. Verneuil, Raynaud und Dieulafoy erwähnen ausserdem noch das Auftreten von Urticaria nach Wunden. Dieulafoy allein sah sie 14 Mal unter 27 Punctionen von Lebercysten. Aehnliche, aber immer ohne Fieber verlaufende Affectionen finden sich bekanntlich auch nach Genuss gewisser Speisen.

Ein Haupterforderniss für unsern Gegenstand ist jedoch, dass wir genau zwischen diesen Formen unterscheiden und nur dasjenige Exanthem als chirurgischen Scharlach bezeichnen, welches die characteristischen Symptome der Scar latina bietet. Wenn es nun auch wirklich richtig ist, dass die Scarlatina einen sehr wechselvollen Symptomencomplex bieten kann,—Exanthem, Fieber, Angina, Abschuppung, Incubation ect. unterliegen den stärksten Schwankungen, so ist doch das Gesammtbild ein so characteristisches, dass man wohl im Stande ist, eine Differentialdiagnose zu stellen. Von L. Thomas und andern sind solche abnorm verlaufende Scharlachfälle gesammelt worden; doch will ich auf diese Puncte nicht eingehen; nur das eine soll hier berührt werden, dass man wohl nicht selten ein Erisypel mit Scharlach verwechselt hat. Auch über den ersten Fall, den ich zu beobachten Gelegenheit hatte, habe ich mich im Anfange getäuscht; ich will diesen hier kurz erwähnen. Es handelte sich um ein sechzehnjähriges Mädchen, bei welchem ich am fünften September 1877 auf der chirurgischen Klinik zu Würzburg ein grosses Lipom, welches in der Lendengegend sass, extirpirt hatte; die Kranke befand sich am Tage der Operation recht wohl, aber schon Tags darauf hatte sie starke Kopfschmerzen, eine Morgentemperatur von 40° und eine Abendtemperatur von 41·2°; der Puls erreichte die Höhe von 120. In Folge dessen wurde der Verband gewechselt und es zeigte sich eine starke Röthe von der Wunde ausgehend und auf den Nacken übergreifend, welche ich für ein Erisypel hielt; die Wunde war dabei nicht besonders alterirt. Die Schwellung im Hals, später Eiweiss im Urin und die characteristische Abschuppung fast am ganzen Körper stellten jedoch den Scharlach ausser Zweifel. Ein Mal auf diesen Zusammenhang aufmerksam geworden, war ich in der Beurtheilung der Röthe, welche von Wunden ausging, vorsichtiger geworden und schon im vorigen Jahre konnte ich in No. 9 des *Central-Blattes für Chirurgie* zehn Fälle aus meiner Praxis zusammenstellen, zu denen ich heute noch 5 weitere hinzufügen kann. Ich verweise auf meine diesbezügliche Mittheilung und füge hier ausdrücklich bei, dass in allen Fällen die Diagnose von andern Collegen bestätigt wurde. Auch von vielen Seiten höre ich jetzt, dass man ähnliche Erfahrungen gemacht hat. In vielen Fällen schloss sich der Scharlach fast direct an die Wunde an; in andern kam der Scharlach später zum Vorschein; der höchste Termin war 14 Tage nach der Verwundung. Interessant ist der Umstand, dass diese Complication mit Ausnahme von drei Fällen, *erwachsene* Individuen betraf und dass darunter allein vier Collegen sich befanden, die jedenfalls vorher schon vielfach mit Scharlachkranken in Berührung gekommen waren. In der Familie des einen hatten bei einer Epidemie alle Geschwister desselben, mit Ausnahme von ihm, den Scharlach durchgemacht. Im Grossen und Ganzen kann man sagen, dass der Scharlach immer ein schwerer war. Drei Collegen hatten sich septische Phlegmonen durch Verwundungen bei

Sectionen zugezogen. In einem Falle handelte es sich um die Section eines an Typhus Verstorbenen.

Wir stehen übrigens in der Chirurgie mit dieser Complication, nicht allein; auch in der Geburtshülfe hat man die gleiche Erfahrung gemacht. Olshausen hat vielfach Scharlach direkt nach Entbindungen gesehen und wahrscheinlich sind die Fälle von Legroux hierher zu rechnen. Fünf Fälle von den obenerwähnten waren antiseptisch behandelt worden. In vielen Fällen war oft weit und breit kein Scharlach vorhanden, und ein ganz besserer, hervorzuhebender Punct ist der, dass in zwei Fällen andere Personen in der nächsten Umgebung angesteckt wurden.

Die Hauptfrage, die sich an diese Thatsachen unmittelbar anknüpft, ist selbstverständlich die, ob die Kranken schon vor der Verletzung inficirt waren, oder ob dies erst nachträglich geschah. Man kann dies natürlich nicht mit voller Präcision für alle Fälle entscheiden. Für die meisten der vorliegenden Fälle dürfen wir den Scharlach, als von der Wunde abhängig, bezeichnen; jedenfalls würde die Ansicht, dass die Kranken vor der Operation schon befallen worden sind, gewiss nicht für alle Fälle passen, da die Incubationszeit dann für manche eine allzulange wäre; um somehr als wir wissen, dass durch Murchison festgestellt wurde, dass der Scharlach zu den Infectionskrankheiten mit kürzerer Incubationszeit gerechnet werden muss und sieben Tage die Höhe ist. Richardson und Trousseau berichten über sieben und achtstündige Incubation, und Gerhardt beobachtete ebenfalls ganz kurze Incubationszeit. Murchison sah in 75 Fällen fünfzehn Mal in den ersten 24 Stunden nach der Infection Scharlach auftreten. Der zweite Punct, der hier nicht ohne Erwähnung bleiben darf, ist der von wo aus das Scharlachgift in den Körper gelangt. Es liegt die Annahme, dass dies auf dem Wege der Wunde geschehen kann, gewiss nahe und wir müssen in dieser Ansicht um so mehr bestärkt werden, als, wie oben erwähnt, das Exanthem seinen Anfang von der Wunde nahm, und in deren Umgebung am stärksten ausgeprägt war. So gut dies bei einem Erisypsel der Fall ist, mit dem ja doch Scharlach, Diphtheritis nahe verwandtschaftliche Beziehungen haben, eben so gut kann dies auch bei unserer vorliegenden Krankheit der Fall sein. Weitere Beobachtungen, die in dieser Beziehung sicher jetzt in reichlicherem Maasse gemacht werden, als früher, dürften wohl schon in der allernächsten Zeit mehr Licht in die Frage bringen.

DISCUSSION.

Mr. Holmes, London: I have myself seen a good many cases of rash occurring after operations, although I have not preserved any numerical or other record of my cases. The result of this experience, as well as the reading of the literature on the subject, together with the interesting observations of Mr. Marsh, Herr Riedinger, and M. Trélat, leads me to the firm conclusion that persons after surgical operations are much more liable to scarlet fever than others, as stated by Sir J. Paget. This is the ordinary disease, and is in all probability contracted in the ordinary manner, though, as M. Trélat seems to think, it is possible that in some instances the inoculation may affect the wound itself. However that may be, the disease has the same essential features as scarlet fever, and, as some of Dr. Riedinger's cases showed, it may propagate scarlet fever to others. But I am equally convinced that many of the cases which are classified as "surgical scarlet fever" are not scarlet fever at all, but some of them the rash of pyæmia, some, possibly (as Dr. Riedinger has hinted), erysipelas, others anomalous roseolous rashes, without any symptom of scarlet fever, and some of these quite destitute of danger, or indeed of any importance

whatever. On the nature, symptoms, and causation of these hitherto unclassified cases, further experience is necessary in order to enable us to speak positively.

M. TRÉLAT, Paris: related particulars of three cases of scarlatina in children, which supervened after slight operations. He had not observed anything peculiar in the disease, and believed it to be nothing but an intercurrent affection, due to direct contagion.

Dr. GOODHART, London: I am quite in accord with what appears to be the now preponderating opinion that the majority of scarlatina-like rashes after operations are veritable scarlatina. I also think with Mr. Marsh that occasionally rashes closely simulate scarlatina, as the belladonna and other rashes may sometimes do, and which are not scarlatina. But these cases are so difficult to distinguish, that for practical purposes it is proper, unless there be unusually clear indications to the contrary, to isolate all patients affected on the appearance of the rash, for the benefit of surrounding patients. But I think we must all be more interested in the question how the peculiarities of the diseases are best explained. To this point I shall address myself; and, as most suggestive, the points which occur to me are chiefly these—(1.) That though such cases are chiefly found after operations (seventeen out of twenty-five of the cases collected by Mr. Paley and myself from the Evelina Hospital for Children were so), still that a fair proportion—one-third in our series—occur in other surgical cases, many of them not having even wounds. (2.) That *medical* cases are by no means exempt, and that cases of phthisis and such like, which have certain obvious relations with surgical cases, are more likely to be attacked than others. This has been so at the Evelina Hospital, and similarly in one of the wards at Guy's Hospital a year ago, a child with scarlatina was admitted by accident, and the only adult case which suffered was a bad case of phthisis. Now what have these cases in common? They are all of them cases of local inflammation, and it is this which I venture to suggest is the important factor. I might support this view if it were necessary by recalling to your minds the experiments which of late years have been made showing that inflammatory foci are good soils for the cultivation of fevers; but with those before me from France and Germany who have made the study of the natural history of fevers so peculiarly their own, it is unnecessary. I would rather appeal to two diseases which I think support me. Pharyngeal diphtheria is allowed to be particularly liable to take root in the subject of chronic tonsillitis and faucial catarrh; and, secondly, I would point to the disease we in England know as ulcerative endocarditis, but which is called diphtheritic endocarditis abroad. In the great majority of cases this is found, not as a primary disease of the valves, but as secondary to their chronic inflammation. It would almost appear as if germs of all kinds swoop upon foci of chronic inflammation like vultures upon a corpse. It seems to me possible that some such occurrence as this takes place in children after operations—takes place in persons the subject of any local inflammation; and thus those who have not previously procured immunity by passing through the disease are peculiarly liable to be attacked. In conformity with this, though no doubt admitting of other explanation, is the fact observed by Mr. Paley and myself, and subsequently by Mr. Stirling at St. George's, that children with wounds dressed upon the antiseptic system did not escape. Thirteen out of seventeen wound cases were dressed under strictly antiseptic precautions, and yet they suffered. Now, I think it is proved by a large body of evidence that such dressings are sufficient to eradicate certain diseases, as erysipelas, and from this I infer that they enter by the wound, and that the scarlatina poison does not. It will, I know, be objected to such a view, that many of the cases, though cases

of operation, have been subjected to so slight an operation as to be virtually exempt from its risk. Tenotomies, for instance, have not unfrequently been followed by scarlatina. I may appear to be somewhat straining my arguments, but it seems to be sufficient to meet this by saying that local inflammations, if they do produce a good soil, are perfectly well able to cultivate the disease, be they small or great. But no doubt severity within certain limits (for some very putrid inflammations appear to be destructive to germs) is favourable to this production, particularly if they are associated with much fever. I would also add that, from what I have seen in peritoneal exudations, there are few fluids so good for cultivation of bacteria as extravasated blood. The modifications which this interesting affection undergoes are also perfectly explicable, or, if not explicable, are rendered probable from analogy upon some such hypothesis.

Ueber Empyem-Operation bei Kindern.

Prof. C. GERHARDT, Würzburg.

Die Schicksale der Empyem-Operation waren wechselvolle. Sie wurde bald als lebensrettend gepriesen, bald als erfolglos verworfen. Schon in der hippocratischen Zeit zu hohem Grade von Vollkommenheit gebracht, (Ausspülung, Offenhalten der Wunde) wurde sie später häufig von der Masse verworfen, immer wieder von einzelnen hervorragenden Forschern in die Höhe erhoben. Eine Zeit lang schien ihr Schicksal mit dem der physikalischen Diagnostik verknüpft zu sein. Laennec und Skoda haben sich beide mit grossem Interesse dieser Operation zugewandt, später durch wenig günstige Erfolge entmuthigt, sie wieder verlassen. Ein wichtiger Fortschritt wurde durch die strenge Scheidung der Indicationen gewonnen: Punction mit Luftabschluss beim Prüfen, freie Eröffnung des Pleurasackes bei eitrigen Ergüssen. Gleichzeitig gewann durch die Probepunction mit Pravaz'scher Spritze die Diagnose des Empyems ungemein an Sicherheit. Unter einen neuen und glücklichen Stern trat der Brustschnitt mit dem Aufleuchten der antiseptischen Methode. Aber auch diesmal wurden die ersten Hoffnungen nicht erfüllt. Brustschnitt und tägliche Carbolausspülung der Pleurahöhle gaben wenig günstigere Resultate als Unterlassen jeder Operation (Statistik von Ewald). Nach diesem Scheitern der ersten Erwartungen bildete sich eine Anzahl weiterer Methoden aus, die in ihren Wegen weit auseinandergehen.

Ihre Berechtigung ist zum Theil nach der Auffassung einer Vorfrage zu beurtheilen, nämlich ob das Empyem einfach als ein, nach innen von den Rippen und nach aussen von den Lungen gelegener, Abscess anzusehen sei. Wenn das Empyem Nichts anderes wäre, wie ein gewöhnlicher Abscess, so müssten breite Eröffnung unter antiseptischen Cautelen, Ausspülung mit Carbollösung und Sorge für genügenden Abfluss ebenso rasche und sichere Heilung erzielen wie bei jedem anderen Abscess. Heilungen auf natürlichem Wege, ohne Operation, dürften nur äusserst selten vorkommen. Nun zeigt aber die Erfahrung, das kleinere eitrige Pleuraexsudate sehr oft, grosse noch hie und da durch einfache Resorbtion zur Heilung kommen, dass die Resultate des erwähnten Verfahrens in weit höherem Maasse die Gefahr der Carbolintoxication mit sich bringen, als dies bei anderen Abscessen der Fall ist, kurz, dass die Resorbtionsverhältnisse an der Pleura mit ihren offenstehenden Lymphgefässmündungen ganz andere sind, als an gewöhnlicher Abscesswand. Gelegentlich der Spontanheilungen sei hier noch jene schon von Hippocrates erwähnte, von Traube eingehend gewürdigte Beendigungsweise eitriger Exsudate betont, welche durch Aushusten des Eiters ohne Lufteintritt in den Pleurasack zur Heilung führt. Sie ist, wie ich glaube,

häufiger, als man gewöhnlich annimmt, kommt namentlich den Empyemen nach Pneumonie so oft zu, dass ich sogar bei diesen mit dem operativen Eingriffe etwas länger zu warten, mich berechtigt halte. Ein zweiter, gerade für diese Dinge wichtiger Gesichtspunkt ist aus den Ausdehnungsverhältnissen der Lunge abzuleiten. Einerseits ist das Pleuraexsudat um so leichter resorbtionsfähig, je geringer die Einschränkung der Ausdehnungsfähigkeit der Lunge, anderseits ist die dauernde Schädigung der physiologischen Leistung und anatomischen Structur um so geringer, je weniger die erzielte Heilung zugleich Einbusse an Ausdehnung und Ausdehnbarkeit der Lunge bedingt. Darauf hat aber die angewandte Operationsmethode bedeutenden Einfluss und man wird wohl sagen dürfen, dass diejenigen Methoden, die grundsätzlich das Einsinken der Seite begünstigen, mehr den Abscess als die Lunge berücksichtigen.

Die Methoden, welche gegenwärtig am meisten in Betracht kommen, dürften sein: 1. Freie Eröffnung der Pleurahöhle unter antiseptischen Cautelen, Drainirung, Abfluss unter antiseptischem Verbande. Die Oeffnung wird nach König's Methode in ihrem Bestande gesichert durch Resection einer Rippe. Nur bei zersetzter Beschaffenheit des Exsudates, Ausspülung und zwar nicht mit Carbollösung, sondern mit einer minder giftigen, desinficirenden Flüssigkeit, z. B. Chlorzinklösung. 2. Punktion des Pleurasackes, Ausspülung durch die Troicartröhre unter Luftabschluss mit einer indifferenten, z. B. Kochsalzlösung oder desinficirenden Flüssigkeit, wie Salicylwasser. Wiederholung des Verfahrens bei Bedarf. 3. Langsame Ausspülung. Ableitung des Exsudates durch die liegenbleibende Troicartröhre unter Carbol- oder Salicyllösung. Oeftere Erneuerung dieser Flüssigkeit. Eintreiben der Flüssigkeit mehrmals täglich durch Erheben des gewöhnlich tiefer stehenden Gefässes. Welche dieser Methoden den unbedingten Vorzug verdient, möchte heutzutage schwer zu sagen sein. Der eigentlich chirurgische Eingriff, wie ihn König entwickelt hat, verfügt über die zahlreichsten und besten Resultate, die bis jetzt aufzuweisen sind. Grössere Statistiken liegen weder vor über den direkten Erfolg, noch auch über etwaige Häufigkeit von Nachkrankheiten. Die zweite Verfahrungsweise ist nach manchen misslungenen Versuchen Früherer, erst durch Bälz zur brauchbaren geworden. Der Eingriff ist geringer, die Resultate sind weniger entscheidend, auch nicht so günstig; ausgedehnte Erfahrungen liegen noch nicht vor. Noch mehr gilt Letzteres von der dritten Methode, die von Playfair, wenn auch nicht ganz in dieser Weise angegeben, von Bäumler neuerdings ausgebildet wurde.

Fragen Sie, was hat dies Alles für das Kindesalter zu bedeuten? so lässt sich die Antwort in wenige Sätze zusammenfassen: Die Kräfte des Kindes werden früher erschöpft, Pleuritis purulenta ist für das Kind eine durchschnittlich acutere, gefährlichere Erkrankung. Die Indication zur Operation tritt daher früher ein. Die Prognose ist günstiger, weil man es mit Individuen zu thun hat von lebhafter Reproduction, ungeschädigten, inneren Organen, biegsamer Thoraxwand. Die Heilungsdauer nimmt kürzere Zeit in Anspruch, das Zurückbleiben von Fisteln und bedeutenden Missstaltungen ist weniger zu fürchten. Während bei Erwachsenen oft nur Abkürzung langen Siechthums, wesentliche Besserung in ernsten Leiden erzielt wird, tritt gerade im Kindesalter der lebensrettende Charakter der Operation bestimmter hervor.

Die chirurgische Behandlung des Empyems.

Dr. Adolf Baginsky, Berlin.

1.—Die pleuritischen Exudate des kindlichen Alters sind weit häufiger eitrigen Charakters, als diejenigen der Erwachsenen.

2.—Die Diagnose des Empyems wird nicht sowohl durch die physikalische Diagnostik und durch die Fiebertemperaturen, als vielmehr neben beiden durch die antiseptisch vorgenommene Probepunction festzustellen sein.

3.—Nach Feststellung der Diagnose durch die Probepunction ist insbesondere bei jüngeren Kindern (1–3 Jahren) nicht sogleich chirurgisch einzugreifen. Man beobachtet ziemlich häufig einen Durchbruch des Eiters durch die Lunge, (Entleerung reichlicher Eitermengen mit dem Husten) unbeschadet der späteren vollständigen Restitution sowohl der Lunge, als der Pleura.

4.—Die Indication zum chirurgischen Eingriff geben *a*) andauerndes Fieber, *b*) andauernder quälender Hustenreiz, *c*) Appetitlosigkeit mit Abmagerung.

5.—Die Punction (antiseptisch) mit einfachem, mit Condom zum Zweck des Luftabschlusses versehenen Troicart, ohne nachfolgende Ausspülung, genügt in einzelnen Fällen zur definitiven Heilung des Empyems. Diese Erfahrung weist darauf hin, insbesondere bei jüngeren Kindern (1–3 Jahren), die Punction jedes Mal vorerst zu versuchen, und bei Wiedererneuerung des Eiters dieselbe zu wiederholen. Die Punction erfolgt an einer möglichst tiefen Stelle des Thorax in der hinteren Axillarlinie. Es darf nicht der gesammte eitrige Inhalt des Thorax sofort entleert werden, sondern nur so lange darf man den Eiter fliessen lassen, als er im Bogen und unter stärkerem intrathoracischem Druck fliesst. Die Punctionsöffnung ist sorgfältig luftdicht mittelst Heftpflasters zu schliessen.

6.—Wenn nach 2–3maliger Punction das Fieber andauert, das Empyem sich wieder erneuert, die Kräfte des Kranken und seine Ernährung herunterkommen, ist zur Incision zu schreiten. Die Operation geschieht unter antiseptischen Cautelen. Langsam, mit präparirenden Schnitten vorgehend, wird die Pleurahöhle eröffnet. Die Wunde wird an einer möglichst tiefen Stelle, parallel zum Verlaufe der Rippen, entsprechend einem Intercostalraume angelegt, so zwar, dass der innere Winkel in der hintern Axillarlinie beginnt. Der Eiter wird zunächst durch freies Fliessenlassen unter Carbolspray entleert. Die Thoraxhöhle wird sodann mit einer 3 % Lösung von Acid. salicylicum sorgfältig ausgespült, bis die Lösung rein abfliesst. (—Carbolsäure ist bei der Ausspülung in der Befürchtung einer Carbolsäureintoxication zu vermeiden.) In die Wunde wird ein desinficirtes, aus einem mittelstarken Gummirohr bestehendes Drainrohr eingelegt. Dasselbe wird mit Faden und Heftpflaster an der Thoraxwand befestigt. Die Wunde mittelst antiseptischen Verbandes sorgfältigst bedeckt.

Die Resection einer Rippe kann bei Kindern in der grössten Anzahl der Fälle vermieden werden.

Ausspülungen des Thorax erfolgen nach der Operation und, wenn neue Fiebertemperaturen ohne anderweitige Complicationen, welche dieselben etwa bedingen können, auftreten, und wenn diesselben auf eine neue, auch physikalisch nachweisbare Ansammlung von Eiter hinweisen.

Die Erneuerung des Verbandes erfolgt sobald eine Durchfeuchtung desselben mit Eiter sich kund giebt.

DISCUSSION.

The PRESIDENT: With a view to making the discussion as practical as possible, I would suggest the following points as especially worthy of consideration:—(1.) The indications for thoracentesis in acute purulent pleurisy. (2.) The indications for the repetition of the puncture, and the comparative advantages and risks of frequently repeated punctures or of a permanent opening. (3.) The question of drainage; the best situation for the counter-opening (if any); and the further question of drainage tubes, or of means for keeping the opening patent. (4.) The employment of carbolised or other injections as a means either of restraining the discharge or of counteracting decomposition.

Dr. A. JACOBI, New York: In the course of a single year I have observed three cases of empyema in young children which required but a single aspiration. Complete recovery took place, though the quantity of pus in one case amounted to three or four hundred grammes. The flexibility of the young ribs is such that, even in those cases in which the lungs will not expand again to their former condition, the sinking in of the thorax is considerable enough to promote recovery. In adults I have resorted to the resection of large pieces of rib to accomplish the same end.

Professor RANKE, Munich: I think the opening of an empyema into a bronchial tube is of rare occurrence. The reason of my scarcely ever seeing it may, however, be that I never allow a case of empyema to run its course unchecked. It is my habit to explore every case of very considerable effusion into the pleural cavity which has lasted for several weeks with an ordinary morphine syringe. If the fluid extracted be pus, it is my habit to operate at once, without waiting for an aggravation of the symptoms; and I obtain excellent results by this method. I will endeavour to give my experience on some of the points Dr. West has mentioned. First, in regard to the question whether a free incision should be made, or only a small opening with a trocar. I always make an incision, through which I can easily introduce a tube to drain off the matter. As to washing out, in my first cases I invariably practised it, using either a one per cent. solution of carbolic acid, or a two per cent. solution of boracic acid; but latterly I have found that washing out is not necessary, and the cases do almost better without it. I have therefore given it up. I perform the operation under the carbolic spray, and apply strictly antiseptic dressings to the wound for about four weeks; after that time I treat the wound as an open one. Frequent tapping instead of drainage I have never tried, and am therefore not prepared to advise it. A counter-opening I have never made, because I never found one necessary. It has often surprised me to notice how the lung, which at first had been entirely compressed by the purulent effusion, will steadily expand again, the drainage tube having to be shortened gradually as the cavity grows smaller. Severe cases of empyema, however, often take a long time to heal—sometimes ten or twelve months. The deformity of the spine need by no means be great after a successful cure. In fact, I could show some of my cases in which the spine shows scarcely any deviation from the normal state. It is wonderful how completely a *restitutio ad integrum* can be brought about in a growing child.

Dr. ROBERT LEE, London: In the surgical treatment of empyema it is probable that in any particular case there will be little difference of opinion—that is to say, the necessity of making a permanent opening and inserting the drainage tube will be decided by the nature of the symptoms. The opinion has certainly been expressed by one who has had opportunities of watching cases of empyema during the period of convalescence, both after aspiration and the insertion of the drainage tube, that better results were obtained by repeating the aspiration than by the latter method. The question of chief interest seems to be the management of the tube, which, as generally used, is clearly opposed to the physical necessities of respiration. When it is remembered that the insertion of a tube through which air can pass freely into the thoracic cavity in the movements of inspiration must deprive the lung of the force of atmospheric pressure, on which expansion depends, the disadvantages of this mode of treatment are obvious. In the direction of meeting this difficulty and in observance of the physical requirements of these cases there is still much to be done, so that, if possible, the

advantages of aspiration may be combined with those of the free opening and the drainage tube—that is to say, a method by which a constant and gentle pressure can be exerted on the lung tissue. That the theoretical considerations of this subject are supported by practical observation I have been able to prove in one case of a child of four years of age, who wore an elastic cup over the opening of the tube, which cup the child replaced when it became detached, as it did at intervals of between three and four hours, apparently finding some comfort from the atmospheric conditions thus obtained in the thoracic cavity. I mention this view of the question of the treatment of empyema as the one which seems deserving of consideration in the surgical treatment of the disease.

Mr. F. Richardson Cross, Bristol: Early removal of pleuritic effusion is necessary to encourage re-expansion of the lung. If aspiration shows pus, and a second aspiration is not followed by recovery, free incision in the ninth or eighth interspace to allow continuous drainage through a tube is indicated. If this is done under Lister's antiseptic method, the pus will be replaced by serum, the wound will soon heal, and perfect recovery of chest shape and lung power will result. Three cases treated on this method, females aged $2\frac{3}{4}$, 15, and 18 years (the only ones the author has had an opportunity of treating), were quoted. The wounds had healed in six weeks, seven weeks, and three weeks respectively. Two months afterwards one chest had returned to its perfect state, the most favourable case was lost sight of, and the third and most chronic showed fair breathing and was improving.

Mr. R. W. Parker, London: As the question of treatment must very much depend on the mechanical condition of the chest, it would be well to divide empyemas into two chief classes, as found (1.) in children, and (2.) in adults. For we have to remember that whatever method of treatment is adopted, we cannot expect a favourable result unless the conditions regulating chest movement assist us. Thus an empyema cavity cannot be emptied unless the lung can re-expand or the chest wall fall in. In children these conditions obtain more commonly than in adults, and hence the disease is much less serious. In old people, whose chest walls are very rigid, empyema is a serious disease, and often all but incurable. I believe that aspiration—two or three times repeated if need be—is the best treatment in childhood, and ought always to be adopted before other measures are tried. No doubt the next best mode of cure is the expectoration of pus through the lung; but bearing in mind how this is brought about, it is hardly safe to postpone treatment until it takes place spontaneously, and unfortunately there are no mechanical means by which it may be hastened. I have seen many cases in which the pus has been evacuated in this manner, and I think it occurs, to some extent, more frequently than authors seem to admit. When aspiration repeated two or three times has failed (or sooner if the collection is large or if the patient seems to be losing ground), a free incision into the lowest and most dependent position of the empyema cavity, with antiseptic precautions, is called for. In some cases, antiseptic precautions notwithstanding, decomposition takes place and the discharges may become exceedingly fetid. For such cases washing out the cavity is then necessary. A solution of quinine (5 grs. – ℥i.) will be found the most serviceable. If the patient be a small child, it may be placed in a bath up to his neck; in this way the abscess cavity may be most thoroughly washed out. In adults I also advocate aspiration before other treatment is tried. But if the cavity is large and the chest walls rigid, I believe that great advantage is to be obtained by injecting filtered and carbolised air into the pleural cavity; for while this helps to displace the fluid, it also lessens the dragging sensation often complained of after tapping;

and by supporting the delicate vessels in the pyogenic membrane, it lessens subsequent exudation, and thus the risk of reaccumulation.

Professor GERHARDT, in reply: My final remarks refer chiefly to the points to which our President directed attention. The successful cure of empyema by expectoration through the lungs (after the manner mentioned by Traube) is much less common in children than in adults, but it certainly does sometimes occur in early age. Antiseptic washing out appears to me only necessary when decomposition of the pleural contents occurs at an early date. In like manner, the need of a counter-opening is only felt in special cases, as, for instance, when great narrowing of the first opening occurs, or a fistula leading to a small cavity becomes established. I am very glad that in the course of the discussion on the mechanism of respiration the difficulty of re-expansion of the lung by atmospheric pressure was dwelt upon. For the present, I believe that the method of treatment by incision promises the best results, but I cannot think that this will permanently be the case.

The Surgical Vice-President, Mr. HOLMES, then summed up the debate. After rendering the best thanks of the Section to Professor Gerhardt for his admirable treatment of the question, he said that the queries propounded by Dr. West seemed to him to cover the whole field of the debate. To most of these questions very clear answers had been given by Dr. Gerhardt, and, as far as he could discover, the following conclusions on them had been accepted by the majority of those present. It seemed to be admitted that simple aspiration of the pus is often successful, and that it is, on the whole, the most successful method of treatment, simple cases requiring no permanent opening into the pleura. The practice of different surgeons seems to differ as to the frequency with which the puncture should be repeated, but most seem in favour of not repeating it unless the symptoms are obviously increasing. That the pus may be spontaneously expectorated seems admitted on all hands, and also that this is not so common as to form any real indication—*i.e.*, that we ought never to take this into account surgically, but operate as the symptoms seem to require. Some of the speakers (*e.g.*, Dr. Ranke) seem more in favour of a permanent opening by means of a canula, the mouth of which is kept always "antiseptic." Permanent opening by incision seems chiefly indicated when there is a portion of the pleura in which there is an encysted collection, and the lung cannot rise into the vacuum formed by the aspirator. In such cases it seems also right to wash out the cavity, either with weak carbolic lotion, with boracic acid, or (as recommended by Mr. Parker) with purified air. In these cases the cavity probably gets obliterated by adhesions, and as these adhesions contract and cicatrise, they probably draw the lung towards the ribs, as well as the reverse. Counter-openings and the retention of drainage tubes in the chest seem rarely necessary, and then mainly in the case of encysted collections. If it be necessary to make a counter-opening, it seems that this should be at no great distance from the other, and not too near the spine. The prognosis and treatment of the resulting deformity was hardly touched on in the debate. In fine, the whole discussion seemed to prove the gratifying fact that the treatment of empyema in early life had become much more successful than formerly.

Observations on the Pathology and Treatment of Genu Valgum.

Dr. WILLIAM MACEWEN, Glasgow.

Flat-foot has been put down as a characteristic of genu valgum, whereas in the great majority of instances the foot is highly arched. Indeed, in all cases where the person is able to walk much, the foot is found arched. In children, who can walk very little, the foot is flat; but as these children grow older and stronger, the genu valgum continuing, the arch of the foot develops and increases. All such persons wear the outside of the heel of the boot, the inside being little touched. The high instep in genu valgum results from the great muscular action required to maintain the equilibrium of the patient while walking. When such a patient walks with nude feet, one perceives that the toes seems to grasp the floor at each step.

Genu valgum cannot be accounted for by a single pathological character. The internal curve of the lower third of the femur is the most common factor in its formation, being met with in 120 out of 166 limbs affected with genu valgum. There is likewise an abnormal elongation of the internal condyle in most cases, but this is to a much slighter extent than was formerly supposed, and it is very seldom sufficient to account for the whole of the deformity. In about 10 per cent. of the cases the head of the tibia is set askew on the tibial diaphysis. In aggravated cases of this kind there is generally a prominent portion of bone resembling a spine projecting from the inner side of the tibia about the position of the insertion of the internal lateral ligament. Where such exists, the limb is rendered more symmetrical by its removal.

The age at which osteotomy for genu valgum ought to be performed.—The question is often asked, at what age ought the operation to be performed for the cure of genu valgum? It is impossible to fix a hard and fast line at any age during the period of growth, and say that on one side of this line operations for genu valgum ought to be performed, while on the other they ought not. It depends on the state of the bones, and each case ought to be considered on its own merits. The question may be answered by stating that no operation for genu valgum ought to be performed during the period of *ramollissement*. Attention to this would at once prevent the performance of operations on patients who might recover by good hygiene and simple remedies, and at the same time it would do away with the likelihood of any return of the incurvation after the performance of the operation. I have very seldom performed operations for the relief of genu valgum under eight years of age, but I have refused to operate on much older patients who bore traces of an acute and recent attack of rickets, and whose bones were still soft.

Some surgeons are stated to operate for genu valgum at two and three years of age. Personally I deprecate such a course, believing that many such deformities occurring at that age disappear by attention to the bodily health along with a little care on the part of the parents. Therefore many such children are subjected to an operation which might be done without. Again, the bones in such young patients are soft and easily yield, and the deformity, though remedied for the time by the operation, would be apt to return, especially after an attack of measles, scarlet fever, or other disease incidental to childhood. Lastly, when the bones are so soft as "to permit the chisel to be pushed through easily by the hand alone," as some surgeons say they do, it must be apparent that under these conditions the operation could easily be avoided, and some simple splint or manipulation substituted.

The results of supra-condyloid osteotomy, as performed by me for genu

valgum, have been excellent. The advantages of the operation and the results may be summed up as follows :—

A deformity of a very aggravated kind has been removed ; the height of the patient has been increased, often by several inches ; the patients have been able to walk easier, much better, and a great deal farther than they could prior to the operation. As a consequence they improved in health and strength, and in many instances they have been enabled to occupy situations from which they had previously been debarred. It would be out of place to give cases in detail, especially as they are numbered by hundreds, but by way of illustration the following may be quoted :—

A lad, eighteen years of age, was affected with genu valgum to such an extent that a horizontal line drawn from the internal malleoli measured 18 inches, while a vertical line running from the horizontal one to the apex of the knees measured 15 inches. He could neither walk nor stand for more than five minutes at a time, and was totally unfitted for work. After supra-condyloid osteotomy his limbs were straight ; he could walk and leap with ease, the movements of the knee-joints being perfect. He was several inches higher than he was prior to the operation. A year afterwards he several times walked eighteen miles at a stretch without fatigue. He is now engaged as a mechanical engineer, in regular employment, besides walking six miles daily while going and coming from his work. A man twenty-five years of age, affected with double genu valgum (15 inches between the malleoli), had to give up his work, some time previous to his admission into my ward, on account of his inability to walk to and from his work and the difficulty of standing at his employment. After supra-condyloid osteotomy his limbs were straight, his knees were freely movable, his stature increased, and he was capable of doing his work with ease. A year afterwards he walked in four consecutive days over one hundred miles without undue fatigue. A third, affected with marked genu valgum, could scarcely go with the help of crutches from his own door to the shop at which he wrought, situated about thirty yards distant. There he had a sedentary occupation. After operation, besides having straight, freely movable limbs and being increased in height, he was able to walk well without any support. He shifted his residence, walking then four miles daily to his work. At one time he walked ten miles at a stretch without fatigue.

Do the femora continue to grow after supra-condyloid osteotomy ? Yes, they do, just as though there had been no operation performed. There are hundreds of cases where the increase has been distinctly marked. For instance, one boy aged eight years, affected with genu valgum, measured four feet in height ; after supra-condyloid osteotomy he measured four feet three inches. Four years after, he was measured, and found to be four feet ten inches, the femora being proportionately increased.

Up to April 6, 1881, there have been 767 limbs osteotomised by me, and in these 1149 osteotomies were performed. Out of these there were three deaths from causes independent of the operation (pneumonia, diphtheria, tubercular meningitis), and published in detail elsewhere ("Osteotomy," p. 165). Of these 767 limbs which were operated on, 521 were for genu valgum, the supra-condyloid method being employed.

This operation has been performed many times in Scotland, England, and Ireland, and very favourable accounts have been received of it from Germany, Italy, and America.

[illegible]

[illegible] of the knee-
[illegible] in appearance
[illegible] from [illegible] of the femur and
[illegible]

[illegible] under two forms—1.) the
[illegible] in its production;
[illegible] the primary disturbing
[illegible] has taken place,
[illegible] to effect locomotion
[illegible] as well in the rachitic
[illegible]

[illegible] to the fact
[illegible] of parts of the knee-
[illegible] the majority—of disordered
[illegible] in their later stages.
[illegible] disease of the
[illegible] the relations of parts
[illegible] termed genu
[illegible]

[illegible] as many clinical varieties of
[illegible] the former, that the distortion may
[illegible] in one limb, from
[illegible] the sound one is overladen and
[illegible]

[illegible] in the one-year-old fast-growing
[illegible] and from too watery a
[illegible] upon congenital rachitis, upon infantile
[illegible] less frequently in the later
[illegible] terminated by measles, hooping-
[illegible] to resort
[illegible] On the approach of puberty in
[illegible] period, say from the twelfth to the
[illegible] sets in.

[illegible] the occurrence of white swelling (strumous
[illegible] of the joint in the flexed position,
[illegible] with corresponding eversion of the
[illegible] With advancing years, the rheumatic
[illegible] were regarded as strumous in their youth, besides
[illegible] in the flexed position, is apt to assume a distinctly valgoid
[illegible] the neighborhood of the internal lateral liga-
[illegible] especially when attempts to use the limb are made,
[illegible] is not checked by art.

[illegible] several cases of considerable genu valgum in tall, robust
[illegible] and [illegible] affected with polyarcia, inordinate height and weight
[illegible] in unequal proportions to the distortion.

This frequent liability to genu valgum under so many conditions does not
[illegible], as often asserted, from the natural form and relation of the component
parts of the limb, and especially of the articulating surfaces of the knee-joint;
for in the normal state, in a well-knit knee, the active and passive structures, the

moving and resisting powers, are so well balanced that the most perfect symmetry, and a large reserve of capability for use beyond the average use exists. It can no more be admitted of genu valgum than of congenital club-foot, as has been stated by some surgeons, that every child born into the world has a certain degree of, or a tendency to, both those affections. All that can be admitted is, that when disorder or disease of the knee or foot takes place and distortion ensues, the form of the distortion will be determined in a certain direction, rather than in other directions, by the natural anatomical relations and functions. In the causation of genu valgum the natural greater size of the internal condyle, the naturally adducted position of the femur in relation to the trunk and to the tibia, the asserted naturally less developed condition of the external articular surface of the tibia, the known greater physiological range of adduction of the tibia over adduction in some positions of the knee, cannot be regarded as primary causes, and should not lead the surgeon to the employment of a particular operation, say, for example, that of detaching the internal condyle from its surrounding relations as a means of cure.

The author equally denies the primary influence of contraction of any muscle, *e.g.*, the biceps femoris, in producing the distortion, and denies the necessity for tenotomy, except in rare and extreme cases.

He places the two more loudly proclaimed causes of genu valgum—viz., enlargement of the internal condyle and contraction of biceps, in the rank of consequences of in-knee, and as secondary obstacles to restoration.

The most important secondary obstacle is that of diminished development of the external condyle, and of the corresponding external articulating surface of the tibia, both of which are caused by the weight of the body during standing and walking being from the earliest stage of genu valgum unduly borne by those parts of the articulation. As time advances, the distortion remaining uncured, the continued compression to which these parts remain subjected occasions a continuous increased disproportion between the size of these compressed parts, and the internal articulating parts of the femur and tibia. Indeed, the internal parts of the femur and tibia from ceasing to bear a due weight, and being instead exposed with the internal ligament to undue stretching and dragging, exhibit undue growth.

From the operation of these causes it is constantly observed in the two-year old infant, or older, affected with statical or rather "non-rachitic" genu valgum, that there exists a gap between the external condyle and corresponding part of the tibia, when the knee is gently straightened by the hand of the surgeon, into which, but for the integuments, fascia, and ligaments, the tip of his finger could be deeply introduced. When the bones of a severe genu valgum in an anatomical cabinet are disarticulated, and are placed as nearly perpendicular as may be, in their natural relation, the gap just alluded to is represented by a triangular space between the external articulating surfaces, the depth of the base of which may measure from one quarter of an inch to one inch according to the severity of the case. This coign of disadvantage constitutes the grave anatomical fact which the surgeon in the treatment of the majority of cases, especially in the non-rachitic ones, has to keep in view.

In the bones of rachitic cases this gap is less marked, probably because the bones in the softened stage had moulded themselves under the pressure of gravity towards one another, and because the curvatures in the shafts induced by gravity have often more to do with the valgoid form of the limb than the essential parts of the articulation. In fact, the author would deny to rachitic leg deformity with in-knee any other term than *spurious* genu valgum, and

would consider true genu valgum to be that statical form in which the joint and its active and passive structures are alone affected.

Leaving out here all mention of adjuvant, dietetic, and hygienic influences, and the narration of all the known symptoms of in-knee, the author may state that a continued experience of over forty years has shown him that to cure in-knee two things are necessary—to keep the knee extended during the treatment by an inflexible apparatus, and to employ a similar contrivance to produce or to maintain, as the case may be, adduction of the leg. The opposite surfaces of the external articulating portions of the femur and tibia being held apart, nature fills up the gap between them; probably she ceases meanwhile to add to the inner side of the joint. The choice of apparatus rests with the surgeon. The author has already described elsewhere that which he has commonly employed.

We may here remark that genu valgum may be arrested and cured by being maintained in a bent position, as C. Hüter has stated. Better still would be to apprentice the patient to a tailor, in the work of which the patient needs to sit *à la Turque*. We have found advantage in causing children disposed to knock-knee to sit in that manner, and do much of their ciphering and reading in that position.

Infants even who cannot walk evince no pain or inconvenience from the treatment; in them the apparatus may also be removed for a couple of hours night and morning, it being permitted to them to spend their day upon the floor. The rule of treatment should be, "*arte non vi*" and "*nullum retrorsum.*" He would remind the surgeon of an axiom of John Bell, applicable to our present subject, to the effect that "force is only successful where it is not needed." If the utmost gentleness be used, nature will do her work well with the utmost docility. All bandages and straps require to be applied with a comparative looseness that may seem ridiculous to the inexperienced. Infantile cases are commonly remediable within two or three months, not needing afterwards retentive apparatus. Children of two or three years and upwards, able to walk, may wear the apparatus day and night, and walk about as much as they like in the day time, with reasonable rest, the apparatus being removed morning and evening for a *few minutes* only for cleanliness, and to enable the nurse twice daily to bend the knee-joint *once*. In all cases the patient may follow his nursery, school, or business vocation; as soon as it is found that the knees evince no decided tendency to return to the valgus form directly the controlling apparatus is removed, a gradually extended permission to habitually use the parts in full mobility may be given. It is rarely necessary in any but the severest, neglected, or unrelieved cases of non-rachitic genu valgum, even in the adolescent up to the age of sixteen or seventeen, to confine the patient to the couch or bed, to resort to tenotomy, or to forcible examination, or forcible straightening under anæsthesia.

When such rapid forcible measures are employed, much is apparently gained at the moment, but apparatus is much less easily borne after such treatment than without its use, and in the long-run the tortoise is apt to outrun the hare. If force be absolutely necessary, the author prefers the more modern form of applying force—supra-condyloid osteotomy, especially as carried out by MacEwen of Glasgow; for when this operation is properly performed, and the limb is secured in a straight position, a gap is formed on the external margin of the femur at the place of division, and the external condyle is lowered to a corresponding amount. By the consequent change of relation of the leg, the tibia is adducted and the genu valgum disappears. The gap in the femur artificially made is substituted, as it were, for the analogous gap felt in the straightened

gum in the young by the surgeon's hand when the laxity of the joint ihe manœuvre.

: MacEwen's operation, nature fills up the triangular gap in the femur s, just as nature, by a return to natural growth where wanted in the d tibia, fills up the gap between the external condyle and the opposing ng surface, when the surgeon, with the aid of mechanical apparatus, osteotomy, has succeeded in holding those structures asunder, so as to er to act.

anical treatment which can be carried out by the mother and nurse, perly instructed and watched by the surgeon, is the proper remedy ases of genu valgum in early childhood, whether or no rachitic. The umber of young adolescent cases of genu valgum, when taken in time, quickly recoverable without osteotomy. When, however, they have g neglected, when they have lost all elasticity of structures, and the nt of size of the external condyle and opposite articulating surface of onsiderable, when the patella has abandoned its natural groove between yles, MacEwen's operation is indispensable.

otomy is then the complement to mechanical treatment, and it will e reader who has followed the author's description of the physical o cure afforded by the deterioration of the outer half of the articulating faces, that correct instrumental treatment and supra-condyloid osteo- rk towards cure in the same groove as regards "indication" of treat-

e rachitic cases termed genu valgum, but in which considerable curva- high and leg bones has more to do with the distortion than any changes nee-joint, after the age of seven or eight years, when instrumental treat- effect little improvement in the *curvatures*, multiple osteotomy can relied on. It should, however, be remembered that a moderate f permanent curvature of thigh and leg bones is not incompatible with use in after-life.

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the Pathology and Treatment of Genu Valgum.

Mr. B. E. Brodhurst, London.

valgum, or knock-knee, is a protrusion of the inner condyle of the vards, through relaxation and yielding of the internal lateral ligament nee-joint. It is a constitutional rather than a local defect, and it is most part accompanied by a relaxed condition of the ligamentous

valgum is, for the most part, preceded by flat-foot, and it may be said duced by it; for, as in flat-foot, the ligaments on the inner side of the those in the sole of the foot have yielded and become lengthened, so rch of the foot is more or less obliterated, and the foot itself everted, malleolus is made to project inwards, and the shaft of the tibia is con- inclined outwards. But, because the tibia is inclined outwards, the the femur is necessarily inclined inwards, and the superincumbent no longer transmitted through the long axis of the femur and of the e arch of the foot, but it is directed unduly to the inner side of the is borne on the inner margin of the foot. Thus, in consequence of

the patient rests the foot and of the tibia and the femur, the internal lateral ligament of the knee-joint yields and becomes elongated, and the condyle protrudes. Thus the condyles assume an oblique position in the articulation, the inner condyle being very much lower than the outer condyle.

The examination of numerous femora shows that there is generally some difference in length of the two condyles, and that the inner is somewhat longer than the outer. But it is also found that in the finest formed and strongest femora there is scarcely an appreciable difference in the length of the two condyles. Such a femur is in the Hunterian Museum, No. 320. This specimen is 20 inches in length, and its condyles occupy the same plane. On the other hand, a rickety femur may present an inner condyle one inch longer than the outer. This may be observed in a crooked and stunted femur, No. 2872^c, also in the Museum. In this instance the outer condyle is flattened, and the inner is elongated and bulging-shaped. These specimens may be seen varying in every degree between the most perfect bone and the most rickety. A moderately healthy bone shows a difference rarely of $\frac{1}{16}$ths of an inch.

It is not to be understood that because the inner condyle is $\frac{3}{16}$ths of an inch longer than the outer there is therefore genu valgum, for such is not the case; but rickets attacks very widely throughout the population, and this is one of the signs of it.

When genu valgum is first observed, the foot rests flat on the ground, the arch has disappeared, and the inner edge of the foot is in contact with the ground. But if now the weight of the body is removed, the child being raised from the ground or placed horizontally, the knee will resume its normal shape and the foot its arch. It will disappear in the same manner as, under similar circumstances, flat-foot will disappear.

As long as deformity becomes permanent and obliquity of the femoral condyles increases. There is no new formation, but pressure is somewhat increased on the outer condyle, and this becomes atrophic, while it is diminished on the side of the inner condyle. And at the same time the biceps tendon becomes tense in proportion to the deformity and the use of the limb; and, later, the external lateral ligament becomes tightly stretched, like a cord beneath the skin, rigid and unyielding, and the iliotibial band is drawn into a sharp line projecting on the limb.

Such, then, being the relative position of the bones and the soft structures, the question to be solved is, How shall the limb be restored so that a right line may pass through the long axis of the femur and the tibia and impinge on the dorsum of the foot?

Genu valgum is not often merely a local defect. It may, however, be induced by rickets. Thus it is seen with flat-foot. For the most part, however, the ankle drops and the foot is flattened and everted, as may be observed in figs. 13 and 14.

In the treatment of genu valgum, it is essential to restore the arch of the foot. Unless this is done, the knee deformity will return.

There are especially three modes of treatment which have been adopted, and to which I will call attention—namely, forcible straightening of the valgous knee, osteotomy, and tenotomy.

I will not refer to the mechanical treatment, for time does not permit of it, but in considering this question it ought not to be ignored.

Forcible straightening (*redressement forcé*) has been applied both to children and to adults. Eighteen years of age is fixed by M. Delore as the limit at which this mode of treatment may be practised. He directs that the limb is to be placed on the outer side, when, the foot being raised from the table,

manual force is to be applied to the inner side of the knee in a series of jerks. The limb is straightened in from five minutes to half an hour, and to effect this as many as eight assistants may be necessary. The epiphyses of the femur and the tibia are separated by this violence, and the external lateral and the crucial ligaments are ruptured, the external popliteal nerve may be torn through, the periosteum is lacerated, and suppurating periostis has followed; violent inflammation of the joint has been induced, and in a large number of cases a useless limb has resulted.

Rizzoli of Bologna endeavoured to straighten these limbs by instrumental means, force being applied suddenly; and Bardeleben employed an apparatus consisting of two cylinders enclosing the limb, which were applied during flexion. The limb was then forcibly extended and thus straightened.

I am compelled to conclude that such treatment is not surgery, and that, notwithstanding famous names, it is not likely to find followers at the present day.

Osteotomy has been practised by some in every case of genu valgum, even

Fig. 13.

Fig. 14.

in the earliest years. Thus, some operate at the third year; others not earlier than the sixth year; while again there are those who prefer to operate at the ninth or the tenth year. At this age, and after, it is thought to be right to perform any number of osteotomies. Thus the femur, the tibia, and the fibula may be cut through, and perhaps in more than one place; or a wedge of bone may be removed from the shaft, and the bone then forcibly straightened, or the inner condyle may be divided. Thus, one author says: "Having operated on 557 limbs belonging to 330 patients affected with various osseous deformities requiring osteotomy, a few observations on the results may not be without interest. Of the 330 patients, 220 were affected with genu valgum, presenting 367 limbs for treatment. The remaining 110 patients were made up of bow-

limb. The tendon of the biceps muscle is to be divided in one operation, and the ilio-tibial band in another. Sometimes in one limb, sometimes in the other, and sometimes in both limbs, the operation must be performed in three stages. With few exceptions, however, the division of the tendon of the biceps muscle, and the ilio-tibial band, will be sufficient to remove the deformity, or to render the limb straight enough for all practical purposes.

Although the operations for genu valgum are by no means harmless, they are not dangerous. Extensive and unnecessary injury rarely happens. Lameness frequently is complained of for a few days after the operation. Partial anchylosis very frequently follows the division of the tendon of the biceps; and sometimes, but not often, the division of the ilio-tibial band. Neither one deformity nor the other is a matter of life or death. Still, I think, good reasons exist for preferring the operation for genu valgum. Many persons who have these deformities think little of them, and would not submit to the slightest operation for the purpose of removing them; but those most frequently do submit to the operation who find the deformity remains as bad, or worse than before. This fact is shown in fig. 14, which represents a limb that had been divided by a skilful osteotomist, but which remained as bad after the operation as before.

The operations have at all times been performed with what may be desirable, but these do not appear to me to be what may be said to be necessary for the cure. Even when the joints have become dense and indurated,

—the division of the biceps tendon, the ilio-tibial band, and the capsule of the knee-joint—enables us to remove the deformity, and to restore the limb to its natural position, either immediately or gradually.

The operation is simple and safe. Any lateral motion of the bones will keep the divided parts again become solid, without the intervention of texture.

Where there is indicated in severe forms of genu valgum, the patient feels at ease, and the limb may be restored both in appearance and in use. There is neither violence nor mutilation, nor is there

In a small number of cases that it is necessary to have the tendon divided in its entirety. Usually it is sufficient to divide I have never known any difficulty in restoring the with a smaller division than of the biceps tendon

years of age.

We are sometimes embarrassed with femoral and tibial curves, these also I have referred to. It has been my object not to complicate this question, but to treat only of genu valgum.

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* Mr. Hey's "Osteotomy," p. 161.

On the Pathology and Treatment of Genu Valgum.

Mr. HENRY F. BAKER, London.

Great interest has been excited amongst surgeons during the last few years regarding the pathology and treatment of genu valgum, owing to the introduction of several new and ingenious operations involving the division of the bones forming the knee-joint, some by severing the internal condyle of the femur and pushing it up, and others by division of either the femur or the tibia and fibula, or all of them, and then placing the leg straight without any wound of the joint.

The consideration of this subject would, I think, be much simplified if the term "genu valgum" were restricted to that variety of knock-knee in which the femur and the tibia are both straight, and the deformity takes place at the joint itself, and thus some other name should be devised for the other kinds of knock-knee which depend on a curvature of bone not affecting the joint.

During a residence of five years at the Royal Orthopædic Hospital nearly 800 cases of genu valgum have come under my notice, and in nearly all of these the deformity took place at the joint itself, and was quite independent of the curvatures of the bones that were sometimes present in the same patients, and it is to this class of cases that I wish to confine my remarks on the present occasion.

The great question to be decided with regard to these cases is whether they can be cured by gradual stretching by means of splints or instruments with or without subcutaneous tenotomy; or is it necessary to resort to the heroic measures that have been advocated of late, and to divide the bones forming the knee-joint either with or without entering the joint? Although it must be allowed that a wonderful immunity from danger has on the whole followed osteotomy, it must at the same time be remembered that cases have happened in which suppuration followed by anchylosis has taken place, and in one recorded case even death has occurred; and I think from the large number of cases in which osteotomy has been practised, it cannot have been confined to very severe examples of this deformity.

Believing, as I do, that almost all cases of genu valgum can be cured by a course of treatment which, when carried out with reasonable care, is devoid of all risks whether to life or the free use of the joint, I am pleased to take advantage of this occasion to place before the Congress in a very few words my opinion as to the points that are necessary to be attended to in the treatment of genu valgum without division of bone.

The great majority of cases of this deformity that come under the notice of the surgeon are not of a severe nature, and on the knees being placed in the extended position and allowed to touch each other, the distance between the inner malleoli will be found not to exceed three or four inches. When this is the case, a very simple and inexpensive mode of treatment is all that is required to ensure a cure—viz., wooden splints applied to the outer side of the limbs, and extending from the hips to rather below the ankles, and having straps and buckles attached to them, by means of which the knee-joints can be gradually drawn towards the splints. If the patient is kept always entirely off his feet during the treatment (and this restriction is absolutely necessary), the deformity will soon be found to yield.

In those cases which are rather more severe, with from four to seven inches

between the malleoli (and indeed in the less severe cases if the expense of the instrument can be afforded), a special instrument should be prepared and made in such a way that it should keep the knee in the *extended* position, and prevent the leg *rotating*, at the same time that, by its means, the knee-joint can be acted upon in the *lateral* direction with sufficient force to gradually overcome the deformity. An instrument that was shown by me at a meeting of the Medical Society of London in February 1880, and a description of which was published in the *British Medical Journal* on May 22, 1880, carries out these conditions efficiently.

Cases are occasionally met with of so severe a nature that it is impossible to overcome the deformity, owing to the contraction of the biceps femoris, and sometimes of the external lateral ligament and the ilio-tibial band. In these cases it is necessary to divide one or other of these structures subcutaneously, and afterwards to gradually extend the limb. After the deformity has been overcome by the means indicated above, it is absolutely necessary that for some time the knee-joint should be supported, or a relapse will assuredly take place. A large number of cases in proof of the efficiency of the modes of treatment that I have advocated here can be seen in the practice of the surgeons at the Royal Orthopædic Hospital, where during my residence I have become convinced of the truth of the statements that I have made in this paper.

DISCUSSION.

Mr. BARKER, London: Mr. MacEwen and Dr Little appear from their papers to be quite in accord in the main as to the pathology of genu valgum. With their views the speaker agrees completely, and believes with them that there are many cases of genu valgum quite unsuited for operation, while a number remain where osteotomy is the proper measure to adopt. The particular stage of the affection it is which determines more than anything the procedure to be adopted for its treatment. Instrumental treatment will generally suffice for the stage of softening, but when this has passed, and the bone has become firm again, osteotomy is called for, and will always have a large field of usefulness. The whole question as to the admissibility of Professor MacEwen's operation appears to the speaker to turn in a great measure upon this point, *i.e.*, is it a dangerous proceeding or not? If it be not dangerous, why not substitute it for a lengthy, expensive, and troublesome treatment with instruments which render repeated visits to the instrument-maker and surgeon necessary, and which is not always even then successful? A parallel might be quoted in the instance of *pes equinus* which might be cured by prolonged mechanical treatment, but which no surgeon would now hesitate to straighten at once by tenotomy on the score of the risk of the operation. Professor MacEwen's 560 operations amply prove that his operation, properly performed, has no more danger about it than ordinary tenotomy; and from a careful study of the English, French, and German literature of the subject, the speaker believes that no charge of dangerousness can be brought against the operation from other quarters. In regard to the theory of causation of genu valgum put forward in the third paper read, *i.e.*, that it is due to stretching of the internal lateral ligament, it need only be said that, if it were correct, the treatment advocated by the author (Mr. Brodhurst), namely, to divide the external lateral ligament, would be most undesirable. By this measure the joint would be left without support either of the internal or external lateral ligament, and would become most unstable. The speaker had

seen a case illustrating this in a remarkable degree. The patient, an adult female, came to him at University College Hospital complaining of a loose knee-joint, with inability to walk with any comfort without instruments. She stated that several years ago her knee had been operated on in this way by no less fervent an advocate of the method of division of the external lateral ligament than Mr. Brodhurst. She now demanded an operation for genu vulgam, which was refused owing to the lateral instability of the joint. Lameness, pyæmia, anchylosis had been alluded to in the paper very freely, and as though of common occurrence after MacEwen's operation. Such allusions (the speaker submits) it would be most desirable to have carefully substantiated, seeing that they are at variance with all that can be gathered from the literature of the subject, home or foreign, which the speaker had carefully examined. The accidents alluded to had followed other modes of osteotomy, but it had yet to be shown to have followed upon MacEwen's operation. Such statements, unless definitely confirmed, were calculated to foster an unnecessary feeling of anxiety, and to be misleading.

M. JULES GUÉRIN, Paris: The papers which have just been read show that there are as many opinions on the nature and mechanism of this deformity as there are methods of treating it. This divergence of opinion among conscientious and experienced practitioners appears to me to depend on a confusion as to the causes which lead to the deformity. It will suffice, I hope, that I should categorise the causes, and place them in relation with the characteristics peculiar to each form, in order to clear away this confusion. The observation of more than 300 cases of genu valgum, and an experience of more than forty years, have led me to recognise four classes of cause for genu valgum, and consequently four forms of the deformity. The first cause is active muscular contraction analogous to that which, in the ocular muscles, leads to strabismus, and in the foot to talipes. The special muscles which lead to genu valgum are the tensor vaginæ fasciæ and the biceps—the one inclining the leg laterally and angularly on the thigh, the other causing rotation of the condyles of the tibia on those of the femur, whence results the external rotation of the leg which so often complicates knock-knees. To these primary and special causes we must add retraction and subsequent shortening of the external lateral ligaments of the knee. This category of cause is indisputable, and I show a series of drawings of monsters and of infants, where examples of knock-knees accompanying other forms of convulsive disorders can be plainly seen. The second form of genu valgum is the result of great weakness of the articulation of the ligaments and muscles intended to assure the verticality of the bones entering into the formation of the joints. It is quite obvious that if the vertical force of gravity is superior to the resistance of the ligaments and muscles of the knee, the joint will yield in the direction of its least resistance. This resistance is least on the inner aspect of the knee, there not being any muscle or ligament to oppose those on the outer side. This result is the more likely as the horizontality of the femoro-tibial surfaces is not always absolute. This form of cause is frequently a local manifestation of a condition of general weakness. In such cases all the joints —feet, hands, fingers, and vertebral column—by their extreme laxity attest the special origin of the genu valgum. The treatment of this form of the disease naturally excludes section of either muscles or ligaments: it necessitates the use of instruments of support, together with local *fortifiants*, such as hydro-therapy and other analogous means. The third form of genu valgum is due to rickets; its mechanism of production is quite special. The genu valgum of rickets is almost always the result of an abnormal direction of the joint-surfaces,

together with the vertical force of gravity. In my researches on rickets I have shown that the deformities of the skeleton are under a developmental law, in virtue of which the deformities are produced from below upwards—from the tibias and fibulas to the femurs, from the femurs to the pelvis, and from the pelvis to the vertebral column. Thus, when the bones of the thigh and leg commence to curve, their articular surfaces take a direction transversely oblique, from within outwards, and so bring about a lateral inclination of the tibias on the femurs. To this initial cause comes now the action of gravity, which, finding the bony system weak, increases the obliquity of the joint-surfaces. From this twofold cause there results generally a lateral depression, which, after *redressement*, leaves a wedge-shaped interval capable of holding the finger, but which diminishes from without inwards. The local treatment which such an origin of the deformity requires should not be less special than the general treatment of the disease from which it results. Locally, orthopædic instruments, the recumbent position, cold douche baths, and tonics; the whole associated with the general treatment such as rickets requires. Finally, as the result of disease of the knee, the internal condyles of the femur occasionally force the leg outwards. This fourth form of genu valgum, not very common, is caused by means which do not require any very long explanation. It is the combined result of relaxation of the ligamentous structures of the joint and of the action of gravity constantly acting on a default of horizontality of the articular surfaces. The treatment required by such complex conditions must respond to their diversity—orthopædic instruments, section of the shortened muscles and ligaments, internal and general medication appropriate to the condition which has preceded the articular deformity. The special orthopædic instrument which I have used for more than forty years, and which is adapted for all forms of genu valgum, consists of a hollowed splint (*goutière*) jointed at the level of the knee, and articulated by means of a hinge which permits the leg-portion of the splint to rotate inwards, and so bring the leg gradually into the axis of the thigh. This graduation is effected by means of an arched screw, the nut of which arrests it at any desired point. Such are the four possible forms of genu valgum, as well as the methods of treating them. These means have always succeeded in my hands, and all subjects of this disease up to the age of seventeen years have been completely and permanently cured, notwithstanding the wedged-shaped interval between the tibia and the femur. As an extreme limit of what may be accomplished by this method of treatment, I may mention the case of a young Nubian aged seventeen years, the detailed account of which may be found in the "Report of the Hospital Commission on Orthopædic Treatment, 1849." But before closing these remarks, I would ask permission to add a few words on the two surgical methods which have recently been introduced for the cure of this deformity. I now refer to osteotomy undertaken with a view to re-establish the horizontality of the articular surfaces by removing the exuberant portion of the condyle, and to crushing of the condyle by *redressement*. Neither of these methods is necessary in young subjects, and any good result which may be obtained can only be secured at the risk of more or less grave accidents, or by destroying the normal function of the joint. One can hardly imagine that the excision or crushing of the internal condyle of the femur can leave an articular surface capable of facilitating the regular movements of flexion and extension. Moreover, I would add that up to the age of fifteen or sixteen years it is always possible by the means I have indicated to secure the re-establishment of the horizontality of the articular surfaces. If in adult life, however, any very considerable deviation should seem to indicate osteotomy, it may be tried; but there must always be the prospective of a very imperfect movement,

or even of anchylosis of the joint, as a consequence. There is, indeed, no other alternative to hope for.

M. Fochier, Lyons: As the successor of M. Delore, I am anxious to advocate the method by *redressement brusque*. Genu valgum results from an altered equilibrium in the knee-joint. Up to the age of ten or twelve years it is caused by a slight rachitic curve of the bones, either of the tibia (in, I believe, the majority of the cases) or of the femur, but close to the joint in either case. This proximity of the curve to the joint is a necessary condition to bring the weight of the body on to the external condyle of the femur, and its corresponding tibial tuberosity. Meanwhile, the internal condyle, relieved of all pressure, hypertrophies, and the disease thus appears as if due to this hypertrophy, while really the hypertrophy is itself secondary to curving of the shaft of the bone. In the genu valgum of adults, this condylar hypertrophy appears to be the primary condition, and to depend on hyperactivity of the "cartilage of conjugation" at the epiphysial junction. Whatever be the cause, however, *redressement brusque* always suffices for the cure, except when the disease occurs beyond the limit of age when the epiphysis is still separate from the diaphysis; for the lesion produced by this method of treatment is a separation of the epiphysis from the shaft; in this manner the deformity is corrected by attacking its cause at its very seat. This simple operation appears to me very much superior to osteotomy: I will point out some of its difficulties. These difficulties are not always *en rapport* with the age of the patient, although age doubtless has much to do with them. Thus I have straightened the knee of a tall and strong young woman aged nineteen years. The chief difficulties are experienced in cases where the rickets has been well marked and has been well cured. On one occasion I was quite unable to separate the epiphysis; but a fortnight later, on making a second attempt, I was astonished at the facility with which detachment was brought about. The idea has suggested itself that some slight inflammation had been set up as the result of the first operation, which had materially facilitated the second. In some children there are symptoms of chronic inflammation of those joints which are the seat of genu valgum; this is no contra-indication for *redressement brusque;* it simply suggests a more prolonged use of the fixative bandage which is usually employed after the operation. In three cases in which I have operated the white swelling has only progressed once, and that only after the fixative bandage had been left off. An application of *ferrum candens*, and two subsequent applications of the bandages, sufficed to relieve this complication.

Mr. Keetley, London: I consider Mr. MacEwen's operation to be right in principle, successful in practice, and frequently necessary. But it is a fact that in a great number of cases osteotomy is not desirable. What is the best treatment for these? I can scarcely think with Dr. Ogston that Delore's operation is suitable for all these. An operation which involves violence on bones and epiphysial cartilages can only be absolutely safe when followed by perfect after-treatment. What surgeon could invariably guarantee this? Still Delore's operation is well suited for *bad* rachitic cases of genu valgum while the bones are still soft; for in such cases, while, on the one hand, treatment by splints and instruments is often extremely tedious, and even, if ultimately successful to the bone, injurious to the ligaments of the joint, yet, on the other hand, antiseptic osteotomy can never be, at all events in human hands, other than a more or less serious proceeding. Weil has recorded two deaths from it in neither of which the knee-joint had been opened, one from septicæmia,

[illegible] and six cases of Mr. MacEwen's had suppurated. [illegible] that cannot yet be operated on with the experience [illegible] Hunter. But I go back to the milder cases with soft bones, [illegible] I have carefully tried Hunter's method of bandaging in the lateral position in about six cases for three months each. Some showed very little improvement, others none. Mr. MacEwen's mode of dealing with this class of cases appears to be medical and hygienic, rather than mechanical, the mechanical part of the treatment being [illegible] I am not one of those who [illegible] the fashionable fondness for new instruments, but the old irons for genu valgum were wrong in principle. No one can study the beautiful work of Malgaigne and the preliminary researches of Busch, Weil, and others, and then compare them with what is to be seen every day in our own museums and hospitals, without being convinced that the pathology of genu valgum has only been half understood and on a false basis. Now, in agreement with this pathology, the motion of lateral movement of the two levers of a good valgum instrument should be, not at the knee, as in the old instrument, but considerably above it. I have ordered two such instruments, and I am not despairing of my being successful in getting them into working order, though there are practical difficulties, especially those caused by the comparative weakness of the thighs. The lateral hinge above the plane of the knee-joint is one of the [illegible] to be solved in constructing such an instrument as to protect the ligaments of the knee-joint from all strain. The old instrument used the weight of the limb to stretch the external lateral ligament, and sometimes, as is well known, and as I can positively affirm from observation made at the Surgical Aid Society, led to deplorable cases of loose and distorted joints. Whether somewhere or another, elastic forces must be used [illegible] otherwise all any effect be hoped to be produced on growing bones. The old instruments stole a little elasticity from the stretched ligament. But I am pleased to point out the thoroughly intelligent character of the present progress in this department of surgery.

Mr. CLEMENT LUCAS. I should like to bring at the pathology of genu valgum as stated in the MEDICAL TIMES AND GAZETTE of quite recent times. It is remarkable how all authorities agree that it is simply the result of relaxed ligaments. Nothing is said of rickets, yet I am most likely that the knock-knee met with in this country is almost always the result of rickets. Rickets is a constitutional affection, which, besides causing softening of the bones, causes weakening of the ligaments and want of tone in the muscles. The affection of the bones is but one symptom, which shows itself first in the bending of the ribs, because these are constantly on the move. I begin to recognise rickets even before the bone affections appear. If a child has a chronically distended abdomen, with a capricious appetite, and is backward in walking, it will most certainly develop rickety bone changes unless the diet be attended to. It is quite an accident in the original direction of force whether the limb be bent in or bent out. It is very rare for us in England to meet with cases of genu valgum which can be traced to spasm of muscles. We have to deal with rickets. When the bones have become altered in shape, and the internal condyle lengthened, the cases may still be treated by splints if the bones are still soft; but if sclerosis has taken place, operation is indicated. Rupture or division of the extended lateral ligament may be of service in slight cases, but it is not indicated in severe

Osteotomy may be performed with the most trivial risk if it be done both subcutaneously and antiseptically. Whenever a new departure in surgical treatment takes place, it is first regarded with suspicion; next carried on with enthusiasm; and then comes a very strong conservation reaction, which would limit the operation almost to its exclusion. Some such conservative reaction osteotomy has now to contend against. It happened that for eight years I had attending under my care an extreme case of genu valgum, which resisted all treatment by splints both within and without the Hospital. He eventually ceased attending, not having gained any benefit. When Dr. Ogston published his paper some three or four years ago, my thoughts recurred to this case, and I wrote to the boy, and found him in the same deformed condition as when last seen. I divided his internal condyle and completely cured the deformity. Encouraged by this result, I have performed that operation many times since, in all some seventeen or eighteen times, and I have never had any ill result. I have divided both internal condyles on the same patient and on the same day, and then there has been no rise of temperature or fever to give one the slightest anxiety. I have never caused a stiff joint. The last case was performed last Tuesday. There has been no rise of temperature since. I have not used Mr. MacEwen's operation, and I do not therefore deny its efficacy. It is used by my colleagues, but the cases that have come under my own care have appeared to me to be most suitable for division of the internal condyle. I always commence movements three weeks after the operation. The operation has been performed also by my colleagues, and some thirty or forty cases must have been performed in Guy's Hospital, and in no case has there been any ill result.

Mr. NOBLE SMITH, London: Our thanks are due to Mr. MacEwen for introducing a safer mode of performing osteotomy than those which had before been employed; but at the same time it would appear that osteotomy had been carried of late rather too far. There can be no doubt that in many cases, as in adults, no treatment short of division of the bones can remove the deformity; but in children under ten years of age—namely, at eight, at six, and even at three years of age—such an operation cannot be justifiable. Osteotomy is said to cure genu valgum in two months or ten weeks; if such is the result, the operation is certainly a brilliant one, but we have yet to learn the ultimate result of patients so operated upon. The few cases seen by the speaker of young children operated upon by osteotomy have been examples of a failure of the operation, or rather examples of relapse after that means of treatment. Osteotomy is not without some amount of danger; it is at all times, even by Mr. MacEwen's showing, a serious operation; and if we can cure cases, as we undoubtedly can, by means of instruments, ought we not to give every patient the chance of being treated, although tediously, yet with perfect safety, and in children with a certainty of success? The speaker had watched the progress of over one hundred cases treated at the City Orthopædic Hospital by instrumental means, and all had been cured, or were progressing towards recovery. These cases had not been left to the care of the instrument-maker, as had been suggested; and the speaker considered that if the surgeon was not capable of undertaking the entire manipulation of the instruments he employed, he had no right to undertake the treatment of such cases.

Mr. MACEWEN, Glasgow, replied: As far as Dr. Little's and my own views concern flat-foot, they are sufficiently near to be considered together, and show that flat-foot does accompany genu valgum in the infant, or in one who

man has disclaimed any intention of imputing such grave results to my operation. With regard to the results of the operation itself, I refer to my paper.

The Surgical Vice-President, Mr. HOLMES, then summed up the debate. He first remarked on the different views of the pathology of the disease entertained by such eminent surgeons as Dr. MacEwen, Dr. Little, Mr. Brodhurst, and M. Guérin, from which different plans of treatment necessarily result. Thus M. Guérin, with an ample experience extending over more than forty years, has never met with a case which in his opinion would justify osteotomy, while Dr. MacEwen, in a much shorter practice, had performed 522 osteotomies on 367 patients. Dr. Little trusts chiefly to orthopædic measures (though he does not reject osteotomy). Mr. Brodhurst and M. Guérin recommend for severe cases the section of the external lateral ligament, ilio-tibial band, and tendon of the biceps; and some other French surgeons agree with M. Fochier in recommending *redressement brusque*. And, further, those who practise osteotomy recommend several different operations. In this state of things, no definite opinion can be formulated, especially considering the very recent introduction of osteotomy in the treatment of the deformity. But one thing comes out very strongly from the discussion—viz., that proper care in the initial stages of the disease would in all cases prevent any such deformity as would justify operation; and this is one of the many proofs how much improvement is still to be desired in the public treatment of the chronic diseases of the poor. As to the method of osteotomy proposed by Dr. MacEwen, the information before the Section appears to prove that the operation is an adequate and a comparatively safe one, allowing that the deformity is severe enough to justify it. But further information is still needed as to what class of genu valgum indicates this or other forms of osteotomy, and also what the danger may be found to be. Any such danger, however, is not to be considered as an objection to the operation, provided that the deformity is so severe at the time the patient is first seen as to prevent any prospect of success from orthopædic measures, or that such measures have failed after a careful and skilful application. The mere fact that death has in some cases followed the operation, even allowing that it was directly caused by the operation, is no objection to it, if the surgical indications for its performance were adequate.

Tracheotomy in Croup and Diphtheria.

Professor GEORGE BUCHANAN, Glasgow.

1.—Tracheotomy is justifiable in diphtheria as well as in croup (if they are not identical diseases). This proposition might be considered unnecessary at the present time, but not many years ago it would not have been so. Even now, there are some who maintain that it is useless to operate in diphtheria, because it is a general disease, and the false membrane is but a local manifestation of it. I do not here wish to enter into the question of identity, but there is one phase of both affections which makes them clinically identical—that is, the tendency to death by suffocation. In order to make my position clear, I define diphtheria to be a disease, characterised

by an effusion of false membrane on the fauces, with a tendency to this membrane to extend through the glottis into the trachea, and even down to the bronchi and lesser bronchial tubes. In croup there is also the false membrane in the trachea, with a tendency to the membrane to extend downwards, but not upwards. The clinical difference then is the situation of the false membrane at the outset.

2.—*The type of the disease in which tracheotomy is justifiable.*—Both croup and diphtheria may be found in an asthenic as well as a sthenic type.

In the asthenic type the tendency is to death by rapid failure of the vital powers. Diphtheria occurs in this form much more frequently than croup. The false membrane is present, and often causes suffocation before the vital power is quite exhausted, but the asthenic symptoms show that the disease is exhausting the patient, independently of any want of air in the lungs. In this type tracheotomy should never be performed; it cannot save the patient, and scarcely, if at all, mitigates the suffering.

In the sthenic type the vital powers are not exhausted early, and the patient, if not relieved, dies of suffocation, caused by the diminution of size of the air passages by the false membrane, and by the frequent spasm of the glottis which accompanies it, and produces the stridor during inspiration so characteristic of it.

3.—*The stage of the disease in which tracheotomy should be performed.*—In a sthenic case, as soon as it becomes apparent that remedies are not producing any appreciable effect, and the disease is progressing unchecked, then tracheotomy should be performed before the struggles of the patient and want of proper aëration of the blood so tell upon the vital powers as to make recovery doubtful. I believe that in suitable cases more lives might be saved by early resort to tracheotomy; for the operation, if skilfully performed, is not a dangerous operation either *per se* or in its results.

4.—*The diagnosis of cases suitable for the operation.*—There are two signs which I rely on as indicative of necessity for tracheotomy:—

(*a.*) Stridor, indicating, on the one hand, a cavity ready to receive air if it could get access, and a passage constricted either by false membrane, or spasm, or both. The louder the stridor the more urgent the call for tracheotomy.

(*b.*) The movements of the chest as seen on inspection. I always cause the child to be stripped naked, in a warm atmosphere, and observe the movements. Approaching suffocation may be caused in these diseases either by the partial occlusion of the trachea, or by the bronchi and smaller bronchial tubes being stuffed with false membrane and viscid effusion. In this latter case the thirst for air—"*besoin de respirer*"—is as great as in the former, for the lungs, being already distended with air, imprisoned by the tough tenacious substance in the tubes, can neither contract nor expand efficiently, and as a consequence the chest remains puffed out and comparatively motionless.

In the former case the lungs are normal, ready to receive air, and the respiratory muscles are stimulated to the most violent efforts to expand and contract the thorax cavity. During the inspiratory movements the lungs fail to expand, in consequence of the air being denied entrance, and, as a result, all the flexible parts of the chest are sucked inwards, the intercostal spaces recede, and even the costal cartilages are bent in. In such a state of matters tracheotomy is urgently demanded.

5.—*The operation itself.*—I have nothing very special to bring forward. I adopt Trousseau's maxim, "Operate slowly, very slowly." The incision in the skin should be free, and should extend from nearly opposite, or little below

the cricoid for an inch and a half. Cut after cut must be made in the middle line until the trachea is visible. Everything which bleeds must be tied; and I have no hesitation in notching upwards, and even in dividing, the isthmus of the thyroid body. I have never seen hæmorrhage from it which could not easily be commanded by a ligature. Whatever bulges into the wound—veins, cellular tissue, apices of the thymus gland—must be held aside by retractors, and the golden rule I believe to be, "Never plunge the knife into the trachea till the white rings are clearly seen in the bottom of the wound."

Chloroform.—The greatest advantage is obtained by administering chloroform when the child is vigorous and the struggles are severe; but when there is a tendency to asphyxia, as indicated by the blue, cold, and clammy face, it is both unnecessary and dangerous. Often I have operated with success when the patient lay without wincing much, owing to the carbonisation of the blood. The opening of the trachea restores the colour and warmth in an incredibly short time.

6.—*After-treatment.*—I order all drugs to be discontinued, and trust solely to nourishment and attention to the tube. I have no reason to give for this but the success which has followed its adoption.

The tube must be kept cleansed from any mucous or other discharge, and from the viscid secretion which so often tends to block it up. The nature of the material which sticks in the tube is often indicative of the progress or recession of the original disease. If it is very viscid, and easily dries and crusts the tube, the respiration has a whistling sound, and indicates an inflammatory condition of the mucous membrane further down, always an unfavourable complication. In such a case it is necessary to take a feather moistened with tepid water and pass it down through the tube into the trachea and mop it out freely. Often I have seen a pellet of thick inspissated mucus shot out after such mopping, giving rise to free respiration.*

When the secretion is copious mucous, even purulent, there is little tendency to obstruction, and the chief trouble is to clear it away from the mouth of the tube, but often it is freely coughed out through the tube.

Viscid, easily-dried secretion with whistling respiration is an unfavourable sign, while copious moist secretion with much gurgling and coughing is a good sign.

The atmosphere around the patient should be temperate, fresh, and free from draughts; if possible, regulated to about sixty-five degrees of heat.

I have given up the keeping the patient in a steam-bath by imbuing the atmosphere with steam introduced under a cover or tent. I am satisfied it is both unnecessary and injurious, the patient being kept in a state of perpetual moisture from his damp clothes.

Many years ago I found that in spite of this precaution the secretion in the tube became viscid and dry in a case with tendency to this kind of effusion. In one case, after I had mopped out the trachea and got away the offending pellet, the attendant kept a piece of sponge moistened with hot water over the mouth of the tube, which kept the entering air moist within. Since that time I have trusted to the use of a moist hot sponge when necessary, but I believe even this is not required. A bit of gauze laid loosely over the tube acts as a respirator in moistening the air, but in my last case I found even this not to be necessary, and in future I shall dispense with all precautions except keeping the temperature equable, and seeing that the patient is not exposed to draughts, with a bit of gauze over the mouth of the tube for a day or two.

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* See *Brit. Med. Journal* for April 10, 1880.

7.—Statistics of my own practice:—Total tracheotomies for any cause, 54; cured, 22; died, 32. Total tracheotomies for croup and diphtheria, 50; cured, 19; died, 31. Tracheotomy for croup, 17; cured, 7; died, 10. Tracheotomy for diphtheria, 33; cured, 12; died, 21.

Taking the two diseases croup and diphtheria together, tracheotomy saved the lives of one out of every $2\frac{2}{3}$ children operated on, who would undoubtedly have died if the operation had not been performed.

DISCUSSION.

Dr. LEALE, New York: The very valuable paper, viz., the question raised by Professor Buchanan, "Is Tracheotomy Justifiable in Diphtheria?" may be answered in the affirmative, but I believe that it should be performed only *after all other means have failed, and when death is impending by suffocation.* I have operated where the disease extended below the bifurcation of the trachea, and when with the small elbow forceps and sponge the passage has been cleared, while the patient was held with head lower than the thorax to expedite the drainage of the mucus by gravitation. I once had recovery follow where the alarming symptoms were only so far relieved after two hours' efforts at artificial respiration that the patient could breathe independently of assistance from the physician. I have been called to perform tracheotomy when the patient presented the most unfavourable symptoms, and when cyanosis had existed, and when physicians present have said recovery could not follow, but when after an emetic has produced its effects, and the expectoration facilitated the removal of the exudation, the patient recovered, without an operation. I firmly believe that tracheotomy is sometimes performed where the danger has been increased by the new incision becoming a starting-point for the rapid advance of the disease, and also that it is our absolute duty to give the patient this his only chance for recovery, after all other means have failed, an operation by which death by apnœa can be averted.

Dr. RANKE, Munich: It has given me real pleasure to listen to Dr. Buchanan's paper, because Dr. Buchanan's views coincide thoroughly with my own experience of tracheotomy in cases of diphtheria. I have performed tracheotomy since April 1878 in twenty-six cases, and sixteen out of the twenty-six have recovered. I lay the greatest stress on early operation. If a child with pharyngeal diphtheria has become hoarse, and shows laryngeal stridor and a difficulty of breathing, which has already led to ever so short an attack of real dyspnœa, that child ought to be operated on at once. At least 90 per cent., if not more, of patients in that condition are sure to die if the operation is not performed. This being the case, tracheotomy should not be delayed, as experience has shown that it makes all the difference in prognosis, whether the operation is performed when the child is still comparatively strong or when its powers have been brought down by the disease to their minimum. In Munich, for instance, tracheotomy was formerly invariably unsuccessful in all cases of diphtheria, because it was always performed too late. I have amongst my recoveries a girl, five years of age, on whom I operated although she had pneumonia of the left lower lobe. But the child being still strong, tracheotomy was performed, and resolution of the pneumonia took place on the sixth day after the operation. I also preserve between two glass plates the bronchial cast of a boy of the age of six years and eleven months, which cast represents a regular tree of ramifications down to the smallest bronchi. That boy recovered, having been operated upon while he had still sufficient strength to cough up the membranes.

Age of course has a great deal to do in regard to prognosis. Of the 26 cases, 4 were between two and three years old, and of these 3 died; 9 were between three and four, and of these also 3 died; 6 were between four and five years old, and of these 2 died; of 4 between five and six 1 died, and also 1 of 2 between six and seven years. The only patient who had attained his seventh year recovered. Older children being, *cæteris paribus*, stronger, have correspondingly a greater chance of recovery. As to albuminuria, in 4 of the 16 cases that recovered, the urine had not been examined. Of the remaining 12 cases, albumen was found to be present in 9, and in 3 only it was never discovered during the whole time of treatment. Besides an early operation when once laryngeal symptoms have fairly established themselves, I lay the greatest importance on systematic ventilation in the treatment of diphtheria generally. I insist upon having two rooms at my disposal for the treatment of every case of diphtheria, whether tracheotomy be performed or not; the one room being kept with its doors and windows wide open, whilst the sick child occupies the other. At least twice a day, and each time for several hours, the rooms are exchanged, so that the patient has always pure air to breathe. In winter time the temperature of the room which has been ventilated must of course be raised again to its normal standard before the child is brought in. This *two-room treatment* (zwei-zimmer Behandlung), as it is called, I have carried through, I might say, pedantically for the last five or six years, and my pupils have adopted it from me; and I have every reason to attribute to it the greater part of my and their good results in treating diphtheria. Bed-curtains and other hangings, as well as carpets, I banish as much as possible from the sick-room. A third point in procuring good results after tracheotomy, and also a point of vital importance, lies, in my opinion, in the nursing. One ought to have particularly skilled and experienced nurses for tracheotomy cases. The management of the tube, principally during the first day or two, requires a great deal of skill and often a quick resolution, which can only be gained by manifold experience. I therefore always take my nurses for tracheotomy cases from *one* institution, in order gradually to get there a staff of really experienced nurses. As to medicinal treatment, I generally use none after tracheotomy, vapour inhalations being almost my only remedy, and I even only use these when expectoration becomes viscid. As to food, I exclusively give good broth and milk; wine I administer only in cases of decided weakness.

Mr. Golding Bird, London: Assuming the operation of tracheotomy to be done early, as I hold it should be, two questions of after-treatment result. The opening being made in the trachea, if the wound is held open with dressing forceps and a light is reflected into the windpipe, the posterior wall may be seen, pink, clear, with longitudinal rugæ. Such is the healthy appearance, and shows the membrane lying in the thyroid region above the wound. In such a case a tracheotomy tube may be employed. If, however, membrane is seen in the trachea, or if, after wearing the tube, membrane begins to appear, the tube should be withdrawn and the wound of the trachea kept open with a dilator, in order that the least forcible resistance may be offered to the passage outwards of mucus or membrane. Cleaning with feathers, &c., also is rendered easy. No harm follows the use of the dilator; for even if slight abrasion does take place at the level of the original wound in the lateral tracheal walls, there is not the same fear from it as from ulceration, the result of the silver tube. This latter causes, when it does so, ulceration in the anterior wall low down, and is in danger of entering the left innominate vein. But by whatever means the tracheal wound is chosen to be held open, I maintain that this method is

mechanically superior to that with the tube, where membrane, thick mucus, or indeed any foreign body, has to be expectorated.

Dr. JACOBI, New York: Tracheotomy is indicated when there is *strangulation*, and the *seat of it is in the larynx*. No sort of complication gives a contra-indication, *not even pneumonia*. The latter will increase the number of inspirations (*which are slow in uncomplicated croup*), and has been taken to form a contra-indication to the operation. To operate late is always a disadvantage. When I could operate early (seventy cases published 1868), I had 20 per cent. of recoveries. Since that time, being called to operate late, I lose many more, and have lost fifty cases in succession. Where the symptoms of laryngeal stenosis are present, being frequently relieved then getting worse again, the case may be one of catarrh, and may sometimes be left alone. Mr. Golding Bird thinks slight ulcerations are not harmful. I have an exception to that rule, losing a child eight days after tracheotomy from hæmorrhage originating in just such a tracheal ulceration.

On the Surgical Treatment of Croup and Diphtheria by the Introduction of Tubes into the Trachea through the Mouth.

Mr. WILLIAM MACEWEN, Glasgow.

Mr. MacEwen, referring to the introductory remarks made by Dr. West on "tubage," stated that "tracheal catheterism" and tubage were widely different. Tubage of the larynx was introduced into France by M. Bouchut. He used an instrument of a cylindrical shape, measuring one inch in length, and furnished with two elbows at its upper part designed to rest on the vocal cords. A thread of silk was attached to this tube whereby it could be removed from the larynx. Whatever merits this instrument may have had, M. Bouchut could not furnish any case which had recovered after this tube had been used. This fatality might have been the result of the original disease quite independent of the instrument. On the other hand, "tracheal catheterism," as introduced by the author some years ago, consisted in using a tube 10 to 14 inches in length, the one extremity of which was passed into the trachea, the other projecting from the mouth. The method of introduction and the details of several cases were given in full in the *British Medical Journal*, July 24th and 31st, 1880, and to these the reader is referred.

The first two cases in which tracheal catheterism was practised by the writer were adults suffering from œdema glottidis. In one the œdema resulted from a severe scald; in the other it supervened on chronic ulceration of the larynx. They were both sent into the Royal Infirmary, Glasgow, with the intention of having tracheotomy performed, and if tracheal catheterism had not been practised, an opening into the trachea would have been imperative. In both, the tubes were finally withdrawn after thirty-six hours, having answered the purpose for which they were inserted. In two cases of children this treatment has been practised by other surgeons. One by Dr. Fraser of Birkenhead, in a child affected with croup, the other by Dr. Lantnany, also in a child similarly affected. In both these cases these surgeons were satisfied with the manner in which the tubes acted, and in both recovery was complete. In one case of

croup with false membranes (diphtheria?), occurring in a boy of eight years, the catheter was inserted, and false membranes were expelled through it, and the patient breathed freely and easily. This patient recovered from the laryngeal affection, the tube was withdrawn, and he was pronounced convalescent. Four days later he became affected with pneumonia, from which he afterwards died.

These, then, are five cases in which these tubes have been used with success, four of these having recovered and are living at the present time; the fifth recovered from the laryngeal affection but succumbed to pneumonia contracted during convalescence. So that it may be said that the five cases hitherto tried have been successful.

Besides these cases, tracheal catheters were introduced in two patients who had to undergo operations for the removal of epithelioma of the back part of the tongue and pharynx. Instead of performing tracheotomy and introducing Trendelenburg's tamponade canula, the catheter was introduced into the trachea, the upper portion of the glottis being occluded by an india-rubber ring placed on the outside of the catheter so as to increase its external circumference. Chloroform was administered through the tube, which projected several inches from the mouth. It was thus easy to exhibit the anæsthetic during the continuance of the operation, while it in no way interfered with the operator, and it also prevented blood from entering the trachea.

DISCUSSION.

Dr. ALEX. ROBERTSON, Glasgow: I saw three of the patients referred to by Mr. MacEwen, who were shown by him at meetings of medical societies in Glasgow. They had all fully recovered. In one of the cases the tube had been in the larynx for thirty-six hours continuously, except for a few minutes on two or three occasions. I carefully observed in that case the state of the voice. It was perfectly normal, and the patient said it was as strong and clear as ever it was. I also saw the child aged eight years spoken of by Mr. MacEwen, after the tube had been in for three days. The little patient was quietly breathing through the tube, which projected about three inches beyond the lips. By examining with a lighted candle, I satisfied myself that the breathing was *through* the tube, not by the side of it. The respiratory murmur was heard by me at the base of the lungs, showing that the admission of the air was quite free. Fearing that the tube might act as a conductor to fluids in any attempt to swallow, I satisfied myself on that point by causing the nurse to give the child a spoonful of milk, which it swallowed with little difficulty, obviously none going into the larynx. Mr. President, I regard this mode of treatment, which I think is mainly Mr. MacEwen's, though M. Bouchut previously made some not very successful attempts by a somewhat similar method, as a valuable addition to the means of treating a most important class of cases. How far, however, it may supersede the operation of tracheotomy it would be premature to say.

Traitement de l'Angine couenneuse et de la Diphtérite par la Dissolution des Fausses Membranes au Moyen de la Papaïne.

Dr. Bouchut, Paris.

La diphtérite est une maladie primitivement locale devenant rapidement infectieuse, et elle peut être guérie sur place par les agents antiseptiques et par les dissolvants de la fibrine parmi les quels il faut citer la *papaïne* extraite du *Carica papaya.*

Les faits que j'ai déjà publiés en 1879 sur la dissolution et la digestion rapide des ténias des lombrics et des fausses membranes du croup devaient me conduire au traitement de l'angine couenneuse par la *papaïne.*

J'avais dû hésiter dans ces essais par la raison que si des fausses membranes baignant dans une solution de papaïne se dissolvaient et se digéraient en quelques minutes, il n'en est pas de même pour des fausses membranes diphtéritiques collées sur les amygdales et qu'en ne fait que laver avec un pinceau chargé de papaïne. Les deux expériences n'ont rien de comparable.

Mais, M. A. Wurtz de l'Académie des Sciences ayant montré que la fibrine, imprégnée par une solution concentrée de papaïne et lavée ensuite sous un filet d'eau pendant un jour, n'en était pas moins bien digérée et convertie en peptone si on la mettait dans une étuve à 40°.

Dès lors, je fus en droit d'appliquer la papaïne au traitement de l'angine couenneuse, à son début, puis qu'il suffisait de toucher la fibrine avec un pinceau imbibé de solution de papaïne pour en espérer la dissolution.

Je mis alors, indistinctement, toutes les angines couenneuses qui entrèrent dans mon service de l'hôpital des enfants, les cas graves ou légers, avec ou sans adénite cervicale, avec ou sans albuminurie, de manière à ne pas laisser croire à un choix des cas les plus favorables, et d'une guérison facile par toutes les méthodes de traitement.

Je pris une *solution de papaïne* à deux grammes pour huit grammes d'eau distillée avec cinq centigrammes de salicylate de soude pour éviter toute fermentation.

Je suis arrivé au chiffre de 46 cas d'angine couenneuse traité de cette façon, et sur ces 46 cas j'ai eu 40 guérisons.

Un de ces enfants avait en même temps une otite membraneuse du conduit auditif externe dont la pellicule diphtéritique très épaisse a été dissoute et digérée en 24 heures.

Dans plusieurs autres cas, il y avait de la conjonctivite diphtéritique et la fausse membrane touchée plusieurs fois par jour avec la papaïne a disparu. C'est un très bon traitement de la conjonctivite diphtéritique.

Les fausses membranes ne se dissolvent pas instantanément comme dans un verre, mais elles l'amincissent graduellement ou se détachent saus reparaître, et en trois jours les enfants sont guéris.

Il suffit de toucher les fausses membranes toutes les deux heures avec un pinceau imbibé de la solution, et dans l'intervalle je fais faire des gargarismes ou des douches dans la gorge avec une solution de benzoite de soude, de bicarbonate de soude ou d'eau phéniquée a deux grammes pour 1000 grammes d'eau.

Des Résections articulaires chez les Enfants, et de leur Influence sur l'Accroissement des Membres.

Prof. L. Ollier, Lyons.

Les résections articulaires donnent en général de bons résultats immédiats chez les enfants, c'est sans doute pour cette raison que quelques chirurgiens les ont multipliées outre mesure et n'ont pas craint de réséquer chez de très jeunes enfants des articulations qui eussent guéri toutes seules avec du temps, une bonne hygiène et un traitement local approprié. Eu égard à la fréquence des maladies articulaires dans l'enfance, les résections sont rarement indiquées, mais il est des circonstances cependant où elles constituent le seul moyen de sauver le membre et la vie du sujet. Dans les trois premières années de la vie, les suppurations articulaires, lorsqu'elles ne sont pas le résultat d'une maladie générale que rendrait inutile toute opération, guérissent le plus souvent par l'ouverture des foyers purulents ; il est des cas cependant où il faut enlever des épiphyses ou des portions osseuses nécrosées. L'affection tuberculeuse des os et des articulations sévit durant toute l'enfance et la jeunesse, mais ici encore bien que l'ablation de toutes les parties malades paraisse théoriquement le meilleur moyen de guérison, on obtient de très beaux succès par l'expectation, tant sont énergiques les tendances réparatrices de la nature, surtout lorsqu'on peut les aider par toutes les ressources de l'hygiène combinées avec une thérapeutique rationelle. Parmi ces ressources qu'on ne peut malheureusement procurer qu'à un petit nombre d'individus, je citerai le séjour sur les bords de la Méditerranée et les bains de mer. La rénovation organique que s'opère sous l'influence d'une athmosphère maritime change bientôt l'aspect misérable des enfants scrofuleux lorsque leurs organes internes ne sont pas altérés. Cet effet se produit rapidement sur ces sujets étiolés qui pullulent dans les quartiers pauvres des grandes villes. On les voit prendre du teint et de l'embonpoint, et des articulations fongueuses et criblées de fistules ne tardent pas à se cicatriser. C'est là un des meilleurs arguments en faveur de la curabilité de la tuberculose, de la tuberculose du jeune âge en particulier.

Mais ce n'est pas sur ce point de vue des résections que je veux insister. Je tiens surtout à démontrer les inconvénients des résections pratiquées chez les enfants au point de vue de la croissance des membres. Nous trouverons dans ces considérations, non pas un argument contre la résection entreprise pour sauver la vie des malades, mais un motif de plus pour persévérer dans le traitement expectant lorsque la vie n'est pas immédiatement menacée.

Toute résection articulaire pratiquée dans l'enfance nuit à l'accroissement ultérieur du membre. C'est là une proposition que j'ai appuyée autrefois sur de nombreuses expériences faites chez les jeunes animaux, et que je crois utile de démontrer encore, en précisiant le danger spécial de diverses résections. Comme les résections chez les enfants ont été beaucoup plus souvent pratiquées en Angleterre qu'en France, ou pourrait trouver dans ce pays de riches matériaux pour résoudre la question ; malheureusement je ne connais pas depuis les beaux travaux de M. Humphry de Cambridge, de travail d'ensemble sur ce sujet, et je n'ai pu me procurer tous les documents que j'aurais désiré ; mais il y a là un sujet intéressant qui tentera, il faut l'espérer, le zèle de quelques jeunes chirurgiens. C'est par la comparaison de cas nombreux, observés longtemps après l'opération, qu'on pourra combler les lacunes que je laisserai nécessairement dans ce travail.

Rien ne peut empêcher un certain degré d'arrêt du développement en longueur, quelque réguliers que soient les processus réparateurs et quelque abondante que puisse être la régénération des extrémités osseuses. Aussi faut-il se méfier des observations dans les quelles un chirurgien, un peu trop épris de son œuvre, annonce qu'à la suite d'une résection d'une grande articulation il a trouvé après la guérison complète les deux membres parfaitement égaux. Il y a là une erreur d'appréciation. Jamais chez les animaux auxquels j'avais pratiqué des résections articulaires, ou diaphysaires étendues, je n'ai trouvé pareille égalité.

Les membres peuvent paraître égaux quelques semaines après l'opération, au moment de la guérison de la plaie : mais l'inégalité entre les membres opérés et le membre sain se prononce de plus en plus.

Sur une jeune fille de 15 ans à qui j'avais enlevé la moitié supérieure de l'humérus, 125 millimètres, j'ai constaté quelques mois après, une différence de moins d'un centimètre entre les deux bras, mais trois ans plus tard cette différence était de six centimètres.

La méthode sous-périostée, qui a tant perfectionné les résultats des résections au point de vue orthopédique et fonctionnel, est impuissante à nous redonner la longueur de l'os réséqué. Jamais même dans les cas les plus favorables, l'os ne prend sa longueur normale. Si la portion reproduite peut être aussi longue que la portion enlevée, elle est toujours plus courte que la portion analogue du côté opposé ; celle-ci continuant à grandir, la première n'ayant plus les moyens de s'accroître en longueur ou ne les ayant que d'une manière transitoire et imparfaite.

J'ai bien constaté dans mes expériences la reconstitution temporaire d'un cartilage de conjugaison et la persistance d'une couche cartilagineuse entre les noyaux de ossification épiphysaire et la masse diaphysaire de nouvelle formation ; mais ce cartilage ne tarde pas à disparaître en s'ossifiant et l'os ne peut plus grandir en hauteur. Dans le cas de résection d'une extrémité osseuse, cette inégalité entre l'os réséqué et l'os sain du côté opposé, s'accuse d'autant plus que l'extrémité retranchée prend une plus grande part à l'accroissement normal de l'os.

Les diverses résections des grandes articulations diffèrent considérablement eu égard à l'arrêt d'accroissement auquel elles exposent. La loi d'accroissement des os des membres, telle que je l'ai depuis longtemps formulée, nous fait prévoir ces différences.

Au membre supérieur, pour les os du bras et de l'avant-bras, ce sont les extrémités éloignées du coude qui prennent la plus grande part à l'accroissement du membre ; d'où l'arrêt de l'accroissement de l'humérus après la résection de l'épaule, et l'arrêt d'accroissement de l'avant-bras après la résection du poignet chez les jeunes enfants. D'où par cela même le peu d'inconvénient relatif de la résection du coude, bien qu'elle porte sur les trois os qui constituent cette articulation.

Au membre inférieur, au contraire, les os de la cuisse et de la jambe, ce sont les extrémités constituant le genou qui prennent la plus grande part à l'accroissement du membre. De là la gravité des résections totales du genou pratiquées dans l'enfance que j'avais annoncée dès mes premières expériences et que les faits cliniques sont venus depuis lors confirmer surabondamment.

C'est l'ablation du cartilage de conjugaison qui est la cause immédiate de l'arrêt d'accroissement. De là la nécessité de diviser les résections articulaires en résections intra-épiphysaires, c'est-à-dire, coupant l'os dans la substance de l'épiphyse et laissant intact le cartilage de conjugaison, et en résections ultra-épiphysaires, c'est-à-dire coupant l'os au delà de l'épiphyse et enlevant en même temps le cartilage de conjugaison.

Mais il ne faudrait pas croire que les résections superficielles intra-épiphysaires, laissant intact le cartilage de conjugaison soient sans influence sur l'accroissement ultérieur de l'os. Si l'on mesure, après l'enlèvement de la croissance, les os qui ont subi ces résections superficielles, on les trouve toujours sensiblement plus courts que les os sains analogues. Il n'y a pas seulement le déficit osseux dû à la portion enlevée et qui ne s'est pas reproduite il y a aussi un arrêt d'accroissement de l'os dû à la perturbation qu'a éprouvée la nutrition du cartilage de conjugaison. Pour le genou cet arrêt d'accroissement peut aller jusqu'a 5 et 6 centimètres. C'est que sans doute quand on réfléchit aux arrêts d'accroissement de 20 et 30 centimètres qui peuvent être le résultat d'une résection totale ultra-épiphysaire pratiquée dans l'enfance, mais c'est un motif de plus pour insister sur les moyens de traitement plus conservateurs encore que la résection, tels que l'immobilisation prolongée, l'arthrotomie antiseptique et l'évidement ou le raclage des fongosités articulaires. On doit d'autant plus insister sur ces moyens que l'ankylose du genou, et même de la hanche en bonne position, laisse en général un membre plus utile qu'un membre réséqué.

Si les fongosités des arthrites ou ostéo-arthrites chroniques donnaient lieu fatalement chez les enfants, à la généralisation de la tuberculose on devrait agir autrement. Il faudrait se hâter de réséquer et même souvent d'amputer le membre ; mais il n'en est pas heureusement ainsi et la plupart des arthrites fongueuses suppurées, arrivent à se cicatriser, chez les enfants, lorsqu'elles ne sont pas précédées ou accompagnées de lésions tuberculeuses internes. Une arthrite fongueuse est sans doute une source d'infection générale, mais elle est aussi la manifestation d'un état diathèsique, antérieur à l'évolution du produit tuberculeux.

Sans cette double notion étiologique de la tuberculose, il est impossible de se rendre compte de la marche de cette maladie. En ne tenant compte que de l'infection qui a sa source dans l'arthrite fongueuse, on est porté à accorder trop de confiance au traitement local qui a pour but de supprimer le foyer infectieux ; et on attribue à la résection une influence curative que l'avenir dément malheureusement dans beaucoup de cas. Il ne faut pas oublier cet effet que toute ostéite ou arthrite développée spontanément chez un enfant ou un adolescent doit inspirer les plus grandes craintes pour la santé future. La phthisie pulmonaire fera dans l'avenir de nombreuse victimes dans cette catégorie de sujets.

D'autre part, en ne voyant que l'état diathèsique, on s'endort sur les dangers de la lésion locale et on laisse le poison tuberculeux infecter, par sa résorption, les divers systèmes organiques ; surtout quand on n'a pas soin de l'annihilir par la destruction des fongosités au moyen du fer rouge ou des divers caustiques chimiques.

C'est pour cela que pour certaines articulations en particulier, genou, cou-de-pied, carpe, tarse, l'ouverture antiseptique des foyers articulaires ou osseux (épiphysaires ou juxta-épiphysaires) le raclage des fongosités, l'évidement des foyers tuberculeux intra-osseux, la cautérisation ultérieure des tissus abrasés, constituent chez l'enfant des moyens de guérison qui nous dispenseraient souvent des résections totales.

Chez l'adulte, on doit se comporter autrement, et l'on ne peut guérir que par l'amputation des lésions qui cèdent chez l'enfant à l'ablation des quelques parties nécrosées ou à l'abrasion ou à la cautérisation des fongosités. On ne saurait trop s'appesantir sur la différence du traitement des maladies osseuses selon les âges. Un os jeune se rapproche beaucoup plus des parties molles qu'un os d'adulte. S'il devient plus souvent malade, il guérit encore plus

facilement; les processus réparateurs étant d'autant plus rapides que le sujet est plus jeune.

Et cependant il ne faut pas s'illusionner sur la valeur des opérations élémentaires qui peuvent dans certains cas nous dispenser des résections. La résection totale d'une articulation est un moyen de guérison plus sûr plus durable; plus on enlève de tissus, plus on dépasse les limites des foyers tuberculeux, plus on a de chances de faire une opération radicale. A ce point de vue l'amputation serait préférable à la résection; mais si nous différons la résection à cause des inconvénients qu'elle a au point de vue de la croissance du membre, à plus forte raison doit-on rejeter une mutilation irrémédiable.

Ces réserves faites sur le pronostic des lésions fongueuses suppurées des articulations, il faut se rendre compte des avantages et des inconvénients de la résection pour chaque grande articulation.

Là où la mobilité articulaire est indispensable au bon usage du membre, au coude, par exemple, il faut réséquer les extrémités osseuses dans le but de faire reconstituer une nouvelle articulation, mobile et solide à la fois, sur le type physiologique de l'articulation primitive. On réséquera d'autant plus cette articulation que l'opération ne nuit pas sensibilement à l'accroissement du membre; l'accroissement du membre par l'épaule (extrémité supérieure de l'humérus) et le poignet (extrémités inférieures du radius et du cubitus), étant environ six ou sept fois plus grand que l'accroissement qui s'opère par toutes les extrémités qui constituent le coude. C'est-à-dire, que si le membre a à s'allonger dans l'avenir de 30 centimètres; quatre au cinq tout ou plus manqueront par le fait de la suppression des cartilages de conjugaison qui seront sacrifiés par l'opération.

Dans la seconde enfance l'abrasion et la cautérisation articulaire sont toujours suivies d'une perte, plus ou moins complète des mouvements; mais il n'en est pas ainsi dans la première enfance, durant les trois premières années, et j'ai vu plusieurs fois des mouvements incomplets, mais assez étendus se rétablir après la suppuration spontanée ou la destruction artificielle des fongosités. C'est une raison de plus pour ne pas se presser de réséquer à cet âge les articulations en suppuration, à moins qu'il n'y ait des désordres osseux étendus ou des nécroses épyphysaires avec décollement. Dans ce derniers cas la résection ou l'ablation des épiphyses nécrosées est très nettement indiquée; et il faut la faire le plus tôt possible, car en attendant on laisse non seulement les forces s'épuiser, mais les membres s'atrophier de plus en plus par suite des troubles de nutrition qui accompagnent les suppurations articulaires.

C'est même dans les cas de ce genre que la résection, en tarissant la suppuration et mettant le membre à même de fonctionner, favorise indirectement l'accroissement du membre. Des résections de la hanche pratiquées vers l'âge de trois ou quatre ans, nuiront à l'accroissement dans certains cas moins que des coxalgies suppurées entretenues par la présence de la tête fémorale nécrosée ou du moins décollée de la diaphyse. Si le sujet vit et résiste à la suppuration, il aura un membre atrophié dans son ensemble, et finalement plus court qu'un membre réséqué au dessous des trochanters.

Il ne faut donc pas être absolu au sujet des résections dans l'enfance. Si les resources de la nature sont grandes à cet âge, il est des formes d'ostéo-arthrite qui ne peuvent pas guérir spontanément et qu'il faut se hâter d'opérer dès qu'un diagnostic exact est posé. La question de l'accroissement ultérieur du membre, si importante lorsqu'il s'agit d'arthrites suppurées qui ne menacent pas immediatément la vie du sujet, devient tout-à-fait secondaire lorsque la résection est le meilleur moyen de conserver la vie.

Si, donc, nous nous élevons contre l'abus des résections dans l'enfance, nous

sommes loin de les rejeter. Si elles sont, relativement au nombre des arthrites fongueuses, chez les enfants, beaucoup moins souvent indiquées que chez les adultes, elles constituent dans certains cas le seul moyen de guérison. On en a abusé dans certains pays, mais dans d'autres contrées on a commis l'erreur inverse. En France, en particulier, la plupart des chiriurgiens ont exagéré leurs tendances conservatrices dans le traitement des lésions articulaires chez les enfants, et quoique cet excès soit peut-être moins préjudiciable que l'excès opposé, il n'en doit pas moins être condamné.

C'est par une appréciation judicieuse des affections curables par l'expectation méthodique et des formes morbides incurables par les ressources de la nature qu'on se tiendra éloigné des opinions extrêmes et qu'on arrivera à formuler les préceptes rationnels de la thérapeutique articulaire.

On the Treatment of Scrofulous Inflammation of Joints.

Professor HUETER, Greifswald. (Abstract.)

The author first referred to the experiment of Villemain showing that tuberculosis is an infectious disease, and then stated that scrofulous disease of joints was part of a general tubercular disease, as might be proved by transplanting some of the fungous matter into the anterior chamber of the eye of a rabbit. After about four or five weeks it developed, and he had been able to demonstrate the tubercular animal side by side with the patient, who under suitable treatment had partially recovered. The treatment consisted in the injection of solutions of carbolic acid into the midst of the diseased tissue: about 1 gramme of a 2 to 5 per cent. solution suffices for each injection. By means of a hypodermic syringe it is quite possible to penetrate even bone when diseased. The syringe must penetrate the cortical layer so as to get into the medulla.

If after three weeks of this treatment the case does not get well, excision of the diseased joint may be practised. But early excision is necessary. Excision is not only a curative agent, but it is also useful, because it allows the surgeon to get a full view of the extent of the disease, so that he can remove every part of it.

He referred also to the use of the gouge, of the cautery, and of iodoform.

If an examination of the lips of scrofulous children be made, almost as large a number of white corpuscles will be found as in a case of leucocythemia. As recovery takes place they become fewer and fewer. In other cases again they increase in number, and then relapses are to be feared.

DISCUSSION.

Mr. JOHN H. MORGAN, London: English surgeons are hardly prepared to accept *in toto* the views of Professor Hueter as to the pathology of these affections. It is desirable that the early pathology of these cases should be more studied, and with it the clinical symptoms which accompany the commencement of disease in the synovial membrane or in the end of a bone. I am not prepared, in some cases, to reject the treatment by free incision or injection, but from my own observation the treatment of synovial membranes by sulphuric acid or of the bones by potassa fusa has been followed by failure.

With regard to excision, I find that the advocates of that form of operation do not give sufficient merit to the results obtainable by rest, such rest as is given in cases of children suffering from disease of the hip, under the care of Mr. Marsh and myself. The subject of excision is a large one, and spoken of generally, it had not yielded all the results which were expected of it; and excision should not be talked of collectively, but according to the results obtained in each joint. Under these circumstances the remarks of M. Ollier are valuable, especially those which point to the share of the epiphyses in determining the length of the limb. But for one joint only—the elbow—can excision be claimed a successful operation. Finally, it seems to me that we should try to make diagnosis of lesion at an earlier date; and if that were done it might be possible to treat the local disease by some means, but regarding always the desirability of not interfering with epiphysial growth, and therefore to avoid excision in young children.

Professor Sayre, New York: If we diagnosticated these cases of diseased joints in their very earliest stages and treated them properly, we should in the majority of cases have a favourable result, and not be compelled to resort to exsection. The first and most important point is *rest*—absolute and complete rest of the joint involved; and as the disease is one of malnutrition, the general health must be sustained. If the rest given to the joint is obtained by confining the patient to the bed, the general health is impaired by the confinement, and therefore the treatment should be, to apply some means that will secure rest to the particular joint involved, combined with sufficient extension and counter-extension to prevent pressure on the inflamed parts. At the same time the appliance should be so adapted as to allow the patient to take full and free exercise in the open air, thus placing the patient in the best position for increased nutrition. If suppuration follows, let out the pus; and if caries increase, notwithstanding the best treatment that can be given has been adopted, then excise, and in many instances the most favourable results may be anticipated. I have exsected the hip-joint in seventy-three cases,* and in some of them the result has been almost perfect, both as to motion and length of the limb, as can be seen by these various photographs. But, as I first stated, if we diagnosticate the disease early and treat it properly, we will have favourable results in the majority of cases. Time is too short to say what I wish; but as my views are well known, it is unnecessary.

Dr. Gustavus Krauss, Darmstadt: I think that before we can accept the teaching that "in scrofulous disease of joints, resection is indicated in those cases in which, after the injection of the antisepticum into the centre of the diseased spot, an improvement had not been effected," we ought to carefully weigh all collateral circumstances, to follow the course of the disease in our mind's eye as far as possible, as well as to consider the condition of joints as we find them after the disease has expanded itself, side by side with our means for rendering such joints useful by operation. Thus the comparative number of cases where a movable joint is obtained as against an immovable one ought to be specially considered, because a more favourable prognosis as to utility can be given in the cases where motion is retained. I believe that movable joints are more numerous than anchylosed ones, and that, as a rule, especially in the joint (knee) now under consideration, by means of tenotomy and a carefully applied orthopædic treatment very useful limbs might be obtained, and which, more especially as regards movement and shortening, compare favourably in

* See Vol. II. of these Transactions, p. 338.

these respects with resected limbs. Thus I hesitate to accept unreservedly the rules that have been laid down (by Hueter) as to the treatment of scrofulous joints.

Dr. FOCHIER, Lyons: I cannot allow to pass without contradiction Professor Hueter's statement relative to the inefficiency of immobilisation in chronic joint disease in children. In the country of Amedée Bonnet many white swellings are cured simply by immobilisation, and the method is not at all likely to be discontinued. On hearing Professor Hueter's statement I am almost led to believe that the disease varies in different countries. As regards resection of joints in children, after having in the past operated freely, I now seldom ever practise this operation. I have lately been able to send a certain number of my cases to the shores of the Mediterranean, and thanks to this climate I have seen some remarkable cures of suppurating joints. On the other hand, after resection of the joint or removal of fungosities, the child remaining in the hospital, its general condition was not improved, the wound again became unhealthy, and assumed much the appearance it had before any operation was undertaken. Quite recently I sent off two cases to the seaside—one a recurrent swelling of the elbow after operation, the other a suppurating fungous arthritis of the same joint: both patients returned after a while cured. Confining my remarks to what I have myself seen, I may say that the articular cartilages are often intact in children. The bone and the synovial membrane are infiltrated, and even suppurating in places, while the cartilaginous part of the joint is hardly if at all eroded. This it is which explains the possibility of re-establishing movement after a suppurating arthritis, and becomes another reason for discountenancing resections, which, *à priori*, have no advantage except that of preventing anchylosis.

Mr. SAMUEL BENTON, London: confined his remarks to the treatment of "chronic scrofulous disease of the knee-joint," and expressed his conviction that prolonged rest in the treatment of these cases might be carried too far. It is not rest, but walking exercise and skilful rubbing of the joint, after all the adhesions have been properly broken down, which is the surest method of securing a useful limb. To break up the adhesive bands requires a sudden, sharp, and quick movement, whilst the patient is under the influence of an anæsthetic, and the patient should be encouraged to walk about directly after the operation. The adhesive bands must be broken where they are thinnest—in the centre; stretching the adhesions or tearing them from their papilla-like insertions is harmful; and after breaking down a stiffened joint to put it up on a back splint is often waste of time. Cases in which the bony structures are affected or in which there are open sinuses communicating with the joint are not suitable for this kind of treatment, but all other cases of chronic strumous disease of the knee-joint can be got well by skilful rubbing and movement after the adhesions have been properly broken down. I have successfully treated several cases in this way, and may mention one of a little girl aged seven years, who had had scrofulous disease of the knee-joint for three years. At the time of the first operation there was pain and considerable serous effusion into the joint, great thickening of synovial membrane, atrophy of muscle of thigh and calf, the limb was flexed nearly to a right angle, the tibia dislocated backwards and every symptom of destruction of cartilage; this case was cured by repeated forcible movement, walking exercise, and skilful rubbing.

Mr. HOLMES, London: The time has so nearly expired, that I can only say

a very few words, and these will chiefly relate to Dr. Hueter's paper. With much that fell from M. Ollier I heartily concur, but am obliged, with regret, to leave the various topics of his address unnoticed. As to the idea that scrofulous disease of the joints is a self-infecting disease, almost locally malignant, as Professor Hueter's ideas imply, it seems opposed to the fact, so constantly observed in practice, that such affections constantly disappear when treated in the case of rich people's children by rest, good air, residence at the seaside, &c. If similar hygienic requisites could be obtained in the case of the poor, probably few operations on joints would be practised. Nor do I feel disposed at once to accept Professor Hueter's rules of practice. Repeated injection of diseased joints, still more the perforation of inflamed bones with a needle in several places, and the introduction of an injection into the medullary tissue, followed after a very short trial by excision, appears to me too rough and "heroic" a method of treatment for a disease so chronic, and which general experience shows to be so curable. Had time allowed, it would have been interesting to have heard more as to the diagnosis of the seat of disease in bones by percussion and auscultation.

After the PRESIDENT had announced that this discussion concluded the work of the Section—

Dr. JACOBI, New York, rose and said: Gentlemen, the vastness of the preparations for the International Medical Congress has been surpassed by its success only. Particularly the foreign members will leave London with a profound gratification, amounting almost to surprise, at the results accomplished, the order of the proceedings, the warm reception on the part of their English confrères, and the generosity and the hospitality displayed by them. In regard to this Section, we have a special cause for congratulation. In the person of our President, Dr. Charles West, we have had a presiding officer who, with his great reputation as a medical man and writer and his knowledge of languages, combined all the great and desirable qualities of a chairman. Patient, urbane, universally both kind and able, he has contributed considerably to the success of our meetings. I move that the thanks of this Section be voted to Dr. Charles West for his earnest, thoughtful, and successful services in our behalf; and to the other officers for their interest in our meetings, their uniform kindness, and for the success attending their industry and punctuality.

Professor SAYRE, New York, re-echoing the sentiments, seconded the vote, which was carried by acclamation.

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*On Some Points in the Etiology and Treatment of the Tuberculous and Scrofulous Diseases of Infancy and Childhood.**

Dr. THOMAS MORE MADDEN, Dublin.

As the value of clinical experience depends mainly on the extent and accuracy of the observations on which it is founded, I may premise that I have been connected, since its opening, with the first special hospital for sick children established in Dublin.

* This paper was accepted by the Council, but from want of time was not read during the meeting.

s institution we have now recorded on our books nearly 23,000
e diseases of infancy and childhood, which have been treated in our
extern departments. Of these 1744 were intern patients.
rge proportion of the cases thus brought before us, the scrofulous or
is diathesis was the *fons et origo malorum*, and within the past few
number of cases thus caused has increased from 35 per cent. in
8 per cent. in 1881, of all the diseases treated at the hospital.
amongst the causes of this greater frequency of tuberculous diseases
eckoned the growing indisposition to suckle their children which is
eable amongst women of all classes, and the unsuitable or injurious
of some of the preserved or artificially prepared substances which are
only substituted for the only natural aliment of an infant.
robability of tuberculosis being thus occasioned is a question of great
mportance. In his recent treatise on "Bovine Tuberculosis in Man,"
hton has shown that tuberculosis, in the human subject, is in many
identical with the tubercular epizootic known as *Perlsucht*, or bovine
sis. And in the acute form of tuberculous disease, so frequent in
nd childhood, the disorder has been observed by Cohnheim, Klebs,
us other writers to "resemble closely the infectious diseases in its
origin, from a specific virus, whether generated in the body from
atter, or introduced from without."
ny other zymotic disorders, milk is well recognised as the agent by
septic germs of disease is carried. The propagation of typhoid fever
ay has been distinctly traced by Dr. Cameron of Dublin to the use
us infected. And the evidence adduced by Dr. Creighton as to the
cal identity between the tubercular *Perlsucht*, affecting the serous
es of animals, fed, for the purpose of experiment, on the milk of
cows, and human tuberculosis, shows the possibility of the disease
is propagated in the human subject, who is equally liable to this
infection when fed on the milk of unknown cows, as the disease is
and does not materially lessen the quantity of milk.
dly, to some extent the larger proportion of tubercular or scrofulous
bserved among our patients may be ascribed to the increasing inebriety
rty of the families of the labouring classes; for, notwithstanding the
ement of wages, the families of even well-paid mechanics and artisans
oorer than was formerly the case. This is owing not so much to the
ding rise in the price of the necessaries of life within the last few
it is to the daily increasing habits of drunkenness in both sexes, in
fspring the toxological effect of alcoholism is evinced by physical
ion and increased tendency to scrofula, tuberculosis, and other diathetic
This point is continually forced on the notice of the medical officers
spital. In treating the tuberculous children of the poor we are often
o supply, by cod-liver oil, Parrish's or Dusart's syrup, extract of malt,
r preparations of the same kind, the want of food of which these
children have been defrauded by their drunken parents, and the
of which is a prime factor in the development of chronic tuberculosis
la.
lly, local circumstances have, I believe, much to do with the preva-
scrofulous and tubercular diseases in Dublin. The situation of the
ie low-lying, badly-drained valley of the Liffey, the densely inhabited
s, too generally devoid of the most necessary sanitary requirements,
the poorest class of a poverty-stricken population are crowded together,
uestionably a deteriorating influence on the physical condition of the

ill-fed scrofulous children, who from thence recruit our hospitals and prematurely fill our cemeteries.

In considering the etiology and pathology of tuberculosis, and especially of pulmonary tubercular disease, those of us whose professional career extends over the last couple of decades must be struck with the entire change in the nomenclature now applied to these disorders as compared with that used in our student days. And in noticing this, some have been tempted to inquire whether the change of doctrine thus expressed is as real as it is apparent, or is this but another illustration of the tendency to employ new sesquipedalian Teutonic verbiage in the place of plain English words, without any corresponding change of idea being conveyed thereby, which is noticeable in other special branches of modern medicine. Many of the polysyllabic pseudo-scientific terms in which some writers appear to delight are almost irresistibly suggestive of that "patched and piebald language" of which Hudibras

> —— "had supplies so vast and large,
> For he could coin or counterfeit
> New words with little or no wit;
> Words so debased and hard, no stone
> Was hard enough to touch them on:
> And when with hasty noise he spoke 'em,
> The ignorant for current took 'em."

The prevailing doctrine concerning the pathology of pulmonary tuberculosis is to some extent the revival of an old idea in altered phraseology. But this is too common nowadays to excite any attention, for we can hardly read up the literature of any department whatever of medical science without being struck with the frequent coincidences between long-forgotten opinions of our ancestors and the newest revelations of modern science.

Thus the present discussion as to the etiology and pathology of pulmonary tuberculosis as a consequence of scrofulous taint was anticipated years ago by Dr. Matthew Ballie, who described the diffused deposits in phthisical lungs as being evidently scrofulous, asserting that whether found in the lungs or any other organs in which this scrofulous matter co-existed with tubercles, both are equally susceptible of undergoing caseous degeneration.

It would be impossible to consider in a short paper so wide and so vexed a question as the relation of scrofulous affections to tuberculosis; but without entering on any lengthened or pathological disquisition, I may venture to say that I still adhere to the doctrine which was impressed upon my mind many years ago by clinical observation in the wards of the scrofula-haunted civil Hospital of Algiers, where I had an opportunity of seeing every phase of this disease. And I have no hesitation in reiterating the opinion that the scrofulous diathesis is the prolific source of all tuberculous diseases, whatever part of the body may be affected—whether it be the lungs, the meninges or substance of the brain, or spinal cord, the mesenteric glands, the cancellous structure or articulating surfaces of the bones, or the external glandular system.

The pathology of scrofulous lymphatic glands, as recently described by Professor Treves (*British Medical Journal*, April 30, 1881), and by those other pathologists who, with him, hold the distinctive character of scrofulous inflammation, I venture to think, would rather sustain the older doctrine of its identity with tubercular disease. In both we have a morbid process, commencing in the medullary or deeper portions of the affected glands or structure; in both we have the formation of large cells with glistening protoplasm, indistinguishable from the "giant cells of phthisis," as described by Virchow, within the lymphatic and in the diseased tissues; in both we have similar reticulated products

of inflammation, the coagulated lymph, later on becoming opaque and retaining the rounded form. As the disease advances these opaque or caseous spots become foci of irritation, leading to suppuration, and giving rise to purulent cavities or infiltration, extending over greater or small areas of the inflamed structure. It would be useless to trace this analogy between tubercular and scrofulous inflammation through all its stages.

In my work on change of "Climate in the Treatment of Consumption," &c., the first edition of which was published many years ago, as well as my later guide to "The Health Resorts of Europe and Africa," I have dwelt upon the connection between tuberculosis and the scrofulous diathesis, and I am well pleased to find that the views I therein expressed concerning the causation of pulmonary tuberculosis are practically identical with those now generally adopted, and which rest on the basis of advanced modern pathology. As Dr. Pollock says, "Scrofula is the key to the whole sequence of these changes. . . . A scrofulous disease of mucous membrane, or of joints, or of a lymphatic gland, has furnished the detritus of a caseation, which was afterwards carried by vessels or lymphatics into the lung, and originated the destructive changes in question."

Miliary tuberculosis, although by no means confined to the pulmonary structures, or necessarily more frequent therein than in other organs, is still, when primarily attacking the lungs, best described by the old name of acute or galloping consumption.

In the Dublin Hospital for Sick Children we have now seen a large number of cases in which pulmonary miliary tuberculosis has passed through all its successive stages within a period of from three to four weeks, from its first manifestation until its victim's death. In one of these cases less than a fortnight elapsed between the commencement and fatal termination of the disease.

This fearful acceleration of the race to death, and the generally attendant deposition of similar tubercles throughout the body, especially in the meninges and substance of the brain, the peritonæum and liver, &c., in such cases leaves little room to doubt the accuracy of Buhl's view, viz., that "miliary tuberculosis is an infectious disease, produced by an auto-inoculation with caseous matter in the body."

The accidental or catarrho-pneumonial origin of phthisis, in many instances, is most evident in the rapid development of pulmonary tubercles, as the sequence of bronchial catarrh and pneumonia in the strumous children of the poor, debilitated by semi-starvation and exposed to cold and damp. Virchow holds that caseous retrograde metamorphosis is invariably antecedent to miliary tuberculosis, and Dr. Pollock observes that tubercle is but a secondary superadded result, arising from inflammatory residua of inflammation that have undergone caseation.

Notwithstanding any pathological objections to the contrary, I believe that none whose experience is founded on actual clinical observation at the bedside in the wards of an hospital, and not confined to histological investigation, will question the fact that some cases of miliary tuberculosis are essentially chronic in their course. In such cases, it would be almost impossible to draw any clear symptomatic distinction between pulmonary miliary tuberculosis, when thus chronic and true, or catarrho-pneumonial phthisis, as developed in early childhood. In both cases we have to deal with a wasting disease, the most evident general symptoms of which are cough, hectic fever, acceleration of pulse, increased frequency of respiration, sudamina, diarrhœa, heightened morning temperature, with evening falls. The physical signs discoverable in auscultation and percussion have, moreover, almost the same sequence

in both cases. In the former, however, we have in addition the symptoms occasioned by the secondary or concomitant deposit of miliary tubercle in the organs and tissues.

The rapidity with which the prodromal catarrh of scrofulous children becomes developed into phthisis is often almost inexplicable, considering the multiplicity and complexity of the pathological changes involved in the progress of the disease, from the first invasion of an attack of bronchial catarrh to the caseous degeneration, and more or less complete alveolar disintegration of the affected lung.

Mesenteric phthisis, or tabes mesenterica, is a not infrequent form of tubercular disease in our hospital practice, where some thirty-seven intern and nearly five hundred extern cases of this kind have now come before us. In these cases, the prominent symptoms were great emaciation, increased or perverted appetite, at first obstinate constipation, and in the more advanced stage diarrhœa. The skin is harsh and rough, the tongue was generally preternaturally clean and red, the child's breath offensive, and, on examination, the abdomen is then found enlarged, tense, and generally tympanitic; so that it is a matter of some difficulty to ascertain the enlargement of the diseased glands, which in two cases I have seen situated immediately below the umbilicus, and in one in the right hypogastrium. These tubercular masses varied in size from a small hazel-nut to that of an orange.

With regard to the treatment employed, it is difficult in our hospital, the disease being essentially chronic, to ensure that long-continued attendance, or that steady perseverance in the regimen and remedies ordered, that are essential in such cases. Still, however, I have had sufficient evidence of the curability of this disease under favourable circumstances, and may therefore briefly allude to the by no means novel treatment which appeared most serviceable. In all cases the patient should be placed on a nutritious diet, varied, of course, according to the child's age, but always containing a fair proportion of well-cooked fresh meat with vegetables. In the first stage of the disease, purgatives, generally in the form of a mixture containing turpentine and Peruvian balsam, were administered for a considerable time for the purpose of unloading the prima via, as well as of stimulating the intestinal glands, and removing that hard tympanitic distension of the belly which is so general in this stage of the disease. At the same time cod-liver oil and syrup of iodide of iron were exhibited. Small doses of grey powder were then generally prescribed, and great benefit was derived from the inunction of a very dilute calomel ointment over the enlarged glands.

Amongst the medicines which I have found of most use in the treatment of this disease in private practice, the pancreatic emulsion, for which the profession is indebted to Dr. Dobell, holds a high place.

Scrofulous disease of joints.—A considerable number of cases of scrofulous diseases of the knee and ankle joints, as well as a few cases of morbus coxæ, and scrofulous disorganisation of the elbow joint, have come under treatment at our dispensary. In the majority of these instances, the constitutional evidences of the scrofulous diathesis were well marked, but in some few cases there was no sign of scrofula other than the local strumous inflammation of the affected joint.

It would be useless to follow, seriatim, the chronic strumo-tubercular disorders which have come before us, in the practice of the Dublin Hospital for Sick Children, as these include almost every disease of this class. The following table, however, shows the comparative frequency of some of these diseases in 1744, intern cases treated in that institution:—

TABLE.

Phthisis, chronic and acute	108	Scrofulous disease of spine	51
Scrofulous disease of cervical and other lymphatic glands	105	Mesenteric disease	43
Scrofulous disease of the joints	95	Tubercular meningitis	16
Scrofulous disease of skin	59	Specific scrofulous ulcers	42
Phlyctenular ophthalmia	59	Scrofulous osteoporosis	8
		Hepatic tuberculous disease	8

The existing prevalence of the scrofulous or tuberculous diathesis is also of considerable practical interest in another and much neglected aspect. I now allude to the light that is thus thrown on the causation of the increasing frequency of the disorders which, in after-life, come under the care of gynæcologists. For, as I have shown elsewhere, the scrofulous diathesis is the most common predisposing cause of chronic endocervitis or endometritis, and consequent sterility, and various other female functional derangements. As this subject is one which has attracted much less attention than its importance demands, and indeed is still unduly ignored in practice by gynæcologists, I take this opportunity of expressing my views as to the frequent connection between a strumous childhood and the subsequent occurrence of the most common forms of chronic uterine or peri-uterine disease, the matter being one which comes fairly within the scope of the present discussion.

In the first place, therefore, I may mention that my attention was originally called to this point by observing that amongst the patients attending the Dispensary for Diseases of Women attached to the Dublin Lying-in-Hospital, a considerable proportion of those suffering from subacute uterine or peri-uterine disorders, such as chronic endometritis, cervicitis, areolar hyperplasma of the cervix, chronic ovaritis, menstrual disorders, tendency to repeated miscarriages, and sterility resulting from the same cause, were persons obviously of the scrofulous diathesis, or were actually suffering from some other evident scrofulous disease at the same time.

I am confirmed in this view by my experience during the last three years in the Gynæcological Department under my charge in the Mater Misericordiæ Hospital, where I have traced, in many instances, the clearest etiological connection between scrofulous taint and chronic uterine or ovarian disorders.

In the cases of cervicitis, leading to ulceration, which were thus caused, the character of the local complaint was clearly expressed with the unmistakable scrofulous type. The utero-vaginal secretion, in these cases, closely resembling the glairy discharge of the specific nasal catarrh of scrofulous children, and being attended by a similar tendency to the production of excoriations of the mucous membrane, gradually extending to the submucous tissues. Like other forms of scrofulous inflammation, that affecting the cervix uteri is apt to thus lead to the formation of ulcerations of the characteristic strumous aspect, irregularly circular in shape, superficial in depth, pale and flabby looking, possessing little natural sensibility, tedious beyond patience when neglected or maltreated, but readily curable when appropriate anti-strumous constitutional treatment is conjoined with the use of the suitable local applications required, and too generally exclusively relied on in such cases.

It need hardly be observed that it would be manifestly impossible to attempt even the most superficial outline of the general treatment of the scrofulous and tuberculous diathesis, within the narrow limits of this paper. This treatment may be considered in a twofold aspect—the first being dietetic and hygienic, and the second pharmaceutical; and on each of these volumes might be written.

Therefore, in the few minutes now at our disposal, I shall merely allude to one or two of the remedies from which I have seen benefit in the general treatment of chronic strumo-tuberculous disorders; and add some observations, founded on my own experience, concerning the curative influence of change of climate, and the use of certain mineral waters, in such cases.

Amongst the remedies most generally applicable in all chronic scrofulous or tuberculous disorders, none are so important as those which act dietetically, as well as medicinally, by rectifying the primary error of nutrition, which is the starting-point in all such cases. Foremost amongst these are cod-liver oil, maltine, extract of malt, and the various preparations of iron, especially the combinations of the lacto-phosphate of iron, with lime and other salts, such as Dusart's, Parrishe's, or Fellowes' compound syrups.

In the majority of the strumo-tuberculous cases under my care at the hospital, I have administered cod-liver oil, either by itself or combined with an equal quantity of syrup of iron, or what I regard as the most valuable method of administering remedies such as this, to infants as well as of those older patients whose stomach rejects the medicine—by inunction.

The carrageen moss, or *Chondrus crispus*, although now little employed beyond the immediate vicinity of its habitat on the south-west coast of Ireland, is one of the most valuable food medicines which can be used in the chronic disorders now under consideration. This species of alga, of which the sea-shores of Clare furnish an inexhaustible supply, was originally recommended in the treatment of pulmonary tuberculosis and scrofulous diseases, by Mr. Todhunter of Dublin, upwards of half a century ago; but at the present day its use as a highly nutritious article of food, as well as an antistrumous specific, appears to be almost entirely unknown, and hence I take this opportunity of again bearing my testimony to its importance as a cheap, abundant, easily prepared, agreeable, and generally most serviceable article of food for children suffering from any of the chronic tubercular diseases which are connected with or originate from the strumous diathesis.

Iodine is admittedly the only medicine that can be regarded as possessing anything like specific power in these cases, and the reason more benefit is not derived from it appears to me to be owing to the non-employment of the metallic iodine recommended by the older writers. And my own experience would lead me to recommend the long-continued administration of metallic iodine in the minute doses originally suggested by Lagol, and given in combination with the iodide of iron and iodate of quinine, until distinct iodism is produced. A combination of this kind, which I have found of singular benefit in the treatment of the scrofulo-anæmic cases, so commonly met with in the out-practice of the hospital, consists of two to four drops of tincture of iodine, half a grain of iodate of quinine, and a grain of iodide of iron, given two or three times a day, in any suitable vehicle.

With regard to local applications to the parts implicated by scrofulous disease, there can be no question of the utility in some cases of the deep hypodermic injection of a few drops of tincture of iodine into the substance of scrofulous glands, nor of the use of cataplasms of the *Fucus vesiculosus* in similar cases and in strumous ulcerations. In the latter I have seen great benefit from the application of poultices of this common sea-wrack, and as I am not aware that this simple but most efficacious application is mentioned in any of the text-books on diseases of children, I now refer to it.

These sea-wrack poultices require to be persevered in, without intermission, for a considerable time, and to be changed at short intervals. When used in this way, and being combined, of course, with appropriate constitutional treat-

ment, I have often seen the most obstinate scrofulous sores, that had resisted all other remedies, in a short time after the application of the wrack poultices, commencing to assume a healthier aspect, gradually contracting in size, and leaving hardly any trace of their existence.

I might readily adduce cases in support of this assertion, but one will suffice however.

J. H., aged about ten, the scrofulous child of scrofulous parents, was treated at the dispensary and in the infirmary from December until the end of the following April. For a considerable time previously he had been under treatment in another hospital, where the disease was at last pronounced incurable, and when brought to the dispensary, it was chiefly in order that he might obtain a certificate for the Hospital for Incurables. On first sight I was inclined to agree in the opinion that the case was a suitable one for that institution. The child was worn and hectic looking, and on being examined, a large sore was found extending on the right side from the angle of the jaw in front along the sterno-mastoid muscle, and backwards along the trapezius from its origin to the scapular insertion, and spreading over the forearm as low down as the insertion of the deltoid; whilst on the opposite side the ulceration corresponded very closely with the extent of the platysma myoides. Throughout this vast tract of disease the ulceration was foul and unhealthy looking, the edges were thickened and everted, and the surface covered with an offensive and ichorous discharge. A mixture containing cod-liver oil with syrup of iron, and minute doses of iodine, was ordered, and the ulcerated surface was covered with sea-wrack poultices; these were changed frequently during the day, and kept constantly applied for fully three months, at the end of which there remained only two small healthy granulating ulcers, one on the right forearm, and the other on the neck. The child, though still thin and pale, had quite lost the cachectic worn aspect he presented when first brought to the dispensary. He was now pronounced convalescent, and was sent to the country for change of air.

The hygienic and climatic treatment of the strumo-tubercular diseases of early life is a subject of considerable practical interest, as hygiene and climate are all-important in the cure of all chronic disorders of this class. And although, unfortunately, we can only extend the advantages of favourable hygienic and climatic conditions to that small proportion of our patients who are the children of the wealthy, still so great are the remedial advantages of such treatment, that it obviously demands our most careful consideration.

The therapeutic influence of change of climate and the use of various mineral waters in the treatment of chronic tubercular and scrofulous diseases of childhood and youth, as well as in other chronic disorders, is a subject on which I can speak with some confidence, having devoted attention to it during several years of foreign travel in pursuit of health or in attendance on invalids in the winter health-resorts of Europe and Africa, as well as at the spas of Germany, France, and Italy. And I may here repeat that the accuracy of my views on these topics has been proved by the freedom with which they have been borrowed from my works on "The Health Resorts of Europe" and "The Spas of Germany, France, &c.," by other writers.

In these works, as elsewhere, I have long maintained that in chronic tuberculosis, and in all scrofulous affections of childhood and youth, no remedies are so generally useful as certain mineral and thermal waters, when used at their sources and conjoined with change of climate. The disorders we are now considering are essentially chronic in their progress, and hence the action of our remedies must be almost as gradual; for it is vain to expect that a brief

course of any remedy can suddenly undo the effect of years of disease. In mineral waters of different kinds, suited to the exigencies of each special case, we have therapeutic agents of undoubted power, the action of which on the animal economy is generally so gradual that they require to be continued for long periods to produce their curative effect.

Three distinct classes of these waters may be used in the treatment of the different forms of chronic strumous and tubercular complaints. The first are the iodated and bromated saline springs, the Iod-und-Bromhältige Kochsalz-wässer, as the Germans term spas containing iodine and bromine, generally in the shape of bromide of manganese and iodide of sodium, dissolved in a muriated saline water. Springs of this kind are seldom thermal.

The most important of these iodated or bromated spas are Wildegg, in Switzerland, which is a most powerful remedial agent in all glandular scrofulous diseases. This water must be used with considerable caution, however, and in small doses of from 2 to 4 ounces twice daily, as in larger quantities it soon produces complete iodism.

Amongst these iodated and bromated springs, besides that just mentioned, we have sufficiently wide choice from which to select a suitable spa for the treatment of glandular enlargements and other scrofulous disorders. If the circumstances permit of the patient going to Germany, we have there, at Kreuznach, Halle, or Salzhausen, strongly iodated waters, whilst for those for whom our English watering-places would be more suitable, we have at Woodhall Spa a mineral spring hardly inferior in therapeutic value to any of its foreign rivals.

The second class of mineral waters above alluded to as applicable to the treatment of some chronic strumo-tubercular disorders are the simple chalybeates, such as Spa or Schwalbach. These waters, which contain little more than the carbonate of the protoxide of iron, with an excess of carbonic acid, are powerfully tonic and stimulant in their action, increasing the number of red corpuscles and amount of hæmoglobuline in the blood, and are thus of special service in the diseases we are considering.

Saline chalybeate springs, such as the Statelbrunnen of Homburg or Kissengen, or Tunbridge Wells, or the Kissengen Spring of Harrowgate, may also be used in certain scrofulo-tubercular complaints.

Another class of mineral waters also applicable, though in a somewhat more limited class of scrofulous or tuberculous cases, are the cold sulphurous waters, such as Lisdoonvarna in Clare, Enghein-les-Bains, the old well of Harrowgate; the warm sulphurous spas, such as Aix-les-Bains, Eaux Bonnes; and the other Pyrenean thermal sulphurous springs, Aix-la-Chapelle or Schiznach—although frequently prescribed in tuberculous affections, and most useful in appropriate cases, especially of glandular enlargements, these, however, require to be used with very great caution in such cases, being powerfully stimulating in their action on the vascular and cerebro-spinal nervous system.

The thermal arseniated mineral waters of Royat, Mont Dore, and St. Nectaire, in the volcanic district of Auvergne, may be used in the treatment of scrofulous glandular enlargements, and some cases of strumous disease of the joints, with great advantage.

In change of climate we have another, and often the most efficient, remedy for chronic tuberculosis. In no patients may we more confidently hope for the beneficial results of change of air than in the case of children predisposed, either by hereditary descent to scrofulous diathesis, or physical conformation to consumption. The constitution, as yet unformed, may be expected to receive and retain whatever impression a pure, bracing, and mild air may produce, and

thus pass in safety over that critical period which intervenes before puberty. The climate required for this predisposition to phthisis is one characterised by a dry, moderately warm, and bracing atmosphere, dryness being the essential condition necessary. The health resorts which are now most in vogue for the treatment of this class of patients are the cold, dry tonic climates of Alpine districts, such as Davos-Plaz, in the elevated plateau of the Engadine, and other similarly situated mountain sanataria. For my own part, I regard the preference given to these cold, bracing mountain climates, although very judicious in the case of children who are suffering merely from the general debility of the undeveloped strumous diathesis, as mistaken when extended to developed tuberculosis, and especially to pulmonary tuberculous disease. The diminished pressure of the atmosphere in these Alpine health resorts, and the sudden variations in its electrical condition and temperature as well as pressure, to which such places are generally subject, must influence the balance of the circulation and tend to occasion congestions and hæmorrhages.

The primary effect of a cold, dry Alpine climate and pure mountain air, such as that enjoyed at Davos, is unquestionably tonic, and braces up the strumous child whose disease has been fostered by life in the impure, variable, and generally humid atmosphere of some of our great centres of population.

When a scrofulous youth is removed from such unfavourable conditions to a dry, cold climate, such as that just referred to, the respiration becomes more energetic, more oxygen being required and more carbonic acid being exhaled, to supply which more food, especially of a fatty character, is consumed. Thus the blood is enriched by a greater proportion of hæmoglobuline, the red corpuscles, the oxygen carriers, are augmented in number, and the general nutrition of the system is improved.

But this picture is not without an adverse view; and, on the whole, the advantages of Alpine climates, even in such cases, are by no means so certain as some believe.

Whilst those who, though of the diathesis we are considering, are still moderately vigorous may thus benefit from the cold mountain climates now in fashion as health resorts, those in whom the constitutional inroads of scrofulous or tuberculous disease are already pronounced will be very differently affected. For whether the influence of cold may be therapeutic or pathological will depend mainly on the state of health and power of resistance of the individual exposed to it, the very young and old, or those debilitated from any cause, being of course the most susceptible to the depressing action of great cold.

In such cases the blood repelled from the skin is thrown on the internal structures of the body, deranging the balance of the circulation and occasioning congestions, especially of the brain and lungs. And even where these effects are not occasioned, a probable result of subjecting debilitated scrofulous children to any of these cold climates would be, by depriving them unduly of their natural caloric, to arrest the development and lower the activity of mind and body.

For these reasons, therefore, I would much prefer some of the moderately warm, equable, and dry climates as winter resorts for the generality of tuberculous children or youths. Of such climates we have an abundant choice. And of these, according to my own experience, acquired during some years of health travel, those which best combine the required advantages are Western Australia, Upper Egypt, Heyeres and the Riviera generally, and Malaga.

In selecting a winter residence for children suffering from or predisposed

to tuberculous diseases attention should be paid to the facilities and inducements it affords for open-air exercise. Consumptive children are generally dressed for the least fatigue, and live to hang over the fire, whence their friends fear to distract them lest they might thus "catch fresh cold." But how mistaken is this view. In the case of the strumous-tainted inheritors of the predisposition to phthisis it is too obvious to need any comment. For such children above all free exercise and exposure to the pure fresh air and sunlight are essential, as by withholding these the tuberculous diathesis may be developed in any child. Hence, in choosing a health resort in the cases referred to, preference should be invariably given to one, the climate of which will permit with safety of the maximum exposure to the open air, and the situation of which will afford the largest inducement and opportunities for free outdoor exercise.

In these respects the new Alpine sanataria are as unfavourably circumstanced as the older health resorts of Pau and Mentone, from which they differ in all other respects, climatic and topographical.

For the same reason none of the artificial sanataria now sometimes recommended in any home climate, be it ever so suitable in other respects, can compensate for the lesser amount of open-air exercise which can be there taken, when compared with the amount which the invalid would enjoy throughout the winter in a dry, warm, and equable climate such as that of Malaga. And, moreover, in our British wintering resorts, not only are valetudinarian children confined to the house for all the wet and cold days that prevail, but also they are further injured by the use of coal fires and gas lights, which in English houses fill the air with carbonaceous particles that must necessarily irritate the sensitive lungs of a child predisposed to pulmonary tuberculosis.

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INAUGURAL ADDRESS,

BY THE PRESIDENT,

Dr. ALFRED H. McCLINTOCK, LL.D.

ing the Obstetric Section of this seventh International Medical Congress, the ost gratifying duty that devolves upon your President, is to offer an earnest welcome to those obstetric members who have come from other nationalities, istant British colonies, to take part in this—the largest convention of medical as ever, perhaps, assembled together at any one time or place.

nt this cordial salutation, not only on the part of the officers and council of lar Section over which I have the honour to preside, but also on the part of icians and gynæcologists of England, Scotland, and Ireland.

proud and happy to meet here on British ground, so many of our brethren us parts of the civilized world, but especially from Germany, France, and nd to accord them a friendly greeting, not only out of respect to their indi- its and high reputation, but as representing those great obstetric schools over names of Mauriceau, Levret, Baudelocque, and Dubois; of Roederer, Siebold,

Nägele, Kiwisch, and Scanzoni; and of Bard, Dewees, Meigs, and Hodge, have severally shed such imperishable lustre!

Not the least of the important objects contemplated in this Congress is, the interchange of friendly feelings among its members. I am fully persuaded that our reunions will be attended, not alone with benefit to us all by the attrition of mind with mind—but that new friendships will be formed, and old friendships confirmed, and that sentiments of mutual respect and regard will be developed, so as to strengthen the bond of brotherhood which should unite us as fellow-workers in the same department of medicine.

Allow me, before going further, to express my deep sense of the unexpected, unmerited dignity which the Congress has conferred by putting me into the position of President of this important Section; I feel it to be the highest and most flattering honour of my long professional life. Such a compliment more than repays one for forty years of labour and devotion; for it sets the seal of approval by contemporaries on my past life, and leaves nothing further or higher to aspire to in the way of professional distinction. At the same time, gentlemen, this feeling of just pride and exaltation is mingled with a very poignant sense of incapacity, and I might well shrink from the responsibility of the post, but that, in the discharge of its duties, I shall have the aid and co-operation of such accomplished men as those who constitute the Vice-Presidents and Council of the Section; they in truth are the giants on whose shoulders I am raised to the exalted position it is my good fortune to occupy in this Congress.

When about to prepare an Address it struck me that, as there would be no lack of able essays on general and particular subjects connected with the business of this Section, it might be well perhaps to deviate from the beaten track, and to take some other direction for my remarks.

If this course do not meet your approval, you have, at all events, the satisfaction of knowing that your patience will not be long taxed.

Inasmuch, then, as this is the first occasion of the International Medical Congress meeting in London, it may not be inappropriate if I pass in review some of the more prominent among the many eminent obstetricians who lived and practised in this city, who, by their writings, teaching and discoveries, have contributed in no small measure to the development of midwifery and gynæcology, as well as to the medico-chirurgical fame of London.

I must, however, study brevity, being desirous, if possible, to keep within the fifteen minutes allowed for the readings of communications, so as to set an example of obedience to the rules of the Congress.

In this retrospective glance, I find only one name standing out in the sixteenth century:—Thomas Raynald, the translator of Eucarius Rhodion's celebrated treatise "De partu Hominis." The original English edition, by Raynald, appeared about 1540, and was the first distinct treatise on midwifery in the English language, and for over 100 years was the sole guide and text book of obstetric practitioners, male and female.

In the early part of the seventeenth century, the immortal William Harvey (*tanto nomini nullum par eulogium*) stands forth conspicuous, the splendour of his fame increasing as years roll on. He spent most of his time here, being physician to the King; and he delivered courses of lectures, at the Royal College of Physicians, on anatomy and surgery. As a practitioner we know from the testimony of his contemporaries that Harvey excelled in midwifery, and in the treatment of female diseases.

Before the pubication of his celebrated exercitations, on generation, parturition, conception, &c., there were, according to Dr. Aveling, "but three works on midwifery in our language; these were translations from Rhodion, Rueff, and Guillemeau. His was the first book on midwifery written by an Englishman, printed in our own language, and the influence which it had upon the practice of the time would with difficulty now be

estimated. His claim therefore to eminence in our department of medicine is beyond question." With this conviction on our minds, we shall the more heartily yield our applause when his magnificent memorial statue is unveiled at Folkestone, the place of his nativity, on Saturday next—a ceremony, I may remark, which has with good taste and judgment been purposely so arranged that this great Medical Congress may take a part in showing honour and respect to the memory of one of the greatest discoverers in the science of medicine, and, consequently, one of the greatest benefactors to the human race.

Contemporary with Harvey was another remarkable man—PETER CHAMBERLEN—the inventor of the midwifery forceps, indisputably the most valuable instrument of the whole *armamentarium chirurgicum*. Unfortunately for him, however, the brilliancy of his reputation is obscured by the unworthy selfish conduct which caused him to keep the instrument a secret for the aggrandizement of himself and family. He was father of Dr. HUGH CHAMBERLEN, the translator into English of Mauriceau's works, and grandfather of Dr. Hugh Chamberlen, to whose memory a handsome monument in Westminster Abbey was erected by his patron and friend the Duke of Buckingham. No less than four generations of the Chamberlen family were eminent in the medical profession here; and Dr. Peter, who attained a great age, had been physician to five English sovereigns.

Towards the close of this (seventeenth) century, RICHARD WISEMAN, "Serjeant Chirurgeon" to Charles I., published his treatise on surgery, in which he gives an excellent description of pelvic abscesses consequent on parturition. He thus anticipated Puzos' essay on the same disease, and put forward much more rational and correct views as to its pathology.

The eighteenth century was destined to see a marvellous development of midwifery, as well as of many other arts and sciences. As might therefore be expected, London can boast of several eminent obstetricians at this period.

In chronological order, the first to be mentioned is Dr. JOHN ARBUTHNOT, F.R.S., and F.R.C.P., physician to Queen Anne. Although he has left no enduring evidence of obstetric superiority, yet he was an eminent accoucheur in his day, and reflected infinite credit on our order by his rare literary talents, his deep scholarship, and his exalted social position. He was skilled in everything that related to science, and held a prominent place among the ablest writers and wits of that Augustan age; one of whom (Swift or Pope) alludes in his poetry to

"Arbuthnot's soft obstetric hand."

A man who was considered a friend and an equal by Parnell, Gay, Bolingbroke, Swift, and Pope, could not fail to adorn any pursuit to which he devoted his vast intellectual powers. Speaking of him, Swift said, "He has more wit than we all, and his humanity is equal to his wit." A higher tribute could not have been paid him.

The next to be mentioned is Dr. JOHN MAUBRAY, not on account of any peculiar merit in either of his works—"The Female Physitian," and "Midwifery brought to Perfection,"—but because he is reputed to have been the first public teacher of midwifery in this country. He lectured, Dr. Denman tells us, at his house in Bond Street, so far back as the year 1724.

Nearly cotemporary with Maubray was DR. EDMUND CHAPMAN. He was the second public teacher of midwifery in this city, and is entitled to our lasting gratitude for having been the first to publish to the world a description of that "noble instrument" (to use his own phrase), the obstetric forceps, the secret of which the Chamberlens kept to themselves for over fifty years. This he did in the "Edinburgh Medical Essays," and subsequently in his treatise "On the Improvement of Midwifery chiefly with reagrd to the

Operation:" the operation meaning the application of the forceps. The first edition of this book came out in 1733.

About this same period also lived Sir RICHARD MANNINGHAM, F.R.S., a man of considerable learning and of great reputation as a successful midwifery practitioner. He was author of some obstetric works of temporary consequence, and his claim to remembrance arises from the circumstance that, in the year 1739, he opened a ward in the Parochial Infirmary of St. James', Westminster, exclusively for the reception of parturient women, which was the first thing of the kind in Great Britain. Shortly afterwards the idea was taken up and enlarged upon elsewhere, and the great Lying-in Hospital of Dublin was founded by Dr. Bartholomew Mosse; being the first hospital of the kind in the British Dominions.

The very same year that Sir Richard Manningham opened his obstetric ward in St. James' Infirmary, as we have just seen, a surgeon from a small country town in Scotland established himself here in London as an accoucheur, who ultimately effected the greatest reformation that had yet taken place in the principles and practice of obstetrics. This man was WILLIAM SMELLIE, a name always to be respected wherever midwifery is cultivated as a science. For twenty years Smellie practised and taught here; and published the first volume of his celebrated treatise in 1751, and his splendid anatomical plates in 1754. Amongst his pupils who later on became eminent in the same branch of medicine, were William Hunter, Denman, David McBride (of Dublin,) John George Roederer (subsequently Professor of Midwifery at Göttingen), Dr. James Lloyd, of Boston, U.S., and Dr. William Shippen, afterwards Professor of Midwifery, in the Pennsylvanian University: these last being, according to Professor Parvin, "the two first American obstetric practitioners." Most gladly would I linger over the life and works of this great man, but I must content myself with a few sentences.

Smellie possessed a wonderful capacity for work, and a clear judgment; but beyond and above this he was endowed with a singularly accurate perception of facts, which made him a correct as well as a close observer of nature. Herein lay the secret of his unrivalled success as a reformer and improver of midwifery. He himself felt this to be so, for in reviewing his practice, he says, "I diligently attended to the course and operations of Nature which occurred in my practice, regulating and improving myself by that infallible standard." (Case 186 Sydenham Society's edition). Truly, he was, in the words of Dr. Hugh Miller, a "noble character and an example of earnest living."

A couple of years after Smellie settled in London, there came to live with him a young man from the Scottish county—Lanarkshire—of which Smellie himself was a native. This young man was no less a person than WILLIAM HUNTER—a name familiar to you all—whose plates and descriptions of the human gravid uterus have gained their author a foremost rank among obstetric writers. By his great reputation as a lecturer and as an anatomist, aided no doubt by his prepossessing appearance, polished manners, and cultivated mind, Hunter proved a successful competitor of Smellie's in practice. Like him, he also gave special courses of lectures on midwifery; MS. notes of which are to be found in many libraries. Dr. Matthews Duncan tells us the College of Physicians possesses two pretty complete volumes of such notes.

In 1748, Hunter was appointed Surgeon and man-midwife to the Middlesex Hospital, and soon afterwards to the British Lying-in Hospital; for though the Physicians claim him as belonging to themselves, yet it cannot be disputed that Hunter was a Surgeon and member of the Corporation of Surgeons of this city.

Besides being a rival, he was in some respects a contrast to Smellie. The school of obstetrics founded by the latter, was not inaptly described by the late Tyler Smith, as the mechanical school, from the importance it attached to the resources of art in

task of supplementing the above list with the names of those eminent London obstetricians and gynæcologists, whom to meet and to know is assuredly the most gratifying of the many privileges connected with this great international gathering.

At the conclusion of the Address, Dr. FORDYCE BARKER, of New York, said: I have the honour of introducing a motion which I am certain will express the unanimous feeling and sentiment of all present. Resolved, That the thanks of the Section of Obstetric Medicine and Surgery are due to its President for his most interesting and suggestive Inaugural Address." The fitness and propriety of this resolution must be so obvious to all that I shall not detain the Section by any extended remarks in its favour. To keep the memory green of those who have gone before us, who have made positive contributions to our science, or who have made valuable improvements in our art, is beyond all question one of the most effective methods of stimulating those of the present age, and those who are to follow us, to increased and progressive labours, and to win honourable reputations by their good and honest work. The roll of distinguished names, with a condensed statement of what our predecessors in obstetrical medicine of London have done to entitle them to grateful and honouring remembrance by us, seems especially appropriate for a meeting of this Section of an International Medical Congress in this city, and must be of great interest to all of us who were "not to the manner born," but who inherit, by right of succession in the same department of study and professional zeal, a common interest in all the great names that have done something of value for all of us. I may be permitted to add one item of information, not mentioned in the Inaugural Address, which may be of some interest, in regard to the "Byrthe of Mankynde," by Raynald, the first work on obstetrics published in England. I possess a copy published, I think, in 1540; and on a blank leaf of this book one of the most eminent bibliophilists of England has written: "This is the first book printed in England with copper-plate engravings."

Dr. WINCKEL, Dresden: Meine Herren! Es ist mir die ehrenvolle Aufforderung geworden unsern hochverehrten Herrn Vorsitzenden zu danken für die Adresse, welche derselbe an uns zu richten die Güte hatte. Es freut mich besonders vor einer so ansehnlichen Versammlung diesen Dank im Namen der deutschen Gynäkologen, die hier versammelt sind, in deutscher Sprache auszudrücken. Sie haben aus dieser Adresse gehört in wie würdiger Weise der Herr Präsident unsern bedeutenden deutschen Gynäkologen gedachte. Wer nun weiss wie sehr seit Jahrhunderten die Deutschen bestrebt gewesen sind die vorzüglichsten englischen, französischen und amerikanischen Rechte zu studiren und sich zu Nütze zu machen und nach Kräften zu verbreiten, wird mit mir die Genugthuung empfinden, dass auch unsere Leistungen und unsere Sprache als gleichberechtigt mit der seiner Forscher anerkannt worden ist. Es wird dies ein neuer Sporn für uns sein auf dem Wege ernster Forschung fortzufahren, stets unter grösster Anerkennung die Verdienste anderer, und so hoffe ich, dass unter dem Vorsitz unseres hochverehrten Herrn Präsidenten auch dieser Congress die besten Früchte tragen wird, in Bezug auf Förderung der Wissenschaft und der Collegialität und den persönlichen freundschaftlichen Beziehungen zwischen den Gynäkologen der ganzen gebildeten Welt.

Dr. TARNIER, Paris: Messieurs,—A notre époque les Congrès Internationaux sont nombreax, mais aucun d'eux ne mérite plus de faveur qu'un congrès médical où se

discutent toutes les questions scientifiques relatives au maintien de la santé publique, à la sauve-garde de la vie humaine, où la science et la philanthropie se disputent le pas. Je puis même dire que la section d'obstétrique mérite une attention particulière, puisque partout on répète avec raison qu'un accoucheur est souvent appelé à sauver simultanément deux existences.

Si j'ai osé prendre la parole au milieu de tant de savants, venus de tous les pays du monde, c'est que je tiens à dire hautement que la France, quand elle consulte ses annales obstétricales, n'a rien à envier aux autres nations. Mais je laisse volontairement de côté tout aperçu rétrospectif sur les progrès de notre art, car, mieux que jamais, nous voyons ici que la science est cosmopolite, et je viens simplement déclarer au nom de mes compatriotes que nous sommes heureux d'être présidés par un homme que doit sa haute situation à son travail et à son amour du bien. Honneur donc au Professeur McClintock, notre Président.

J'ajouterai que nous serions impardonnables si nous n'étions pas reconnaissants du zèle et des efforts du Comité d'Organisation. Tout a été prévu pour faciliter nos travaux et même pour nous récréer. L'hospitalité qui nous entoure est si large que nous marchons, pour ainsi dire, d'étonnement en étonnement. Aussi nous vous remercions bien cordialement et nous formons de tout cœur des souhaits pour que le succès du Congrès médical de Londres soit aussi grand que possible.

Perfectionnement dans la Construction et dans l'Application du Forceps.

Prof. TARNIER, Paris.

MESSIEURS,—Le forceps à courbure pelvienne, dont tous les accoucheurs se servent depuis longtemps, a rendu de si grands et de si nombreux services, qu'on le considère volontiers comme un instrument parfait. Cependant, quand on étudie très-attentivement le mode d'action du forceps ordinaire, on ne tarde pas à s'apercevoir que cet instrument n'est pas irréprochable ; c'est ce qui a été bien démontré, je pense, par Hermann de Berne, Hubert de Louvain, le docteur Chassagny de Lyon et par moi-même.

En effet, le forceps ordinaire est passible de deux reproches principaux :

1.—Les tractions faites sur le manche de l'instrument sont loin d'être dirigées suivant l'axe du bassin et déterminent des pressions dangereuses pour les parois du bassin.

2.—La force est appliquée trop loin du centre de la tête, et le forceps est transformé en un levier dont la puissance est menaçante pour l'intégrité du bassin chaque fois qu'on imprime à l'instrument un mouvement quelconque, soit d'un côté à l'autre, soit de haut en bas, ou en sens inverse.

Hermann de Berne* ne s'est occupé que de la mauvaise direction des tractions et pour y remédier il a imaginé un forceps dont la courbure est si singulière que jamais personne n'a songé à l'utiliser dans la pratique, et que de l'aveu même de son inventeur, les tractions faites sur le manche de cet instrument seraient absolument défectueuses et plus mal dirigées qu'avec un forceps ordinaire. Aussi Hermann complète son forceps en y ajoutant une pièce supplémentaire qu'il emploie de la façon suivante : pendant qu'avec l'une de ses mains il tire directement sur le manche de son forceps, il tient de l'autre main la pièce supplémen-

* "Ueber eine neue Geburtszange zur Extraction des im Beckeneingange stehenden Kindeskopfes." Bern ; 1844.

[illegible] après l'abaissement de l'articulation et exerce en ce point une [illegible] quand la tête fœtale apparaît à la vulve il change de place la pièce supplémentaire. Il insère au-dessous de l'instrument à la hauteur des oreilles et s'en sert pour tirer dans une direction variable avec l'une de ses mains tandis que l'autre main tire directement sur le manche du forceps. En un mot Hermann avec son forceps, fait à peu près ce que ferait un opérateur qui tirerait directement sur les manches du forceps ordinaire, pendant que la tête tournerait en bas et plus ou moins en avant, à l'aide d'un bras passé à travers l'articulation. Mais, que cela vous le savez, a été [illegible] par quelques accoucheurs.

Comme il faut rendre justice à l'ingénieux mémoire de l'accoucheur de Berne, mais je n'irai pas jusqu'à dire que le Dr Fessler Stolz et le Docteur Saenger aient dit que mon forceps ressemblait à celui d'Hermann, car ces deux instruments diffèrent absolument par leur forme et par leur mode d'action. Plus heureux qu'Hermann, Hubert de Louvain* après avoir démontré très-nettement la mauvaise direction des tractions faites avec le forceps ordinaire, a imaginé deux forceps qui tirent directement dans l'axe du bassin. Il a même réussi d'appliquer la force le plus près possible de la vulve en cintrant le manche près de son articulation, afin de raccourcir le levier au bout duquel on agit mais il n'a pas été plus loin.

Le Docteur Chassagny de Lyon† a montré l'avantage qu'il y a à appliquer la force près du centre de la tête, mais il a méconnu l'importance considérable des tractions faites suivant l'axe du bassin.

Je ne m'étendrai pas plus longuement sur les qualités et les défauts des forceps imaginés par Hubert de Louvain et par le Docteur Chassagny, parce que j'ai déjà analysé leurs mémoires dans une brochure publiée en 1877‡ mais je suis heureux de proclamer ici en plein Congrès International le grand mérite qui s'attache à leurs travaux.

Dans une application de forceps, il ne s'agit pas seulement d'extraire un fœtus des parties génitales, il faut encore l'extraire aussi heureusement que possible pour la mère et pour l'enfant. Or, j'avais remarqué qu'une opération heureuse en apparence, et dont on se félicitait tout d'abord, laissait assez souvent après elle des lésions produites par une pression excessive de l'instrument, lésions qui ne tardaient pas à devenir le point de départ d'accidents graves, et quelquefois mortels pour l'opérée. J'ai donc essayé, à mon tour, de modifier le forceps en m'efforçant de le rendre plus inoffensif que le forceps ordinaire.

Mon forceps repose sur les quatre principes suivants :—

1.—Tirer directement suivant l'axe du bassin ;

2. Appliquer la traction le plus près possible du centre de la tête.

3. Laisser à la tête saisie par le forceps la mobilité qu'elle a dans l'accouchement naturel, soit pour se fléchir, ou se défléchir, soit pour pivoter autour de l'axe fictif du bassin.

4.—Avoir une aiguille indicatrice, qui montre à chaque instant à l'opérateur les mouvements exécutés par la tête fœtale, et qui le guide dans la direction qu'il doit donner à ses tractions.

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* Note sur l'Equilibre du Forceps et du Levier, par L. T. Hubert : *Mémoires de l'Académie Royale de Belgique*, 1860.

† "Méthode des Tractions Soutenues. Le Forceps considéré comme Agent de Préhension et de Traction. Preuves Expérimentales." Paris : 1872.

‡ "Description de Deux nouveaux Forceps." Paris : 1877.

Pour réaliser et réunir ces quatre principes sur un même instrument, j'ai longtemps tâtonné, et c'est seulement le 23 Janvier, 1877,* que j'ai fait connaître mon forceps.

A ma grande satisfaction, ce forceps attira l'attention des accoucheurs des différents pays et mérita un assez grand nombre de communications et de publications. Parmi les partisans ou les adversaires du nouveau forceps, je citerai les noms du MM. Pajot, Stoltz, Robert Barnes, Matthews Duncan, Galabin, Lombe Atthill, Fordyce Barker, Baudin, Wedder, Reid, Aveling, Morgan, Nordau, Vincenzo Maggioli, Wasseige, Polaillon, Robert Bell, Charles Bell, Kucher, William Lusk, A. R. Simpson, A. Pinard, Gustav Braun, Carl Braun, A. Reith, J. G. Lyon, Thorburn, Saenger, &c.† Dans tous ces travaux les modifications que j'ai apportées au forceps furent tantôt critiquées, tantôt approuvées. Toujours est-il que la question des règles nouvelles que l'on doit suivre dans la construction du forceps, s'impose de plus en plus aux méditations de tous les accoucheurs. Les principes sur lesquels repose mon forceps sont du moins, je le pense, considerés comme inattaquables; mais la construction d'un forceps qui réalise ou réunit tous ces principes est difficile, et peut varier; c'est ce qui explique les nombreuses modifications déjà subies par mon forceps, et qu'il subira encore.

Mon premier forceps, dont je vous épargnerai la description, se composait, vous le savez, de deux branches de préhension à courbure périnéale, et de deux tiges de traction parallèles à ces dernières, et qui se prolongeaient en arrière, aussi loin que les branches de préhension. Cette disposition instrumentale avait l'inconvénient d'embarrasser l'opérateur quand il voulait appliquer et articuler ce forceps; aussi peu de temps après j'ai coupé les tiges de traction un peu en avant de l'articulation du forceps, et sur ces tiges ainsi coupées j'ai ajouté une poignée mobile, assez longue pour que l'opérateur ne fut gêné, ni par les parties génitales de la femme, ni par le lit sur lequel elle repose. Cette modification instrumentale, qui a été publiée le 6 Juillet, 1878, dans le *Progrès Médical* (p. 517–518), simplifiait le mécanisme de l'instrument; aussi fut-elle adaptée ou imitée par W. Lusk‡ et par Alexander R. Simpson.§

Le forceps ainsi modifié conservait une courbure périnéale analogue à celle du

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* Académie de Médecine de Paris.

† Pajot, *Annales de Gynécologie*, Mars et Mai, 1877; Stoltz, *Archives de Tocologie*, Juin, 1877; Société Obstétricale de Londres, Discussion, 1877; Lombe Atthill, *British Medical Journal*, July 7, 1877; Fordyce Barker, *American Journal of Obstetrics*, January, 1878; P. Budin, "Les Derniers Expériences sur la Compression Cérébrale," &c., in *Le Progrès Médical*, 1878, p. 198; Wedder, *Medical Record*, March 23, 1878; Reid, *Glasgow Medical Journal*, June, 1878; Aveling, Obstetrical Society of London, Séance du 5 Juin, 1878; Morgan, *British Medical Journal*, 1878, June 29; Nordau, *Pester medicin. Zeitsch.*; Wasseige et Polaillon, *Bulletins de la Société de Chirurgie de Paris*, Séance du 23 Avril, 1879; Robert Bell, *Edinburgh Medical Journal*, April, 1879; Charles Bell, *Obstetrical Journal*, May, 1879; Kucher, *Wiener medic. Presse*, 1879, No. 20; W. Lusk, *American Journal of Obstetrics*, April and July, 1880; A. R. Simpson, *Edinburgh Medical Journal* October, 1880; A. Pinard, Article "Forceps" du "Dictionnaire Encyclopédique de Science Médicale," Paris; G. Braun, *Wiener medic. Wochenschrift*, 1880, Nos. 24, 25; C. Braun, "Lehrbuch der Gesammten Gynäkologie," 1880; A. Reith, *Edinburgh Medical Journal*, February, 1881; J. G. Lyon, *British Medical Journal*, March 19, 1881; Thorburn, *British Medical Journal*, June 25, 1881; Saenger, *Archiv für Gynœkologie*, Band xvii. Heft 3, 1881; Wasseige, *Annales de Gynécologie*, Paris, Juillet, 1881.

‡ *American Journal of Obstetrics*, April to July, 1880.

§ *Edinburgh Medical Journal*, October, 1880.

forceps de Johnson. Cette courbure périnéale devait, suivant mes prévisions, bien protéger le périnée, tout en permettant de tirer suivant l'axe du bassin. Cependant, je l'ai abandonnée ; je vais dire pourquoi :—

1.—Avec mon forceps à courbure périnéale, pour que les cuillers s'appliquent régulièrement sur la tête, il faut que les manches de l'instrument soient dirigés presque directement en bas, ce qui constitue une petite difficulté opératoire que ne savent pas vaincre la plupart des médecins qui, en appliquant ce forceps, donnent aux manches de l'instrument la direction qu'ils sont habitués à donner à ceux du forceps ordinaire. Il en résulte que mon forceps se trouve mal appliqué, que ses cuillers basculent trop en arrière, que la tête est mal saisie, et que le forceps glisse.

2.—En France, dans toutes les positions obliques du sommet, nous trouvons grand avantage à appliquer le forceps sur les deux bosses pariétales, et par conséquent, la courbure pelvienne de l'instrument n'est plus dirigée d'avant en arrière par rapport au bassin, mais obliquement, comme la tête elle-même.

Quand mon forceps est ainsi appliqué, la partie convexe engendrée par la courbure périnéale des branches de préhension, forme une saillie qui vient heurter l'une des branches ischio-pubiennes et qui fait dévier le manche de l'instrument du côté opposé ; de telle sorte que les tractions faites sur le forceps ne sont pas dirigées suivant le plan médian du corps. Ainsi par exemple dans une position occipito-iliaque-gauche-antérieure, la partie convexe de mon forceps vient appuyer contre la branche ischio-pubienne gauche, et le manche de l'instrument se trouve dévié vers la cuisse droite.

Telles sont les raisons qui m'ont fait abandonner la courbure périnéale des branches de préhension, et revenir pour ces dernières à la courbure de Levret et de Smellie. La courbure périnéale est reportée sur la tige de la poignée mobile et les tractions peuvent être faites suivant l'axe du bassin comme avec mes modèles précédents.

Mais avec un forceps à courbure ordinaire voici ce qui arrive toutes les fois que l'on veut faire une application oblique : en raison même de la courbure de l'instrument, si on laisse les manches du forceps sur le plan médian du corps, les cuillers sont forcément inclinées vers l'un des côtés du bassin et la tête est mal saisie, ce qui constitue toujours un inconvénient, surtout quand il s'agit d'une position occipito-iliaque-gauche-postérieure, ou occipito-iliaque-droite-postérieure. Ainsi dans ce dernier cas, si les cuillers sont en rapport avec les deux côtés de la tête, leur extrémité est fortement dirigée vers la moitié gauche du bassin qu'elle menace, et vers le front du fœtus qu'elle déborde quelquefois. Quand on tire sur le forceps ainsi appliqué, on agit plus sur le front que sur l'occiput, par conséquent, on tend à défléchir la tête, et l'opération devient laborieuse.

Dans une application oblique quand on veut que les cuillers soient régulièrement appliquées par rapport au bassin et à la tête fœtale, il faut de toute nécessité que les manches du forceps ne restent pas dans le plan médian du corps, et soient portées vers la cuisse gauche si les cuillers sont aux deux extrémités du diamètre oblique droit de l'excavation pelvienne ; vers la cuisse droite, si les deux cuillers sont aux deux extrémités du diamètre oblique gauche.

Mais les tractions faites sur les manches du forceps ainsi déviés ont une direction très defectueuse si l'on se sert d'un forceps ordinaire, tandis que si l'on emploie mon forceps, la poignée mobile a une courbure telle que les tractions sont précisément faites dans le plan médian du corps. C'est là un point que je n'avais pas encore signalé. Cet avantage n'est cependant pas tout-à-fait satisfaisant parce

que si la traction est ramenée dans le plan médian du corps, elle n'est plus ramenée dans la direction de l'axe du bassin. J'ai cherché à obtenir ce dernier résultat, et je crois que j'y suis parvenu au moyen d'une poignée mobile que l'opérateur pourra incliner comme il le voudra.

Voici en effet une poignée mobile qui présente les modifications dont je viens de parler; mais c'est là un premier modèle que je voudrais rendre plus simple, ce qui je l'espère ne sera pas très-difficile à réaliser, si j'en juge par quelques essais en voie d'exécution.

Rotation.—J'ai dit que mon forceps laissait à la tête fœtale la liberté de tourner autour de l'axe fictif du bassin, mais quand la rotation ne ramène pas spontanément l'occiput en avant, comment faut-il faire pour produire artificiellement cette rotation?

Si l'on fait pivoter les manches du forceps autour d'un axe qui les traverserait dans toute sa longueur, ainsi qu'on le fait malheureusement trop souvent, l'opération est impossible, difficile ou dangereuse, parce que les cuillers tendent à décrire un cône dont la base répond à leur extrémité.

Pour produire correctement la rotation artificielle de la tête il faut que le manche du forceps décrive au dehors un arc de cercle afin que les cuillers puissent pivoter dans le bassin autour de l'axe fictif qui le traverse.

Le mouvement de circumduction que l'on doit imprimer aux manches du forceps dans toute rotation artificielle est employé depuis longtemps par quelques accoucheurs, mais il est trop peu connu, à tel point que je ne sais même pas s'il a été décrit, aussi je crois faire une chose utile en attirant l'attention sur lui.

Ce mouvement est bien mis en évidence par mon forceps, car avec lui, si la tête pivote pendant qu'on tire sur la poignée transversale, on voit les manches des branches de préhension décrire un arc de cercle très-évident. Il est facile d'en conclure que lorsque l'on veut produire artificiellement la rotation de la tête, il faut, pendant que l'on tire avec une main sur la poignée transversale, avec un doigt de l'autre main pousser l'extrémité du manche du forceps de manière à lui faire décrire l'arc de cercle dont je viens de parler.

L'expérience m'a d'ailleurs appris que pour réussir il importe d'abaisser préalablement la tête afin qu'elle soit fortement fléchie, et souvent ce n'est qu'au moment où elle apparaît à la vulve que la rotation artificielle devient possible.

Je vous remercie, messieurs, de la bienveillante attention que vous m'avez accordée; j'ai d'ailleurs été bien servi par les circonstances en pouvant au début de nos travaux faire une communication sur le forceps, car cet instrument, qui a rendu de si grands services à l'humanité, a été—je me fais un plaisir de le rappeler—imaginé dans ce pays, et pour mon compte je ne saurais oublier que c'est devant la Société Obstétricale de Londres qu'a eu lieu la première discussion sur mon forceps.

On the Obstetrical Forceps.

Professor F. Lazarewitsch, Kharkoff.

Science is in her very essence progressive. Freedom from arbitrary dogmas is the condition of her life.—Robert Barnes, M.D., *Obst. Trans.*, 1880, p. 121.

In the art of obstetrics there scarcely exists any action that can, for the complication of mechanical conditions—very difficult of comprehension—be compared with the operation of extracting the fœtus by the head. In order to elucidate these conditions there still remains much to study in the mechanical properties of

the fœtal head and generative canal, and the forces acting in the application of obstetrical forceps.

In the manipulation of this instrument with ease, and without danger to mother or child, skill can be attained only after a long and difficult course of comprehensive study of the mechanism of labour, after having arrived at a perfect acquaintance with the action of the forceps.

To some conclusions at which I have arrived, after a long study of the construction and action of forceps, I now beg to call the attention and criticism of other experts in midwifery.

I will point out some peculiarities respecting :—

The fœtal head.

The generative canal.

The mechanical action of forceps.

The cranial curve of forceps.

The force holding the blades of the forceps to the head.

The pelvic curve of the forceps, and the curvature of the pelvic canal in the mechanism of extracting the head.

The construction, action, and manipulation of forceps.

To this I add a description of my new straight forceps, and an artificial pelvis for practice in the manipulation of forceps.

In conclusion, I give a description of two clinical cases, and three of private practice, in which my new straight forceps were used.

The Fœtal Head.—The head of the living mature fœtus is elastic, compressible, capable of changing its form, and, very slightly, its size.

The greater transverse diameter of the head and the places on it generally grasped by the forceps are from $3\frac{1}{2}$ to $4\frac{1}{2}$ inches; these dimensions, however, may be diminished from $\frac{1}{4}$ to $\frac{3}{4}$ inches without endangering the life of the fœtus.

Dr. Barnes observes that "the degree of compressibility compatible with life is, no doubt, a variable quantity. The following conditions influence the result:—The degree of development of the head as to size and ossification, and the mode in which the compressing force is applied. If this force be applied *gradually* and *continuously*, a much greater extent of moulding, with less injury to the child, may be obtained than what Baudelocque thought possible."*

The vault of the cranium will bear compression without danger to the life of the fœtus during several hours; but, as Professor Martin, of Berlin, has observed: "the base of the cranium cannot be safely compressed for more than five minutes."

The compression of the head during labour is rendered possible by the mobility of the articulations of the bones forming the vault of the cranium with those forming its base. From this circumstance the margins are capable of overlapping each other. In this compression of the head the brain is to a considerable extent protected, partly by the removal from its cavity of the cerebro-spinal fluid, which can pass through the large occipital aperture into the vertebral canal, but principally by the rigidity of the cranial base, and its immovable articulation with the frontal bones. But by excessive compression of the head, the action may extend to the brain, and thus premature respiration and asphyxia of the fœtus may ensue. If the pressure is transmitted to the medulla oblongata the life of the child will be exposed to danger.

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* "Lectures on Obstetric Operations."

As with regard to the mechanism of labour generally, so also in regard to the ction of obstetrical forceps, the degree of development of the protuberances on the œtal head is of great importance.

In proportion to the development of these protuberances the head loses its oval, nd assumes a polygonal shape.

They serve as a point of departure to the inclined planes, and by this circumstance exercise influence on the mechanism of the head passing through the generative canal.

The wedge-like action of the head on the generative canal is facilitated by their presence.

They, however, offer the greatest resistance to the movement of the head when it enters the narrow space.

The more prominent the protuberances are, the more easily may the head be grasped by the blades of the forceps.

On its entrance into the generative canal the fœtal head is usually flexed upon the trunk, and then the posterior parts of the parietal and superior part of the occipital bones, bounded by the parietal and occipital protuberances, correspond to the pelvic canal. This polygonal space may be considered as the base of an irregular truncated cone, the top of which is the face bounded by the chin and frontal protuberances.

When the head enters the generative canal by the base of the cone the movement is at first difficult, but it afterwards continues easily as it grows narrower.

By presentation of the face with the chin directed forward, the head enters the generative canal with the top of the truncated cone, and its movement is partly facilitated by its wedge-like action.

The Parturient Canal.—If we turn our attention to the canal of the true pelvis, we shall see that the mechanism of the movement of the fœtal head is influenced by its dimensions, by the direction of its curvature, and by the eminences and intervening spaces of its walls.

The upper part of the true pelvis forms a continuous osseous ring. In its lower part, at almost equal distances, are three triangular walls, a part of the sacrum with the os coccyx, and the ossa ischii, terminating with the ischiatic tuberosities. In these walls are formed arched intervals, of which the two posterior are filled with elastic soft parts; and the anterior, adjacent to the arcus pubis, is vacuous. This aperture is bounded posteriorly by an elastic wall, completely dilatable to the limits formed by the coccyx and ischiatic tuberosities, and by this wall, force acting in a forward direction is exerted. The branches of the arcus pubis in the interior surface are convex, and this convexity facilitates the forward movement of the fœtus.

The posterior wall of the true pelvis has its origin above in an angular promontory, from which inclined planes descend posteriorly and laterally. The plane descending posteriorly terminates in the middle of the third sacral vertebra, and from this place runs another inclined plane downwards and forwards. By this change of direction in the posterior wall of the pelvic canal is determined the movement of the fœtal head forwards to the arcus pubis.

On the lateral walls of the pelvis the spinous processes form pyramidal eminences, from which run inclined planes, directing the fœtus forwards and downwards, or backwards and upwards.

On the interior surface of the symphysis pubis may be observed a greater or less development of the eminences, directing to one side or the other the fœtal head.

The influence of the eminences projecting into the pelvic canal on the mechanism of movement of the fœtus is proportionate to their development.

In order to understand more perfectly the part which the pelvic canal plays in the mechanism of labour, it is necessary to have a perfect acquaintance with the direction of its inclined planes, and with their angles of inclination. It is important to know not only the angle formed by the vertebral column with the superior part of the sacrum (140°), but also that angle which the superior part of the sacrum forms with its inferior part (150°). Neither is the angle (130°) formed by the ilium with the lateral wall of the true pelvis, nor that formed by the superior with the inferior parts of this latter (160-170°) without signification. This latter part is bounded by a line running from the spinal processus to the upper margin of the foramen ovale.

The remaining inclined planes in the pelvic canal are of less importance in the mechanism of labour.

For a more exact comprehension of the structure of the pelvic canal we may examine the inlet terminated by the linea innominata; the narrowness designated by the line running from the end of the sacrum over the spina ischii to the upper part of the foramen ovale and symphysis pubis. This line may be named linea innominata media. Lastly, the outlet bounded by a line from the tip of the coccyx over the tubera ischii and branches of the arcus pubis to the inferior margin of the symphysis pubis. Thus may be examined in the true pelvis three boundary lines: superior, medial, and inferior.

The direction in which the head moves into the pelvic canal varies from the action of the following principal points: into the inlet from the promontorium, into the narrow space from the spinous processes, and into the outlet from the tubera ischii.

Besides the rigid walls of the pelvic canal directing the movement of the fœtal head, on its movement influence is exerted by the fascia, by the ligaments and muscles of the pelvis, and particularly by the m.m. *levator ani et transversus perinei.*

Cranial Curve of the Forceps.—During traction of the head by the forceps the following powers are brought into action :—

The expelling power acting on the head from above, and moving it along the parturient canal. This force is exerted from the upper part of the uterus, from the abdominal walls, and from the extremities of the blades of the forceps.

The *directing power*, acting from the lateral walls of the parturient canal, and the middle of the blades of the forceps—by their inclined planes determining the movement of the head.

The *contrary power*, which is exerted from below—from the head and the parts of the parturient canal below it.

Of the results of these powers we can only form an approximate idea, there being many data to be taken into consideration: the head which, during labour and its extraction by the forceps, gradually and variably changes its position and form under the influence of the walls of the parturient canal, presenting a very complex system of inclined planes; the forceps itself acting variably, according to the situations of the head and to the direction of traction of the head.

The head may be made to pass through the pelvic canal: by pressure on it from above (*vis à tergo*), or by grasping it from below (*vis à fronte*).

The latter is possible by directly grasping by any means its lower section,

rned to the outlet of the pelvic canal, or by grasping it on the sides with metallic ates introduced between it and the wall of the parturient canal. When such ates are parallel, then only can we move the head into the pelvic canal, ovided their grasping power be greater than the friction of the walls of the rturient canal on the surface of the head; or if the grasping force of the plates superior to the grasping force of the walls of the parturient canal. If we increase e grasping power of the plates by forcibly approximating them, the head being essed in one direction must be increased in the other, and in this direction the asping force of the walls of the parturient canal must be augmented.

The grasping force of the parallel plates and the asping force of the parturient canal are in pro-rtion to the surfaces of contact of the head with e plates and with the walls of the parturient nal. On this account parallel plates applied to e head cannot set it in movement.

Fig. 15.

Modifications in form of head.

That movement of the head into the parturient nal may be produced by the force of traction with e blades, it is indispensable that the tips of the tter should approximate beyond the head, and ush it from above. The approximation of the ades beyond the head may be accomplished by bending their extremities. Consequently, the head can be grasped only by the curved blades. Thus, it is the cranial rve of forceps that constitutes the essential characteristic of this instrument.

When the head is situated above the elvic inlet, in order to move it along e parturient canal in the line *a b* Fig. 16), power may be applied either om above (*c d* and *c e*), or from below *f d* and *f e*).

Fig. 16.

Directions of applied force.

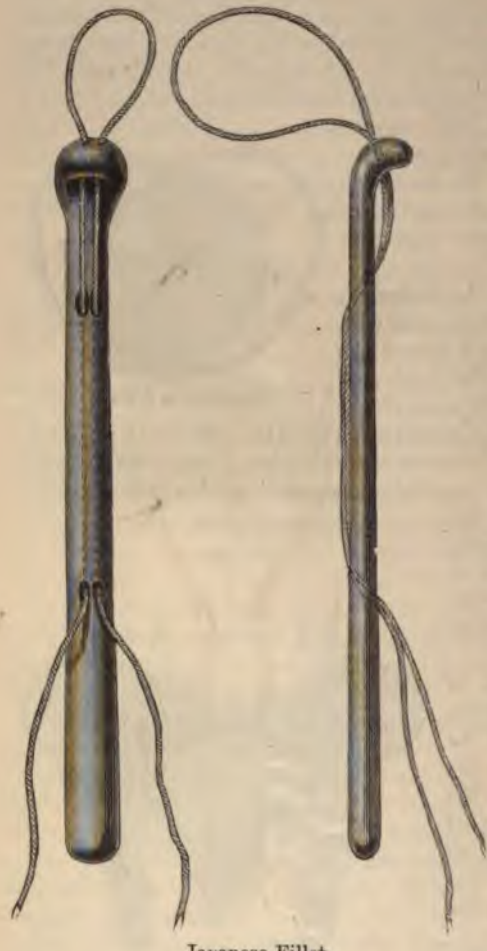
For movement from below it is ecessary that the power grasping the ead on its lower surface should be reater than the power opposing the ovement of the head through the arturient canal. The presenting part f the head offers no means for grasping . Such a means of grasping it was ttempted by the celebrated Professor impson.

The head grasped, and traction being ade from below, must be subject to ome elongation favourable to its passng through the parturient canal. Such change in the form of the head must ake place when it is being extracted by power applied to the neck, trunk, and he lower extremities, drawn by the hands of operator.

The existing and effective methods of extracting the head, without destroying t, all without exception consist in the application of power acting on the head from bove (*vis à tergo*).

For extraction of the fœtus in head presentations an instrument has been invented by Mitzu-Sada-Kirngava, a Japanese obstetrician, consisting of a polished wooden blade (Fig. 17) with holes in it, through which is passed a strong cord, so as to form a loop. To extract the fœtal head the loop is passed over the occiput or chin, and afterwards the blade is held in the right hand, and with the left is drawn, traction being made along the curvature of the parturient canal. Such an instrument Dr. Savtchenks, during his stay in Japan, procured from a native obstetrician and presented to me. In principle this instrument exactly corresponds to Westmacott's fillet. With its loop the head is grasped, and traction of it is made into the parturient canal. This method is very difficult of performance, even in exceptionally easy cases.

Fig. 17.



Japanese Fillet.

The first instrument adapted to the purpose of extracting the fœtal head was the ingeniously contrived forceps of Chamberlen. His invention of the obstetrical forceps, consisting of two halves, with fenestrated blades and cranial curve, is undisputed.

On extracting the head with the forceps, only the extremities of the blades (Fig. 18) *a b*, act as a power expelling or propelling the head from above; the parts of the blades in contact with the sides of the head, *b c*, act as rails —they give the direction to the head, keeping it between them when in movement.

Pressing the head from above, the extremities of the blades act in the same manner as the contracting uterus, or the expelling power of the hand, in the operation "expressio fœtus." But in this operation the directing power is absent, and this power only acts in contractions of the uterus from its lateral walls, and in the action of the forceps from the lateral part of its blades.

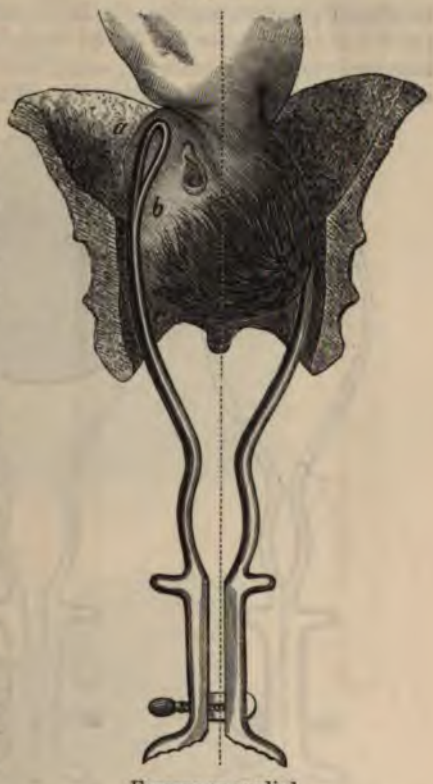
The expelling power of the curved extremities of the blades, in their action on the head, are opposed by the following powers:—by friction of the head and walls of the parturient canal, by the elasticity and contractibility of its soft parts, by the inclined planes of the pelvic canal, and by the elasticity and dimensions of the head itself.

In the more usual cranial presentation the head enters the parturient canal as the base of a cone, on the lateral inclined walls of which the extremities of the blades press from above. The greater the surface on which the blades act, or the more curved the blades are inwards, the greater will be the expelling power.

The greater or less curvature of the blades inwards may be determined by the length of the diameter of the circle corresponding to this curvature.

Dr. Aveling, comparing the various cranial curves of blades, forming arcs of circles 5, 9, and 14 inches in diameter, finds that the nine-inch head-curve may be advantageously adopted for introduction, prehension and traction.* This curve more than the others corresponds to the form of the head, and, therefore, pressure on a greater surface of it is effected.

Fig. 18.



Forceps as applied.

To ascertain the truth of Dr. Aveling's conclusion on the best head-curve, I experimented on an artificial pelvis with forceps having different head-curves. But these experiments did not lead me to a safe conclusion, because the influence of the curve varies with the breadth of the blades, with their pelvic curvature, and with the angle formed between them.

I therefore sketched a normal fœtal head, and to it I applied different arcs made of zinc wire. The result was that the nine-inch head-curve, apparently, more than any other, corresponds to the convexity of the head. The blades with those of greater diameter are less liable to injure the head, but their prehension is not so true; those of less diameter, on the contrary, have better prehensive power, but are more liable to injure the head.

The two cases at the end of this article (III. and V.) convinced me of the superiority of the eight-inch curve to the nine, because the prehensive power of the latter in some difficult cases is too slight. The more considerable the dimensions of the head the greater is the angle formed by the blades, and the less is the surface of the head grasped by the extremities of the blades.

On opening the blades, in order to preserve the same, or nearly the same angle, I have constructed a lock with the tenon entering the mortice freely. The tenon is long enough to admit of divergence of the two halves of the forceps, close to the lock, preserving their parallelism.

The Prehensive Power of the Forceps.—During traction with the forceps one power acts on the head *from above*, moving it along the parturient canal, and the other

* J. H. Aveling, "The Curves of Midwifery Forceps," *Obst. Tr.*, vol. xx. p. 137.

acts on it *from the sides*, pressing at the same time to it the blades of the forceps. This latter power is exerted from the walls of the parturient canal in a greater degree, the more considerable the resistance to the movement of the head. The power acting from the sides may yet be produced as an additional power by the hands holding the forceps. This additional power, being injurious, ought to be entirely avoided by the proper construction of this instrument. While traction is being made with the forceps, the head moves out of the pelvic canal with the blades, and while it is in the parturient canal the blades are pressed by its walls. This pressure increases as the head approaches the pelvic outlet, because the rigid and soft parts in that direction become narrower. On passing the parturient canal the head undergoes pressure, particularly in its narrower parts. If at this time it were pressed in another direction by the hands holding the forceps, this pressure must increase it to the dimensions it would have had under increased pressure by the walls of the parturient canal. Such additional pressure could but very slightly

Fig. 19. Fig. 20. Fig. 21.

Application of the Forceps.

diminish its size; but this augmented pressure and modified form of the head would be unfavourable to its movement through the parturient canal (see Fig. 15, p. 247).

Not only for the living, but also for the dead foetus, no advantage is gained by applying the additional compressing power of the hands, because in places violently compressed the head may lose that elasticity and convexity which are indispensable for preventing the forceps from slipping.

On employing the forceps the operator should exert force on the head, moving it only from above, leaving the lateral pressure to the action of the walls of the parturient canal.

The Pelvic Curve of the Forceps.—About the middle of the last century, almost

eously, in England, Benjamin Pugh and Smellie, and in France, Levret, heir attention to the fact that the pelvic canal was not straight, bent the their forceps to give them a *new curve* corresponding to the *axis of the*

pelvic curve is generally considered as the most essential improvement the forceps since the invention of the instrument. It is supposed that this pelvic curve it is impossible to apply the forceps to the head when it the pelvic inlet; that without it the perineum cannot be preserved from and finally, that without it the forceps would not have come so quickly These merits I am convinced the pelvic curve does not possess.

Farnier, admitting the importance of the pelvic curve, finds that the

Fig. 22.

Application of the Curved Forceps.

Fig. 23.

Application of the Straight Forceps.

ies of the blades should rise 87 millimètres above the horizontal line. In ion, if this curve is too pronounced, an operator of little experience, drawing l in a direction not sufficiently curved, is liable to injure with the extremi- he blades the posterior surface of the pubis. But in what does this curved

direction of traction with the forceps consist? If the point at which the pow acts be the extremities of the blades grasping the head, and the point at whi the power is applied be the handle of the forceps, the direction of the power w correspond to a straight line uniting these points, notwithstanding the curvatu of the instrument. The curved movement of the body can be produced only b an additional power. Thus, the direction of movement of the head, while tractio is being made, is modified by the inclined planes of the walls between which moves.

To economize the power applied, and to diminish the resistance, the forme should act in a direction as nearly as possible parallel to the plane on which the hea has to move. Consequently, on drawing the head it should be borne in mind tha the right line between the extremities of the blades grasping the head and the handle—as a point of application of power—should be directed parallel to the plane on which the head must move. This plane is formed chiefly by the posterior wall of the pelvis, or the concave surface of the sacrum, which is more spacious and long than the lateral and anterior walls of the pelvis, and therefore by this surface we may be more easily guided in traction of the head.

When the head is high, the forceps should be held so that the line

Fig. 24.

Lines of direction of Forceps.

a, sacrum; *b*, pubis; *c*, perineum. Direction of the forceps when the head is—
1, in the pelvis; 2, in its cavity; 3, in its outlet.

uniting the tips of the blades with the handle should coincide with the axis of the pevic inlet, or be parallel to the upper part of the sacrum. But in this case, if the blades of the forceps are with the pelvic curve, the convex edges of the blades must produce from above pressure on the inferior part of the posterior wall of the parturient canal, and on the perineum, increasing thus the resistance; while in straight forceps the resistance is diminished by their pressing only on the anterior margin of the perineum (Figs. 22, 23).

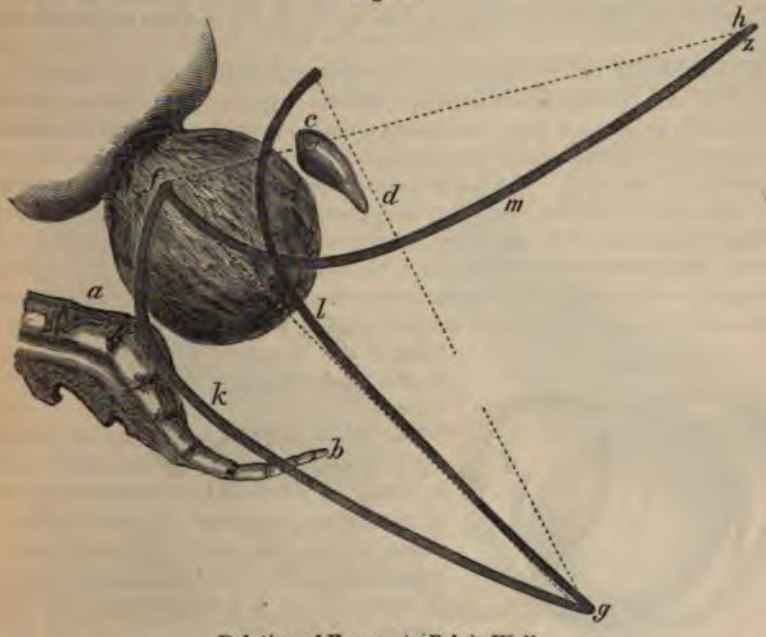
When the straight forceps is inserted in the direction of the axis of the pelvic inlet the perineum is dilated only posteriorly; but when the head reaches the outlet of the pelvis, the perineum is dilated posteriorly and laterally by the head itself. Emerging on the axis of the pelvic outlet the head moves in a direction

parallel to the dilated perineum. The annexed diagram shows the direction of the acting power coinciding with the longitudinal axis of the forceps.

On applying the forceps to the head in a high position, when the pelvic curve ought apparently to be of the greatest service, it actually renders more difficult the movement of the forceps along the axis of the pelvic inlet, because then the posterior convex margin of the blades will approach nearer to the coccyx than the same margin of straight forceps.

The annexed figure, when the head is in a high position, shows what relation the forceps has to the anterior and posterior walls of the pelvis with and without the pelvic curve.

Fig. 25.



Relation of Forceps to Pelvic Walls.

a b, the posterior wall of the pelvis; *c d*, the anterior wall of the pelvis; *e*, the head; *f g*, the axis of the pelvic inlet and the position of the straight forceps applied to the head situated in the pelvic inlet; *f m h*, situation of the forceps with the pelvic curve, whose blades coincide with the line of the axis of the pelvic inlet. The straight line, *f h*, shows the direction of the forces acting, on traction with the forceps, when they grasp the head in the point *f*, and the tracting hand holds the forceps in the point *h*; *i l g*, the position of the forceps with the pelvic curve, whose handle coincides with the line of the axis of the pelvic inlet. The straight line, *i g*, shows the direction of the force acting in this position of the forceps. The head is not grasped by the forceps in this instance. *f k g*, the situation of the forceps with the pelvic curve, when the extremities of the blades and handle coincide with the axis of the pelvic inlet. Such a position is impossible.

By the figure we may be convinced that, on employing the straight forceps, when the head is high, the perineum is not so violently pressed backwards as on employing forceps with the pelvic curve; that the straight forceps, acting in a direct line, does not in its movement press with its anterior and posterior margins on the parturient canal, as must be done with forceps having the pelvic curve; and that along the axis of the pelvic inlet the straight forceps is easily inserted,

that the curved forceps is difficult of insertion, and that the insertion of forceps with a too large pelvic curve is impossible.

Essential Conditions of good Forceps.—In order to appreciate forceps we should bear in mind the general leading ideas, based on definite positive data. Then there would no longer exist such varieties in the size of the instrument, in its curves, in the breadth of its blades, in the movability or immovability of its lock, in the construction of its other parts—as we still find in different forceps, and which cannot but be followed by injurious results.

The leading ideas concerning the construction of forceps I consider to be the following:—

In improving forceps we should pay less attention to facilitating the action of the operator than to diminishing the injury to the person subjected to the operation.

Forceps should not be large. In introducing into the confined limits of the parturient canal a large instrument it is difficult to guide it with the fingers. The considerable weight of a large instrument dulls the muscular capacity of perceiving the degree of resistance which the instrument meets with in its insertion, and in its application to the head. The less the dimensions of the forceps the more skilfully may it be guided, and the less injury may result from its incorrect employment.

The forceps should be, as it were, a continuation of the hand, rendering it possible to grasp the head confined in a limited space, and serving as feelers to transmit the impression to the sensitive nerves of the hand holding the instrument. When employing the forceps the essential guiding part should be performed by the hand of the operator. For the correct action of the hand the more simple the forceps the better.

Fig. 26.

a, linea innominata; *b*, the head; *c*, anterior margin of the right blade; *d*, posterior margin of the left blade.

The *pelvic curve* in the blades of forceps is not only useless, but it may also cause injuries, by rendering it difficult to ascertain the direction of the acting power and by not allowing the action on the head with such certainty of the correctness of movement communicated to it as is accessible in the employment of straight forceps.

When forceps with the pelvic curve are applied to the head, during traction, the transmission of the force acting is effected only by the anterior or posterior arm of the fenestrum; the opposite arm is superfluous, merely occupying space, obstructs the action of the instrument, and presses on the soft parts of the mother.

If the forceps is applied to the head situated in an oblique diameter of the pelvis, one blade generally presses the head with the anterior, and the other with its posterior margin. Hence, on the one hand, the posterior margin not being in contact with the head, it is not only useless, but has an injurious effect; on the other hand, the same action is produced by the anterior margin of the opposite blade.

This ineffectual, and even detrimental, action of the blades, will increase with their breadth, and the rigidity with which their parallelism is maintained.

To facilitate their introduction, the blades of the forceps should not be broad. The perineum is generally ruptured by its distension when the head emerges; if the head is held with broad blades, they, increasing the distension of the perineum, and distending it unequally, may co-operate in its rupture.

The convex margin at the extremities of the blades should not be so thin as to cut and wound the soft parts of the mother, or so thick as to obstruct the introduction of the blade under the thin distended margin of the os uteri, when it holds firmly the head (Figs. 27, 28).

At one inch from the tips the upper and lower margins of the blades should be somewhat thicker, that, during traction, the soft parts of the mother should not be wounded by them.

When the head is in a high position, the forceps, with its arms more or less approximating, distends the perineum less in the transverse direction, but presses more violently on it in the direction of the coccyx. To prevent rupture of the perineum, the arms, from the blades to the handle of the forceps, should be rounded and not thin. It is well if the diameter of a section of each arm be from $\frac{3}{8}$ to $\frac{1}{2}$ in. The annexed drawings show the comparative advantage of this shape.

The distended perineum is more liable to be ruptured with flat than with rounded arms, because rounded, and not thin arms, by the pressure on the perineum, distend it more equally.

Fig. 27.

Fig. 28.

a, the head ; *b*, cervix uteri ; *c*, extremity of blade.

The *lock* of the forceps should admit of the easy and sure locking of the halves, at the same time admitting variation of their position by means of their mobility in a transverse direction, and the possibility of turning each half on the longitudinal and transverse axis. This variation of the position of the blades renders it possible to accommodate them to the fœtal head and to the walls of the parturient canal.

The *handles* of the forceps are estimated according to the convenience of holding and guiding them, and in what degree superfluous and detrimental pressure of the head during traction may be dispensed with.

The handles of the forceps should be so constructed that in difficult traction of the head they may be held in different ways.

In making improvements in forceps we should not be guided by any single peculiarity of their action, or improve one of their parts at the expense of the rest. The instrument should form, as it were, an entire organism, in which, similar to organisms created by Nature, all the parts should exist for a definite purpose, and harmonize with each other. They should not render difficult the functions of each other, but, on the contrary, facilitate the action of the entire instrument and its separate parts. The forceps should be an organism, the soul of which should be in the operator's hands.

But, if led astray, we invent more and more complicated machinery, and the result may easily be an instrument which in practice is useless, and acts

not in consonance with theory. The want of harmony in the parts, and the complexity of construction, form a labyrinth of varied conditions, out of which it is difficult to emerge into the right path.

The Employment of Forceps.—Dr. Barnes truly observes, that "in the operation of extracting the head with the forceps, for conscious muscular labour, there should be a sufficient amount of muscular power, and that for its existence, we should possess an instrument, which, by its mechanism, would facilitate the action of that power."

Fig. 29.

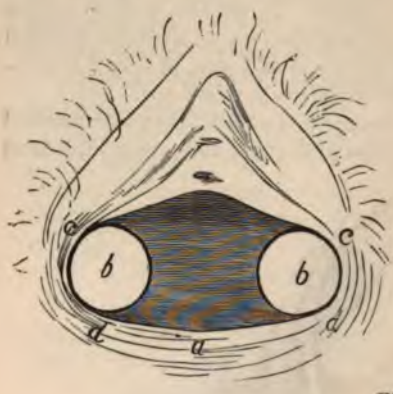
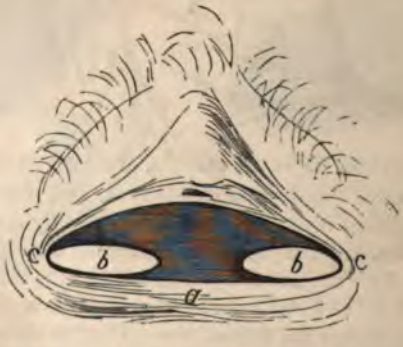


Fig. 30.

Fig. 31.



a, the distended perineum; *b*, section of the arms of the shank.

Fig. 29. Rounded branches, *b b*, press uniformly on the line *c d*.
Fig. 30. Vertical—flat branches, *b b*, press chiefly at the points *c c* and *d d*.
Fig. 31. Horizontal—flat branches, *b b*, press chiefly at the points *c c*.

Besides the amount of power, we should also, I think, for conscious muscular labour be guided by a *muscular perception*.

The amount of muscular power should correspond to the demand in a given case; this demand may be calculated more correctly in proportion to the development of this perception, which consists in a reflex transmission from the motor to the sensitive nerves, giving consciousness of the degree of muscular tension or contraction. The muscular perception of the arms should serve to indicate the direction in which the head may move, and in which traction is to be made. The direction of traction cannot be indicated by any mechanism in the construction of forceps like that of Tarnier's.

In this forceps the blades are united with movable curved branches, to which is fitted a transverse handle. These branches Tarnier calls tractile (*les tiges de traction*). They form a continuation of the blades grasping the head and, as it were, straightening the pelvic curve of the forceps, and make it insignificant.

Owing to the tractile branches, the power is transmitted in a straight line to the extremites of the blades. In this case it acts as in straight forceps. The prehensile branches (*les branches de préhension*), as indicators of the movement of the head, are insignificant, since they can indicate only the way in which the head has already moved, but they cannot indicate the way in which it is still to move onward.

The significance of the indicators of the prehensile branches may also be disputed, because the pressure on them by the walls of the parturient canal, and the direction of traction, approximate them more or less to the tractile branches.

Fig. 32.

When the traction is made, not in the true direction, and the head notwithstanding is moved forward according to the resultant of the forces applied, there will be a loss of power. Notwithstanding this disadvantageous condition of the acting force, the head may assume right positions if its indispensable turnings should not be obstructed. But to give the handle significance as an indicator of the movement of the head, it is necessary that the latter should be held firmly with the blades; being compressed between the blades in one direction it is entirely dependent on them. The result of such a condition is that the adaptation to the parturient canal of the form of the head, as well as of its position, becomes difficult.

The way in which the hand has to move, may and ought to be indicated by

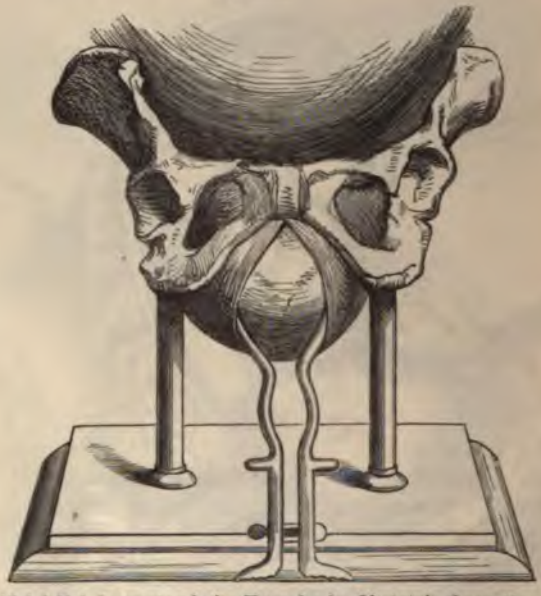
muscular perception of the arms alone. The arms ought to feel when the head is resting on the pelvic wall, when it should be moved, not perpendicularly to that wall, but parallel to the direction of the surface.

On extracting it with the forceps, the head frequently enters with its greatest diameter the narrowest part of the pelvis, and becomes fixed; then, by turning it on its longitudinal axis, it is liberated, and may easily be moved along the inclined plane.

This may happen at the pelvic outlet, when its direct diameter is contracted, and the head enters it with its larger diameter. By turning on its longitudinal axis the forceps, the head can be placed in the transverse diameter of the pelvic outlet.

It still more frequently happens, when the transverse diameter of the pelvic outlet is contracted, that the head passes into this diameter. Then it should be

Fig. 33.



Artificial Pelvis on stand, for Exercise in Obstetric Operations.

turned, so that its direct diameter may coincide with the direct diameter of the pelvic outlet.

Such versions of the head are particularly easy with the straight forceps, because it can be turned on its longitudinal axis, its relations to the pelvic walls remaining almost the same.

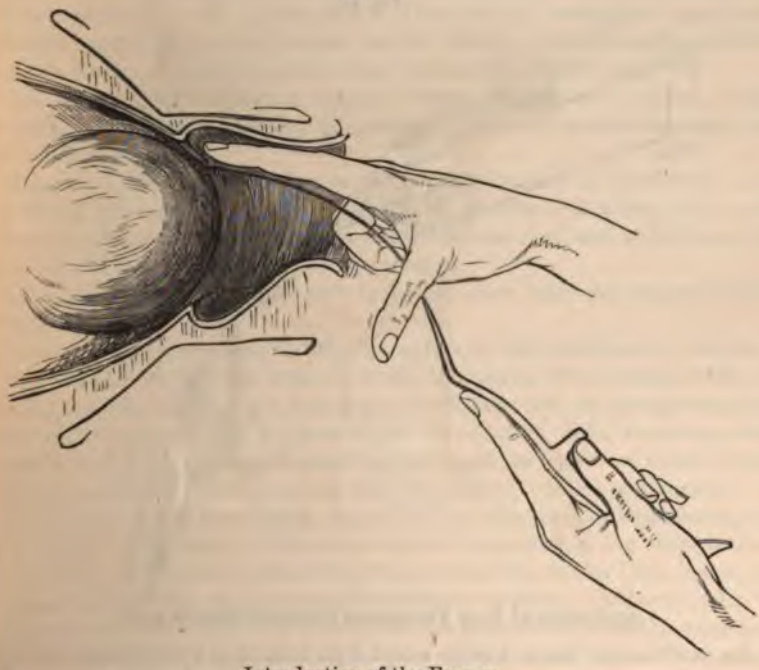
On the employment of forceps, all conditions obstructing and facilitating the action of the power should be borne in mind.

During traction with the forceps, the trunk of the child may be expelled or retained by the uterus. In a favourable position of the uterus, and the successful action of its contractions, the trunk of the child, while being expelled, facilitates the action of the forceps. If the uterus does not contract, if it holds the fœtal trunk firmly, and if it does not correspond with the axis of the pelvic inlet—under these conditions it will act on the fœtal trunk, and consequently on its head, obstructively.

In one clinical case (Fig. 32), by the cyphosis of the lumbar vertebræ, the abdo-inal cavity was shortened, not allowing sufficient space for the pregnant uterus; n this account it protruded excessively forwards and to the right. In consequence f this, the expelling power of the uterine contractions did not act in the direction f the axis of the pelvic inlet on the fœtal trunk, as it formed with the head almost right angle. The head being of small dimensions was retained, not by the pelvic alls, but the fœtal trunk, which did not emerge from the uterine cavity. In such case the extraction of the head with the forceps must be very difficult, whilst by ectifying the uterus, and by exciting it to contract, pressing on its fundus, our fforts may quickly be crowned with success.

Exercise in the Operation of Extracting the Head with the Forceps.—The extraction of the head with the forceps is an operation which may be performed with greater or less skill in the art, which may be attained in various degrees of perfection.

Fig. 34.



Introduction of the Forceps.

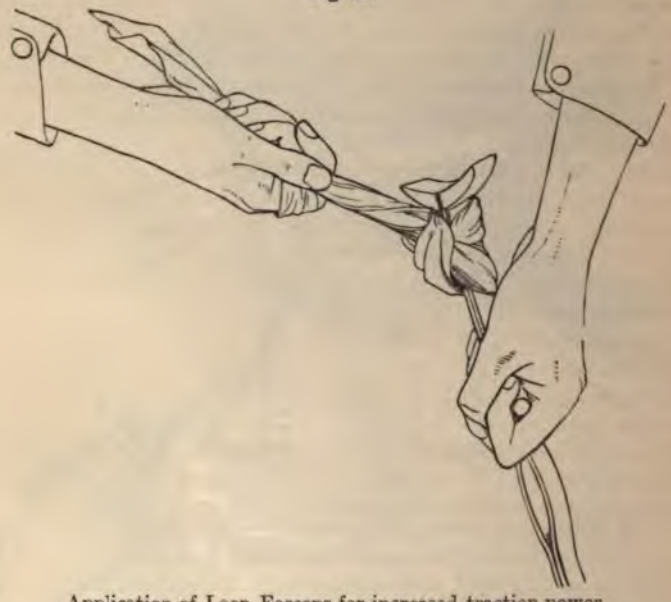
For exercise in obstetrical operations I employ an artificial female pelvis, nade of zinc, and placed on a stand (Fig. 33). Into the pelvis I put a piece of rash-leather, folded so as to form under the pubic arch an opening. At lectures uch an artificial pelvis is convenient, as the student can see the action of the orceps, and the changes which take place in the position of the head.

When occupied with students, I hold the fœtal head with the hand; the tudent introducing the blades has to take the margins of my hand, grasping the ead for the margins of the uterine orifice. Thus, it is possible, not only to verify rith my own hand the action of the student, but also to deduce useful conclusions, egarding the action of the forceps. In this manner I have observed that, to avoid njury to the margins of the uterine orifice and vaginal vault, the blade should be

introduced so that its extremity should glide, without deviation, along the palm of the hand in the parturient canal, until it reaches the uterine orifice (Fig. 34); it should afterwards glide along the head, being kept close to it. On the application of forceps by a student, not only can I perceive with the hand holding the head every movement of the extremities of the blades, but I can also feel with it very distinctly every violent action, as the blades produce the painful sensation of pressure or friction with their margins.

New Parallel Straight Forceps.—Since 1866, when, at the Conversazione of the Obstetrical Society of London, I first determined to exhibit my forceps, I have never given up the idea of improving this instrument. I have changed its form several times, preserving its essential qualities—not crossing of the halves, and easy locking. To test the above properties, which I find to be indispensable in good forceps, I have constructed a new forceps, which, besides having these chief characteristics of my former instrument, is entirely without the pelvic curve.

Fig. 35.



Application of Loop Forceps for increased traction power.

My new forceps has a handle adapted for holding it conveniently and firmly. In case of necessity it may be lengthened at will, by throwing a piece of tape below or above the lock. If the loop is thrown below the lock (Fig. 35), the fœtal head is not pressed in the least by the forceps, however violent the traction may be. On throwing the loop above the lock the blades during traction approxima te.

The lock consists of a tenon, $1\frac{1}{2}$ inches in length, and $\frac{3}{4}$ in. in breadth, rounded at the extremity, and of a mortice allowing free entrance to the tenon.

By means of a screw passing through a nut in the right half of the handle, and pressing against the other half, the blades may be diverged, and kept either parallel or at an angle.

With the rounded form of all the parts, and with the homogeneity of the material of my new forceps, it is easy to keep the instrument quite free from all impurities.

Forceps are frequently sold bearing the name of one and the same inventor, yet differing in the construction of their several parts, and sometimes, even not according to the idea of the inventor. Such deviations from the regular construction of forceps depend, either on the wish of the maker to improve them—but these improvements generally turn out to be the reverse—or these deviations are still more frequently the result of his not having an exact representation of the instrument.

I have frequently found exposed for sale, or employed by practitioners, my own instruments with various irregularities, the consequence, perhaps, of their not having been represented on a sufficiently large scale, and without diagrams to explain the details. Such representations of the instruments may have been sufficient to show their chief characteristics, but not for making them perfectly correct, or for verifying them when made.

My new parallel straight forceps I have several times employed when the head was low; and here I will cite five cases in which it was employed when the head was situated in the pelvic inlet. These cases, presenting considerable difficulties, prove that straight forceps may be employed with facility, and with favourable results, even in the most difficult extractions of the head.

Case I.—Eudoxia Mashina, a peasant woman, of forty years of age; in her fifteenth year menstruation began; at nineteen she married, and subsequently gave birth to ten children.

In January, 1880, menstruation ceased, and at the end of April of the same year she felt the movements of the fœtus. On the 31st of October, 1880, in the lying-in hospital, at one P.M., the labour pains commenced, and at half-past nine the next morning the membranes were punctured.

Afterwards the head descended into the pelvic inlet, with the occiput directed forwards and to the right.

I saw the patient five hours after the rupture of the membranes. She was of strong constitution; the pelvis was normal; the uterus, of an oblong shape, and somewhat declining to the right, was almost uniformly firm, in consequence of the absence of the liquor amnii. On the left of the abdomen fœtal movements were perceptible, and below the umbilicus the pulsations of the fœtal heart were audible. The position of the fœtus was normal, with the occiput directed to the right.

The external generative organs, especially the labia majora and minora, were in a very œdematous state; the anterior wall of the vagina, of the shape and size of a hen's egg, protruded from the vulva; the os uteri was dilated three inches, and its anterior margin was in a considerably œdematous state. On examination the left parietal bone was found to be pressed by the promontorium, and consequently situated below the level of the margin of the right parietal bone. The occipital bone was slightly depressed; the right parietal protuberance was covered with an œdematous tumour. Notwithstanding the violent labour pains, the head, after the rupture of the membranes, remained in almost one and the same position for five hours. Rectifying the uterus, and pressure on its fundus, had no effect on the course of the labour.

It was necessary to have recourse to the forceps. I introduced, first the left, and then the right, blade of my straight forceps. The locking was effected without difficulty, the halves of the handle being half an inch apart. I first made traction along the axis of the pelvic inlet, slightly pressing the arm of the forceps on the perineum. Four strong tractions were necessary to bring the head into the pelvic canal, and then the forceps began to take the direction of the axis of the pelvic

outlet. In this new direction, after three moderate tractions, four more strong tractions were necessary to liberate the head from the pelvic cavity.

Round the neck of the fœtus the umbilical cord was found to be so tightly coiled that it was necessary to cut it before it was possible to extract the trunk.

A male child was born, in a state of asphyxia, from which it quickly recovered. Its weight was 4,650 grammes, and its length 20 inches. The biparietal diameter of the head was four inches, and the occipito-mental 5½ inches, with the corresponding circumference of the head, 17¾ inches. Over the right parietal protuberance was a tolerably large caput succedaneum, giving the head an asymmetric appearance. On the left of the head, close to the eye, was a reddish spot, having an area of a square half-inch, and in front of the ear a similar spot, but somewhat larger. On the right of the head were such spots; one over the eye one inch square, and another over the ear half the size—both oblong. These spots, caused by the blades of the forceps, showed that the head had been grasped at the sides regularly.

The weight of the placenta was 1,050 grammes. The puerperal period was quite normal, and on the ninth day after labour the mother left the hospital with a healthy child.

In this case the forceps was employed under the following unfavourable conditions:—The œdematous state of the anterior margin of the os uteri and of the external generative organs, the high position of the child's head, the large dimensions of the child, and the entire absence of the liquor amnii.

This last condition greatly increased the friction of the child's body, as the walls of the uterus grasped it firmly. However, notwithstanding the difficulty of the operation, the child's head was extracted with perfect success.

The asphyxia of the child was caused by the trunk being delayed after cutting the umbilical cord.

Case II.—Proscoira Kravtzow, a soldier's wife, of twenty-five years of age, middle stature, strong constitution, with rather small conjugate (four inches). At eighteen menstruation commenced, at twenty she was married and had one child.

At the end of her second pregnancy she was admitted into the Lying-in Hospital of the University of Kharkoff, on the 31st of October, 1880. Her last menstruation was on the 16th of February, 1880, and on the 18th of June of the same year she felt the child's movements.

On the 5th of November, at six A.M., labour commenced. At half-past four P.M. the membranes were punctured, and about a pound of liquor was discharged. An hour afterwards two pounds more were discharged, and the head became fixed in the pelvic inlet, having the occiput to the right and the spine forward, and a long coil of very thick umbilical cord prolapsed behind and on the right. An attempt to rectify it was unsuccessful, on account of its thickness and the narrow passage through which it had prolapsed.

After more than half an hour after the prolapse of the funis I found the uterus of an oblong form, somewhat declining to the right. On palpation it was found to be equally firm, on account of the absence of the liquor amnii and the forward direction of the fœtal spine. Below the umbilicus feeble, irregular pulsations of the fœtal heart—about 80 per minute—were audible.

On internal examination I found the head in the pelvic inlet, with the occipital bone below the level of the parietal bones, and with the posterior parietal bone below the margin of the anterior. The sutures and fontanelles were clearly defined. The occiput was directed to the left and forward. The left parietal

protuberance was behind the symphysis pubis. Behind and below the head, close to the right incisura ischiadica, was a thick, feebly-pulsating umbilical cord, forming an elongated coil bent forwards.

The fixation of the head, and the protracted ineffectual action of the pains, showed the necessity of having recourse to the forceps. With the head in the left oblique diameter, and the left parietal protuberance lying much lower than the right, it was impossible to grasp the head with the forceps so that its blades could be applied symmetrically. The introduction of the left blade was difficult, the side of the head being firmly pressed against the linea innominata, almost without interval, from the promontory to the symphysis pubis. The left blade was introduced with difficulty, while the right passed quite easily, in front of the prolapsed funis, along the convex surface of the head. The locking of the forceps was perfectly easy.

Traction was made along the axis of the pelvic inlet, then, by degrees, in the direction of the outlet. For complete liberation of the head twelve violent tractions were necessary.

The operation, under chloroform, lasted twenty minutes. The child, as was to be expected, was in such a state of asphyxia, with scarcely audible pulsation of the heart, that all efforts to revive it were unsuccessful.

The placenta was expelled with the aid of pressure on the fundus of the uterus twelve minutes after the birth of the child. The placenta weighed 698 grammes. The child, a boy, weighed 3,120 grammes, and was 19 in. in length.

On the left side of the head, above the eye, was a bluish-red spot, produced by the pressure of the blade; on the right side was a scarcely perceptible reddish spot, a little above and in front of the ear. The puerperal period passed quite satisfactorily.

In this case the blades of the forceps were not applied to the sides of the head symmetrically, the right blade being about 1 inch from the eye, and the left close to the eye. The left blade pressed more violently, and the tip left a distinct mark. The unequal action of the blades, on account of the inclined position of the head, was not attended by any difficulty, nor had it any unfavourable consequences.

Case III.—Mrs. P., the wife of an engineer, of fair complexion and weak constitution, was married at nineteen years of age, and was soon pregnant. The first pregnancy lasted but seven months, and, without apparent cause, terminated in abortion.

Twice after this she menstruated, and then pregnancy followed. On the 20th of June, 1880, menstruation ceased, and on the 9th of November of the same year she perceived the movements of the fœtus. Three months afterwards the lower extremities became œdematous; pain, with heaviness of the head, at first only occasional, but afterwards daily and prolonged attacks, began to disturb her more and more; her face became puffy, and covered with reddish spots.

On the 23rd of February attacks of asthma with spasms in the larynx occurred, and the next day, losing consciousness, convulsions in the face and extremities. On that day, at one o'clock, I saw Mrs. P.; after several attacks of eclampsia, in a half-conscious state. She lay with her eyes half closed, with a very swollen and purple face, her pulse about 80 per minute; the abdomen was large, as at the expiration of the eighth month of pregnancy; the uterus was symmetrical, oval, of equal firmness; on auscultation no sound was audible; over the os uteri, which was dilated half an inch and lay backwards, was the head, covered with the membranes. On pressing the uterus slight labour pains began.

Applying cold to the head, chloroform and natrum chromatum, had no effect on the eclampsia. Artificial respiration, induced by Dr. Podrez pressing the base of the thorax, was effectual in removing the attacks and renewing respiration.

At three o'clock P.M. the os uteri was dilated 1½ in. The bones of the presenting head were yielding, the sagittal suture was in the transverse diameter of the pelvis, with the occipital fontanelle directed to the left, and the frontal to the right.

To accelerate labour I tried version of the fœtus, but without success. The fixity of the head did not admit of the introduction of Dr. Barnes's dilator-bag for the employment of which there was sufficient indication.

I ruptured the membranes and an insignificant quantity of liquor amnii escaped. The fits of eclampsia recurred more frequently, and with greater force. During short intervals a deep comatose state prevailed.

The os uteri was dilatable with the fingers to 2¼ in. On the posterior margin I made an incision half an inch deep, and afterwards applied to the head in a high position my straight forceps.

After introducing the blades the fits of eclampsia ceased and the patient's consciousness returned. During the traction the labour pains became manifest.

By the action of a few rather violent tractions with the forceps the head entered the cavity of the pelvis and descended to the vulva. When it became necessary to extract it, the head was found to be so soft and yielding that there was no hold for the forceps. Its delivery was at length effected, when, during traction, an assistant pressed the fundus of the uterus.

he right hand was found to be behind the neck, and the cause of obstruction to delivery of the trunk.

The placenta could not be expelled by the method of Credé, therefore I grasped it with the fingers, introduced into the cavity of the uterus, and extracted it.

To a slight rupture of the perineum I applied a silver suture. The patient fell into a quiet dose—with slow pulse (60 per minute)—which lasted about two hours. Afterwards recurred four slight fits of eclampsia.

The third, fourth, and fifth days after delivery there was an increase of temperature (38° C.) corresponding to the swelling of the breasts. The urine for a few days after delivery contained a gradually diminishing quantity of albumen, and headache occurred, which passed off on the administration of caff. citr. The patient recollected nothing that had taken place, and only on the third day after delivery was aware of having given birth to a still-born girl. The twelfth day after delivery the patient began to walk, and her health was soon fully restored.

In this case the forceps was employed under such unfavourable conditions, that each one of them might have served as a contra-indication to its use had it not been indispensable for the acceleration of delivery, in order to save the mother's life. Several cases mentioned in my "Manual of Obstetrics" have convinced me that in violent eclampsia, accelerating delivery acts beneficially; the irritation produced in the sphere of the sexual apparatus diminishes pressure in the cerebral vessels, and thereby dulls the reflex. On the escape of the liquor amnii, and when the fœtus has passed into the parturient canal, the mechanical action of pressure of the uterus on the vessels is diminished.

As a contra-indication to the use of the forceps may serve—the narrowness of the os uteri, the high position of the head, and total absence of its elasticity, on account of the maceration of the dead fœtus. Notwithstanding all these unfavourable conditions, however, the head was grasped with the forceps, and drawn down

to the vulva. The final delivery was obstructed by the arm being coiled round the neck of the fœtus.

Case IV.—Mrs. A., of fair complexion, middle stature, good constitution, aged twenty-seven, and two years married. At the close of pregnancy the waters began to pass, and only on the third day afterwards, 22nd of April, 1881, the labour pains commenced. The next day, at one o'clock A.M., I was invited to attend her, and on exploration I found her considerably exhausted, after prolonged labour pains. The uterus was of an oblong shape; the os uteri, with thin margins, was dilated three inches. The head was almost immovable in the pelvic inlet, with the occiput to the left and forwards, with a rather large œdematous tumour on it. After waiting an hour, notwithstanding the violent pains, no change took place in the position of the head and in the os uteri. On auscultation the pulsation of the fœtal heart and the fœtal movements were not perceptible.

My straight forceps was introduced without difficulty, and after eight rather violent tractions the fœtus was delivered. It was a girl, in a state of profound asphyxia, out of which it was impossible to revive her.

After the fœtal head had been drawn into the pelvic cavity, leaving my hold of the handle of the forceps, I began to observe at the commencement of each pain its circular movements from left to right. This served me as an indication that in the given case the forward movement of the head was obstructed by the funis being tightly coiled round the neck. After extracting the head my supposition was confirmed.

Notwithstanding the insufficiently dilated os uteri, and the fact that one blade lay on the left side, close to the occiput, and the other on the right, close to the eye—consequently unfavourable for grasping—the head was held with the forceps firmly, and extracted without great difficulty. On the forehead, close to the right eye, was a small red streak, produced by the margin of the blade.

The perineum was not ruptured, owing to two small lateral incisions having been made in it. The puerperium was favourable.

Case V.—Mrs. K., of middle stature, good constitution, thirty-six years of age, six years married, primipara.

The labour pains commenced on June 13, 1881, at 4 o'clock, A.M., and at 9 P.M. I found as follows:—The position of the child was with the occiput to the right, the os uteri was dilated two inches, and the uterus was inclined to the right. On this depended the low position of the great fontanelle on the left of the pelvis.

After rectifying the uterus and rupturing the membranes the labour-pains grew more violent, but the following day they began to abate. Twenty-four hours after my first visit I found the os uteri dilated 3½ in., the head fixed in the pelvic inlet, remaining with the great fontanelle directed to the left. There was no tumour on the head.

I applied my straight forceps with the nine-inch cranial curve, and after a few tractions brought the head to the vulva. But here was repeated what occurred in the third case: prolonged efforts to extract the head were unsuccessful, because with the violent tractions the blades of the forceps began to slip.

When, however, I applied my forceps with the eight-inch cranial curve, the blades held the head firmly, and the head was extracted with perfect ease.

The funis was coiled once round the neck.

The live male child weighed about 4,000 grammes.

The placenta was removed by pression. The perineum was uninjured.

The successful action of my straight forceps, proved in three more cases in my private practice, when the head was situated in the cavity of the true

pelvis, with the five above-described, in which the head was in a high position, may serve to show its merits, and to prove that the cranial curve is of the greatest importance for grasping the head. Cases III. and V. showed clearly that the cranial eight-inch curve is quite satisfactory, whilst forceps with the nine-inch cranial curve does not grasp the head firmly, and in difficult extractions is liable to slip. These cases prove that in various difficult extractions of the head there is no necessity for pressure with the forceps, and that the pelvic curve is perfectly useless in this instrument.

DISCUSSION.

Dr. Fordyce Barker, New York: A few months after the forceps of Tarnier were made known to the public I had the pleasure of exhibiting them at a meeting of the New York Academy of Medicine, and demonstrating their use on a mannikin. As the opinions then expressed were published in the *American Journal of Obstetrics*, which I have great pleasure in believing is generally read by prominent obstetricians of all countries, and as the views I then expressed as to the value of this new addition to our obstetrical armament has in no respect been modified, but my opinions have been confirmed, I will not detain the Section by repeating them. These forceps are really the first successful result of an attempt in which many before have failed, to assist the efforts of Nature by traction with an instrument, with safety to the soft parts, in the direction of the axis of the superior strait. In this point of view I think that all competent men who have thoroughly studied the principles of the instrument, and have practically learned how to use it, will agree that it is the most important modification of the forceps that has been devised within the past quarter of a century. By its use the head can be brought down from above the superior strait, under circumstances where this result could not be effected by any instrument which had before been suggested, without injury to the soft parts, into the cavity of the pelvis, and frequently to the lower strait. It is this feature which constitutes the great and original merit of the Tarnier. As regards its principle, and the practical application of the force of traction in the upper strait, it seems to me perfect. The difficulty in its use consists chiefly in the danger to the soft parts of the lower strait. Without great care it will cut through these tissues like a knife-blade. I therefore think it a safe procedure to recommend that, when the head has been brought down to the floor of the perineum, the Tarnier forceps should be taken off, and a Simpson's forceps, or one of this kind, should be applied. In saying this I hope that I shall not be regarded as in any way detracting from the merit of the Tarnier forceps. I have the pleasure of exhibiting the forceps of Tarnier, modified somewhat by my friend and successor in Bellevue Hospital, Professor William T. Lusk, of New York. The instrument is somewhat lighter than those made in Paris, its blades are more rounded off in their edges, with a slight change in their curve; the handle can be more readily adjusted, and a key arrangement for locking has been substituted for the socket-joint. On the part of Professor Lusk I have the honour of presenting the instrument thus modified in its construction to M. Tarnier.

Professor A. Russell Simpson, Edinburgh, deemed it a happy circumstance that the discussion on the construction of the forceps had been opened by two distinguished foreigners of opposite views on this important question. For his part he ranged himself on the side of those who thought that a pelvic curve was an essential feature in a properly constructed forceps, and since Professor Tarnier had shown

how traction could be made in the correct axis, he had come to believe that the days of the straight forceps were numbered. He accepted the four principles which M. Tarnier had laid down in his address to-day, though he thought there was still room for discussion as to the manner in which these principles could be best carried out in the fabrication of the instrument. He need not repeat the advantages which had been urged by M. Tarnier; but in opposition to the plea of Professor Lazarewitch for the use of a straight forceps he must insist that for seizure of the head at the brim a curved instrument was absolutely necessary, and that the same instrument was the best for application in any part of the passages. Although men who had got long accustomed to the use of a particular instrument might continue to use it till the end of their days, the coming generation would appreciate and employ the instrument constructed on the most scientific principle. The more his experience with the use of axis-traction forceps grew, the more impressed he became with the value of the guidance afforded as to the proper line of traction by the unimpeded movements of the application handles. This guidance is so correct that in following it the student or young practitioner will be less likely to do damage to mother or child than a more experienced practitioner working with a less perfectly constructed instrument. Professor Lazarewitch had said that the practitioner must be guided by his muscular sense as to the direction in which he is to make the head advance. Well, it was one of the advantages of the axis-traction forceps, on which he (Professor Simpson) thought it the more necessary to insist as Professor Tarnier had not himself referred to it—viz., that it enabled the practitioner to appreciate the progress of the head and the condition of the passages with a much higher degree of sensitiveness than he could enjoy under the older forms of instrument. However easy or however difficult the case, some of the muscular energy employed in extracting with the old forceps must always be expended in compressing the handles; by so much there must be a loss of the power of sensation, and, consequently, of the power of regulating the advance of the head. With axis-traction forceps less muscular energy was required, because there was no loss of effort; and as all the effort was made in the way of simple traction, the sensory nerves were free to appreciate, with a degree of acuteness not to be attained in the use of the old instruments, the impressions that the head was making on the maternal canals. Professor Simpson concluded by expressing the gratification he had experienced in witnessing the efforts made by some obstetricians —as by Lusk in America, and Sanger in Germany—to apply the principles of the Tarnier forceps to the forceps in use in their own countries; and by showing the forceps which he had himself constructed on this principle. They were the widely used forceps of Sir James Simpson with the traction hooks removed, and with traction rods permanently jointed to each blade; somewhat simpler in construction than those of Professor Tarnier. He had employed, or seen them employed, in all sorts of cases at the brim, in the cavity, and on the perineum, with the head in all the various positions, and always with results entirely satisfactory.

Dr. Budin, Paris: I take part in this discussion because I was a pupil of M. Tarnier, and I have had opportunities to use his forceps, and have taught many students and physicians its use. I will not revert to the objections to this instrument which have been made by M. Lazarewitch, for Professor Simpson has completely answered them. M. Lazarewitch maintains that if the needle indicates the direction in which traction is made, it is, nevertheless, useless, for it does not indicate the direction in which traction becomes necessary. In practice his opinion is scarcely

tenable. The needle, M. Lazarewitch admits, indicates exactly the direction in which traction should be made, when the head is fixed. It is evident, then, that the needle will also point out the direction in which traction should be made when the head has descended one or two millimètres. M. Tarnier, moreover, brings forward an experience which proves the value of the needle. I take his forceps, whose handles have no perineal curve, and which, therefore, have the direction of the handles of the forceps of Levret. I place the blades in the hands of a person who endeavours to prevent their withdrawal. He succeeds if the operator pulls on the handles. On the other hand, he fails if the operator pulls on the traction rods. The explanation of what happens is simple: the person who holds the blades of the forceps between his hands can change at will the direction of the blades, and in this way break the force exerted by the operator without the latter knowing it; if, on the contrary, the operator seizes the traction rods, the situation of the blades, which varies, shows him without intermission in what direction he should pull in order to draw in the axis and lose no force whatever. Some have objected to the instrument because of its too great size and complexity. A blow with a light switch is, doubtless, less hurtful than one with a massive stick. In like manner, traction made with a voluminous forceps, badly directed, may be more injurious than with a shorter and smaller instrument. The important thing is to know how to direct traction; now this is easy with the assistance of the indicating needle. This remark, which is applicable to the ordinary forceps, loses much of its value when applied to Tarnier's forceps, the traction rods of which are fixed near the centre of the head. The arms of the lever being very short, traction badly directed will be less injurious. It is said, moreover, that the instrument is more complicated; this is true; but if this complexity is a slight inconvenience, it has, nevertheless, great advantages. The complexity is real. When the instrument is applied, it is necessary to bring together the handles, to put on pressure, to lower the traction rods, to unite them to the iron rod which joins the transverse handle, by means of which traction is to be made. But when this instrument has been in this manner fixed on the head, the making of traction is considerably simplified: it is enough to pull slowly and leisurely, one can pull even during the uterine contractions only. One can dilate the soft parts gradually and without danger to them bring about the exit of the head at the most favourable moment, without having to think if one presses the head too much or too little. There is, finally, a last objection that has been made to Tarnier's forceps. It presents a power, by the aid of which we fix the prehensile branches on the head; now this power has been considered as acting after the manner of the force of the cephalotribe. Struck with this objection, M. Tarnier used this force less during the first applications of his forceps, but little by little he has grown bold, and has never seen inconvenience result from it. He has done more; he has allowed the head to remain after extraction compressed, for ten or twelve minutes, between the blades of the forceps without any bad result. The remarkable experiences of Ducet have demonstrated that, although rapid compression of the brain is dangerous, slow and continuous compression is much less so. An injection of twenty to thirty grammes, made suddenly into the cavity of the cranium, causes mischief, while a much larger quantity, injected slowly, is harmless. The slow and continued compression made with Tarnier's forceps is certainly much less dangerous than a sudden compression which is made with another forceps when one meets with resistance.

Dr. Matthews Duncan, London: A scientifically constructed instrument is not

necessarily better than a scientifically imperfect instrument which has been gradually improved by generations of practitioners in all parts of the world. Tarnier's instrument is constructed according to theory, but does not satisfy all points theoretically demanded. It is constructed nearly exclusively with a view to furnishing deliveries of naturally placed heads in natural pelves, and much is required for its complete scientific adaptation to difficult and unnatural pelves. A great objection is the persistent or constant compression of the head, instead of, as in ordinary forceps, compression less or more, according to the extracting force applied. It is only time and experience, by many practitioners in many places, that will decide whether the comparatively complicated instrument of Tarnier will displace the old long forceps. I hope that Tarnier's forceps will get a full and thorough trial, and it will be necessary that much should be proved against ordinary forceps to displace an instrument which I have always found sufficient to do what I reasonably expected.

Professor STEPHENSON, Aberdeen: No one can admire more than I the amount of thought and ingenuity which Professor Tarnier had expended on his forceps, yet I cannot accept his modifications as improvements. The ordinary forceps have their defects, but in the forceps of Professor Tarnier many of these are increased instead of diminished. The former are bulky enough, the latter are larger still. The former often hinder rather than aid flexion and rotation, the latter are yet more likely to do so. The special principle for which Professor Tarnier contends, —viz., drawing exactly in the pelvic axis, is an erroneous one in cases where the pelvic brim is narrowed. Nature passes the strait by certain rotation movements of the head; Professor Tarnier argues for direct traction, accurately made, in a given straight line, with the head screwed tightly between the blades. The difficulties met with should be overcome by manipulating the head in imitation of Nature, not by sheer hard pull alone. The muscular sense or *tactus* of the operator is the proper guide to what is taking place; the tight compression by the hands, which Professor Simpson finds destroys the sense, is unnecessary, and often injurious. Improvements in the forceps are required, but in the direction of facilitating delicate movements, not the strong pull of a powerful arm.

Dr. BARNES, London, declared himself in favour of M. Tarnier's forceps, having studied its action thoroughly. He thought there was little to say in answer to the objections that had been made against it, after the statement of M. Tarnier, and the admirable reply of Dr. Budin. He could not assent to his friend Professor Lazarewitch's argument in favour of the single-curved forceps. The pelvic curve he considered had been definitely gained. With regard to the statement of Professor Simpson that the muscular sense was lost by grasping the handles of the forceps, he observed that the forceps, if of proper length, was a two-handed instrument, and thus the two hands, acting together or alternately, economized force, and preserved the muscular sense unimpaired, if necessary, for hours. Dr. Duncan's objection to Tarnier's forceps, that it had been worked out in natural cases, was really out of place, since it was pre-supposed that all cases in which the forceps was used were fit for the forceps. It was not employed in cases of marked contraction of pelvis. So, granted a pelvis fit for the forceps, Tarnier's was applicable like any other. Professor Stephenson's dictum that there had been no imprrovment in the forceps was surprising. The idea that the forceps should be employed to rotate the head, seemed to show he had not yet grasped the true idea of the mechanism of

labour. To rotate the head artificially could in most cases only disorder the natural relation of head to pelvis, and create difficulty. Simple traction, to give progressive movement, could not fail to be followed by the due rotation, just as it occurred under the natural expulsive powers.

Dr. ATTHILL, Dublin, Master of the Rotunda Hospital: As one who has given a full and certainly a fair trial to Tarnier's forceps, I wish to state my experience of their use. The two chief advantages stated to be in favour of these forceps are that they favour the rotation of the head, and admit of traction being made more easily in the pelvic curve. As to the latter, I think that there is in it a good deal of force; but as to the former, I do not find that rotation occurs more frequently than with the forceps in common use, while there are great disadvantages in other respects. 1st. The constant compression of the head by means of the screw by which the blades were fixed after application, is very objectionable, and in more than one case I believe that fœtal life was in consequence lost. 2nd. The instrument on several occasions slipped, this being probably due to imperfections in the cranial curve. 3rd. The complexity of the instrument is a grave objection.

Professor TARNIER, Paris: In the midst of the opinions which have been ventilated, it may be concluded that the ordinary forceps is sufficiently powerful, and that it suffices to complete the majority of deliveries in which it requires to be applied. This is certainly true; but the objection I urge against the common form of forceps is that it is too powerful, especially as a lever, and that it demands the employment of too much force, part of which is expended against the pelvic walls, which then become exposed to a dangerous degree of pressure. My forceps is characterized by the quality that no part of the power is expended in compression; that it can be utilized, and that, in consequence, the traction force need not be so great as with the ordinary kinds. It is, in a word, less violent in its application. Dr. Mathews Duncan says my forceps have proved efficient in simple cases, but that we must await evidence of its power to deal with difficult ones, especially those associated with narrowing of the pelvis. My reply is that since 1877 I have employed no other kind; and that, in a large hospital like the Paris Maternité, I have used it over and over again where the pelvis was deformed, and always with excellent results. My pupils, as well as myself, are convinced that we have obtained results at least equal to those following the use of the ordinary forceps, and that generally we have succeeded better. Dr. Barnes lends to this opinion all the weight of his large experience. It results from this discussion that the principles I have formulated as regulating the construction of the forceps remain intact; no one has even attacked them. As regards the method of application, differences may exist, as may be seen in the forceps of Professor Simpson and Dr. Lusk, shown to you by Dr. Fordyce Barker. If my forceps be too bulky for some persons nothing is easier than to make it smaller and its blades thinner, but I should not myself consider this any advantage. In conclusion, the only objection urged against my forceps is that it is too complicated—an important one, if the results attained be superior. Possibly it may be simplified; but we have already found in it an instrument really "scientific," if I may quote a happy expression I have just heard. I am much pleased, gentlemen, with the favorable reception you have given to my forceps, as well as with the cordiality and kindness you have shown towards myself.

PROFESSOR LAZAREWITCH, Kharkoff: It is only after a long experience and careful study that I have arrived at the conviction that the pelvic curve of the forceps is not rational. The true merit of M. Tarnier is that he has applied the traction rods which have rectified the direction of the forceps. By this correction he has brought about the conditions met with in my new straight forceps. For several years I have used the forceps of Tarnier and of Levret. With the forceps of Levret the force employed appears to me so great, with the forceps of Tarnier so little, in comparison, that I have now recourse always to the latter.

Further Remarks on the Use of the Intermittent Contractions of the Pregnant Uterus as a means of Diagnosis.

Dr. J. BRAXTON HICKS, London.

In 1871 I read a paper before the Obstetrical Society of London (and it was published in the Transactions of that year, vol. xiii. p. 216), in which it was shown that at least from the third month of pregnancy to the completion of labour, the uterus underwent a contraction, lasting generally one or two minutes, though sometimes longer, at intervals varying much, but from five to twenty minutes generally. The value of these contractions was indicated, both from a physiological point of view, as supplementing the action of the heart, in parts so remote from its influence as the venous blood in the uterine walls and placenta; and also from a diagnostic aspect—namely, the great help this knowledge affords us: 1st, as to the fact of pregnancy; 2nd, that the pregnancy is intra-uterine; 3rd, that even where no fœtus is present, or where it is dead, we can distinguish this state from a uterine or ovarian tumour.

Since the reading of this paper my attention has been continuously directed to the subject, and the object of this communication is to emphasize the remarks made on that occasion and to add something to them.

In that paper the hand was the only means by which it was stated that one could recognize these contractions; but since then I have been enabled to detect their occurrence by means of a gastrograph,* which, placed over the pregnant uterus in a flaccid state, records the respiratory wave, as if no pregnant uterus were present; whereas, when the uterus is in a state of contraction this wave nearly disappears; the uterus acting similarly to a solid mass, though in a less marked degree. But it is in regard to the diagnostic value of these contractions that I desire to add some more information. I may here repeat that these contractions are not the result of external manipulation, but occur spontaneously, as can be readily observed from the fact that, on first placing the hand over the uterus, this will be found to be hard and firm, relaxing even during the examination. However, this is of no great importance in regard to diagnosis, because, if we find a tumour changing in density and hardness we have an assurance that it is the uterus. But in one point we may be deceived if we were to look for the contraction of the uterus only under examination, because if the contraction lasted long, and was existing at the time we first placed our hand over the uterus, then we might, if in

* 'Proceedings of the Royal Society," March, 1878.

haste, think the solid state belonged to a fibroma or other solid growth: whereas, it will be always necessary to occupy some little time, if we wish to make use of this means of diagnosis. I was not aware, at the time of the publication alluded to above, that anyone had made use of these contractions for the purpose of diagnosis; but our present excellent colleague, the President of this Section, had, in his "Clinical Memoirs,"* already described a case where he had made out the existence of pregnancy in a case of vesicular mole by means of this alteration of density, which was well marked in this case. I therefore, with much pleasure, embrace this opportunity of correcting the error into which I had inadvertently fallen. From the numbers of cases in which diagnosis has been assisted by, or entirely dependent on, this change of solidity of the uterus, I will instance a few.

The first one was in a woman who had been tapped in the abdomen a few days before, under the notion that the swelling was owing to enlargement of the ovary. As only about three pints of pinkish fluid escaped, and as much solid remained, she was sent to have the ovary removed. The surgeon to whom she was sent, thinking that the case was mixed with pregnancy, referred the diagnosis to my colleague, Dr. Galabin, who asked me also to see it. I found, as he had done, the mass became harder and firmer at times; and therefore there could be no doubt as to the fact of pregnancy. The fœtus also, though very mobile, was after some difficulty detected. But the problem yet unsolved—namely, was it all uterus, or a co-existent tumour—was cleared up by my being able to notice that the change of density affected the *whole* mass. So that we had three points cleared up by this examination. 1st. That there was pregnancy; 2nd. That it was intra-uterine; 3rd. That there was nothing but a pregnant uterus. The uterus was about the size of seven to eight month pregnancy, notwithstanding its having lost lately three pints of amnial fluid; but as she had only had five months' amenorrhœa, I concluded that it was a case of hydrops amnii; and after waiting a few days the membranes were ruptured and a large amount of fluid escaped, but not till I had removed from inside the opening in the amnial sac some large clots, which had doubtless formed within in consequence of the tapping. I was anxious not to wait longer, as the uterus was again rapidly increasing in size. She recovered without any bad symptom. The fœtus had been dead some time before the labour.

Other cases of hydrops amnii, accompanied by twin pregnancy, have occurred to me, in which the assistance rendered to diagnosis by these contractions has been very great; and the more particularly so, that in one of these there was, in consequence of one amnial sac being natural, a difference of density between the two sides of the uterus, giving rise to a suspicion of some tumour co-existing with ordinary pregnancy.

In one such case the patient was at the seventh month of pregnancy as large as at full term. The uterus was irregularly shaped, and it was difficult at first sight to make out the condition. However, after waiting carefully for a short time I felt the *whole* mass, under the influence of the contraction, assuming a unity of form corresponding with that of the pregnant uterus. Thus, I felt assured that the whole mass was included in the uterus, and I ventured to pronounce it to be a case of twins, with probably one sac over-distended with hydrops amnii. This turned out to be correct. The difficulty in this case was increased, because no fœtal movements nor heart sounds could be felt, as the fœtuses were dead.

In a case of hydrops amnii which occurred in my practice before the fact of these contractions was known to me, the uterus, extremely distended by excess

* "Clinical Memoirs on Diseases of Women." 1863.

of amnial fluid, showed an oblique depressed line, giving me the impression that it was hydrops amnii coupled with an ovarian tumour, which proved incorrect, for the whole was the uterus with hydramnios. Had I looked for the contractions I might, and most probably would, have noticed that the whole mass was included in the change. Now, the inclusion or non-inclusion of the whole mass in the general contractions is of great value as a means of determining the co-existence of pregnancy with a tumour, and how much belongs to each; but also, whether in a given case there is both extra and intra-uterine conception; and, still further, to distinguish the one from the other.

In one case, where there were fibroid growths about the lower part of the uterus, the existence of pregnancy was made clear; but it was for sometime undetermined whether the tumours were the uterus and the fœtus in an extra-uterine sac. The matter was, however, set at rest by watching the portion in which the fœtus lay; as this went through the variation of density I was able to decide this question in favour of intra-uterine pregnancy, which opinion proved correct.

But there are other conditions in which we find a difficulty in determining the existence of pregnancy, in consequence of the death or absence of the fœtus, as in a case of vesicular mole pregnancy (hydatidiform degeneration of the chorion). I have already pointed this out in my paper referred to. But a case presented itself for determining the fact of pregnancy, where the uterus was so high up that the os could scarcely be reached, much less could investigation by ballottement be accomplished. There was a tumour above the pubes reaching to the umbilicus, central and symmetrical. On patiently waiting it was found to be much firmer at intervals, and for a short duration, and thus the diagnosis was completed. Of course, it cannot determine whether the fœtus be living or dead; but I have always recognized that it is very difficult to make out the fœtus during the contraction, though generally it may be felt during relaxation. If a fair amount of laxity be present, and we are yet unable to make out the fœtus, we have a strong reason for presuming that it is a kind of mole conception.

But there is another state of the pregnant uterus upon which I have many times been consulted by medical men, simulating the co-existence of a tumour with pregnancy. It is more noticeable about the fifth or sixth month. When the uterus is relaxed the fœtus may be generally easily felt; but on one side a large, somewhat firm mass is felt, often reaching above the level of the fœtus. This more solid mass is the placenta and uterine wall, and gives the impression of a tumour in addition to pregnancy. Indeed, it possesses much the physical characters of an extra-uterine fœtation, with the enlarged uterus and placenta on one side. Now, all these points are cleared up at once by finding that the whole mass, solid and fœtal portions, are converted into one globular firm mass. These cases are by no means rare, and I have always found complete assistance from recognizing this remarkable change of shape and density.

DISCUSSION.

Dr. MATTHEWS DUNCAN, London, pointed out that some considerable subtraction must be made from the value of this diagnostic sign, because a soft fibroma without pregnancy—itself, or its capsule, or both—contracted quite as distinctly and with as much change of shape as a gravid uterus.

Professor HENNIG, Leipzig: There are rare cases in which the contraction of a doubtful tumour may cause difficulty in diagnosis—namely, those in which pregnancy

intra- or extra-uterine, at an early stage, is complicated with soft myoma uteri. In such cases, even by repeated investigation at different times, it is not always possible to determine the locality of contractions; whether they take place in the uterus alone, or in the myoma, or in both.

Proposal for a Common Nomenclature in Obstetrics.

Prof. ALEXANDER R. SIMPSON, Edinburgh.

What is the right oblique diameter of the pelvis?

If I paused for replies from the representatives of different schools here assembled I should receive different answers. Some would say, The right oblique diameter of the pelvis is the one that runs between the right sacro-iliac synchondrosis and the left pectineal eminence. Others would say, It is the line, rather, which runs across from the right pectineal eminence to the left sacro-iliac synchondrosis. There is no good reason why the diameter which one author calls "right" should be called "left" by another. I take the opportunity of this International Congress of Obstetricians to indicate some of the points on which there seems to be an unnecessary divergence of nomenclature, and to suggest the possibility of arriving at a common terminology.

MECHANISM OF LABOUR.

Movements of the passenger through the passages.

Supposing the fœtus during its progress to be making a constant movement of descent from the commencement to the close of the second stage, we note four movements which are more or less distinct and successive.

I. Flexion.
II. Rotation (internal).
III. Extension.
IV. Rotation (external).

STAGES OF LABOUR.

Let these be three in number.

I. Leaving out of view the premonitory symptoms as not belonging to labour proper, let the First stage begin with the pains that actually dilate the os, and let it end with the full expansion of the cervical canal and orifice. Let this first stage be called the "Stage of Dilatation."

II. Let the Second Stage be the "Stage of Expulsion," and comprise the phenomena attendant on the extrusion of the entire child.

III. Let the Third stage be the "Stage of the After-birth," including the phenomena associated with the separation and extrusion of the placenta and membranes.

FACTORS OF LABOUR.

Let us speak of the three elements that are present in every labour and in every stage as the "Factors." They may be looked at in any order. I would suggest the following:—

I. The Powers or forces by which the purposes of the labour are effected,

constituted by the Primary and essential Powers, or the efforts of the uterus, and the Secondary or accessory Powers, or the efforts of the respiratory and abdominal muscles.

II. The Passages or canals, soft and hard, through which the powers have to propel the uterine contents.

III. The Passenger or product of parturition, which is chiefly the fœtus, but includes the entire contents of the gravid uterus.

Diameters of the Pelvis.

There are some of the less important of the pelvic diameters, such as the interspinous, intercristal, sacro-cotyloid, that it will be best to indicate by their anatomical designations. But the leading diameters of the true pelvis cannot thus be easily named, and we must still speak of the antero-posterior or Conjugate (C. D.), the Transverse (T. D.), and the Right and Left Oblique or diagonal diameters (R. O. D. and L. O. D.). Only let us all understand by the "right oblique diameter" that which begins at the right sacro-iliac synchondrosis; and by the "left oblique diameter" that which begins at the left synchondrosis.

Diameters of the Fœtal Head.

Let the Occipito-Mental diameter run from the tip of the occiput to the centre of the chin, and be the O. M. diameter. Let the Occipito-Frontal run from the occipital protuberance to the glabella, the projecting portion of the frontal bone above the root of the nose, and be the O. F. diameter. Let the Sub-Occipito-Bregmatic run from the point where the nucha meets the occiput to the anterior fontanelle or bregma. We may think of it as terminating either at the anterior or posterior angle of the bregma or anywhere between, according to the degree of flexion of the head; but for purposes of measurement it should terminate in the centre of the bregma, and be indicated by the letters S. O. B. diameter. The transverse diameters are the Bi-Parietal running between the extremities of the parietal protuberances, the BI.-P. diameter; and the Bi-Temporal running between the two lower extremities of the coronal sutures, the BI.-T. diameter.

The Presentation or Lie of the Fœtus.

Let there be three chief presentations.

I. Cephalic, including—
   - Vertex and its modifications, and
   - Face and its modifications.

II. Pelvic, including—
   - Breech,
   - Footling,
   - Knees.

III. Trunk or transverse, including—
   - Shoulder or arm and other rarer presentations.

The Position or Look of the Child.

In the various presentations let us take a determined point in the circumference of the engaging part of the fœtus, and regard it as directed—first, either to the left side of the maternal pelvis or the right; and secondly, either forwards or backwards. This will save us the misunderstandings that arise from conflicting numerical designations. Apart from the rare directly transverse positions, when the positions would be simply left and right, we should have the positions—left . . . . anterior

or posterior, or right . . . . posterior or anterior. Taking the occiput for vertex presentation, the chin (mentum) for face, the sacrum for breech, and the acromion for shoulder presentations, we have—

Left (L.)	Occipito-(O.)	Anterior (A.)
	Mento-(M.	Posterior (P.)
Right (R.)	Sacro-(S.)	Posterior (P.)
	Acromio-(A.)	Anterior (A.)

DISCUSSION.

Professor HORWITZ, St. Petersburg: The proposal of Dr. Simpson appears to me somewhat strange. How admit a uniform nomenclature in obstetrics when all are of one accord? To simplify, for example, the number of fœtal positions is impossible, because it is reduced to the last degree. Taking the diameters of the pelvis, I believe that everyone knows quite well what the internal conjugate is. What is there new in the proposal of Dr. Simpson to divide the divers factors of labour into (1) forces; (2) mechanism; (3) fœtus? That the proposal of Dr. Simpson regards principle and not form, I will only indicate the manner in which he proposes to divide the mechanism of labour. He has divided it into flexion, rotation, extension, and restitution. Who wishes to describe restitution? Dr. Simpson has not done so, forsooth. I cannot understand this technicality. In one word, the proposal of Dr. Simpson is not realizable, because it requires a uniformity in the manner of looking at scientific objects.

Dr. A. F. A. KING, Washington, spoke of the necessity of defining the obstetrical vertex, which he (Dr. King) considered to be a circle around the centre of the occipital pole of the head. Dr. King also mentioned that the termination of the first stage of labour should be the extrusion of the head through the os uteri into the vagina, rather than "full dilatation of the os," for the reason that the definition of the latter must depend, practically, upon the size of the head, which is seldom exactly alike in two cases.

Dr. BUDIN, Paris: It would be a great advantage to possess a common terminology, and for different countries to accept words and expressions clearly defined. We shall evidently meet with difficulties in the realization of this idea; each country will, perhaps, try to preserve its nomenclature; while the task will be made easier by the proposal of Dr. Simpson, to accept, as far as possible, an anatomical terminology. Besides, two kinds of expressions—the old and the new—could be employed in each country until the latter had become understood by all. Recently one of our American *confrères* has proposed to accept the metric system in medicine; it is an analogous step that Professor Simpson would bring about. I do not doubt that many compatriots present at the Congress join with me in asking for the proposal careful consideration.

It was moved by Professor Simpson, seconded by Professor Stephenson, and resolved, that a committee, consisting of Dr. Budin, France; Dr. Halbertsma, Holland; Dr. Hennig, Germany; Dr. Hubert, Belgium; Dr. King, America; Dr Mangiagalli, Italy; Professor Simpson, Great Britain; Dr. Slavjansky, Russia; Dr. Sadtfeld, Denmark, be appointed to draw up a scheme for a common nomenclature in obstetrics, to be considered in the Obstetrical Section of the next meeting

of the International Medical Congress, the representative of each country to consult with a committee of his fellow-countrymen.

Sur une Disposition Particulière des Œufs dans la Grossesse Gémellaire. Deux Observations.

Dr. P. Budin, Paris.

Le 19 Juin, 1880, en examinant à l'Hôpital des Cliniques un arrière-faix provenant d'un accouchement gémellaire qui avait eu lieu pendant la nuit, je constatai qu'il présentait une disposition particulière. Au premier abord il semblait n'exister qu'un œuf; il y avait une grande poche formée par les membranes, mais on voyait sur la périphérie deux masses placentaires distinctes. En regardant par l'orifice unique qui existait sur les membranes on trouvait une première cavité dans laquelle existait un cordon se rendant à l'un des placentas. On voyait en outre un second orifice sur une cloison membraneuse, et par cet orifice on entrait dans une seconde poche où se trouvait un autre cordon allant s'insérer sur l'autre placenta. On aurait donc pu croire à une poche unique, mais dans la première poche s'en trouvait une seconde. Chacune de ces poches avait contenu un fœtus. L'accouchement n'avait rien présenté d'extraordinaire. Le deux enfants étaient du sexe féminin, bien portants, celui qui était sorti le premier pesait 2,050 grammes, le second pesait 2,650 grammes. La délivrance avait été naturelle.

Frappé de la disposition particulière que présentait l'arrière-faix, je le mis de côté me réservant de l'étudier dans la journée, et j'allai assister à la visite des malades, puis à la leçon clinique faite par le chef de service, M. le Professeur Depaul. Un peu avant 11 heures on vint me prévenir qu'une femme chez laquelle il existait une présentation anormale venait d'être amenée du dehors à la salle d'accouchement. Je trouvai une grossesse gémellaire; le premier enfant se présentait par l'épaule; il y avait eu outre un rétrécissement du bassin. Je fis l'extraction des deux enfants qui étaient du sexe masculin; le premier pesait 1,600 gr., le second, 2,100 grammes. La délivrance fut naturelle. J'examinai l'arrière-faix et je constatai que par une coïncidence singulière, il présentait presqu'absolument la même disposition que l'arrière-faix vu quelques heures auparavant. On ne pouvait relever qu'une seule différence entre les deux cas. Dans le premier, les deux cordons allaient s'insérer sur le bord des deux placentas (insertions en raquette) dans le second il y avait une insertion en raquette de cordon sur l'un des placentas et une insertion centrale sur l'autre placenta.

J'étudiai avec soin ces deux arrière-faix dans l'espérance d'avoir l'explication d'une disposition qui au premier abord me paraissait difficile à comprendre. Tous deux, disséqués avec précaution, me fournirent les mêmes resultats.

Je pus, dans chaque arrière-faix séparer, isoler complétement deux œufs. L'un des œufs était situé au fond de la cavité utérine et semblait avoir une forme régulière. L'autre œuf, qui était placé au-dessous, en rapport avec le segment inférieur de l'utérus avait au contraire une forme irrégulière. Il formait une grande poche qui recouvrait complétement le premier œuf, l'enveloppant de toutes parts, arrivant jusqu'au niveau des bords du placenta, le coiffant en realité comme un bonnet de coton coiffe une tête, comme une séreuse coiffe un organe.

Chaque œuf avait un chorion et un amnios distinct. L'œuf qui était en rapport avec le segment inférieur de l'utérus et qui enveloppait l'autre œuf présentait une caduque sur toute la partie de sa surface qui était en rapport avec

l'utérus. L'autre partie de sa surface qui enveloppait l'œuf logé dans le fond de l'utérus ne présentait pas une caduque constituant une membrane, mais entre les deux chorions on voyait quelques filaments blancs rosés qui n'étaient évidemment autre chose que des filaments de caduque.

Au niveau de la cloison qui séparait les deux cavités amniotiques il y avait donc quatre membranes; un amnios, un chorion, un chorion et un amnios; entre les deux chorions existaient quelques débris de caduque.

Mais comment expliquer cette disposition? Ces deux cas ne pourraient-ils pas faire penser à la fécondation d'un ovule alors qu'il aurait déjà existé un autre ovule fécondé et développé dans la cavité utérine?

On peut faire une première hypothèse: un œuf s'étant déjà developpé dans la cavité utérine, un second œuf se trouve fécondé. Il descend à son tour dans la cavité utérine. En s'y développant il refoule devant lui les membranes du premier œuf dont il s'enveloppe complétement pour ainsi dire. Dans cette hypothèse l'œuf qui se trouvait à la fin de la grossesse en rapport avec le segment inférieur de l'utérus se serait développé le premier; l'œuf qui occupait le fond de l'utérus se serait développé ensuite.

Mais dans les deux cas le fœtus de l'œuf qui était contenu dans le fond de la cavité utérine pesait 500 et 600 grammes de plus que le fœtus de l'œuf qui était en rapport avec le segment inférieur de l'utérus.

Une autre hypothèse peut être faite. L'œuf occupant le fond de l'utérus se serait développé le premier. On sait qu'entre la caduque utérine, et la caduque ovulaire, il n'y a pas immédiatement adhérence, que cette adhérence ne survient au contraire qu'au bout d'un assez grand nombre de semaines. Quand un second œuf est arrivé dans la cavité utérine, il s'y est développé comme il a pu, il s'est insinué partout dans l'espace resté libre entre la caduque ovulaire de l'œuf déjà développé et la caduque utérine. C'est ainsi que l'œuf qui occupait le segment inférieur de l'utérus a pu s'étendre partout, envelopper, englober pour ainsi dire l'œuf qui occupait le fond de l'organe. Les débris de caduque trouvés entre les chorions des deux œufs seraient donc des débris de caduque ovulaire.

DISCUSSION.

Professor SLAVJANSKY, St. Petersburg: I think that the investigations made by Dr. Budin explain perfectly the cases of twin pregnancy, where one of the ova has been long dead and is very small. In such cases I have found a similar arrangement of the membranes. The explanation by superfœtation, as given by Dr. Budin, would have been more certain had a microscopical examination of the membranes—especially of the decidua—been made by him.

Dr. BUDIN, Paris: I regret that it was not possible for me to make a microscopic examination of the rose-white coloured filaments existing between the two chorions; but their appearance was precisely similar to the filaments found in the decidua. I do not believe that the existence of an intermediate membrane between the two chorions is necessary. At the commencement of pregnancy it is evident that the decidua of the ovum might form a continuous membrane; but as the result of a considerable development of the two ovules, this membrane must become so stretched as to disappear, and to be only represented by filaments.

Oöphorectomy—Battey's Operation—Spaying—Castration of Women.

Dr. Robert Battey, Rome, Georgia.

Definition.—The operation which has been proposed for this discussion is peculiar in that it has for its object not simply the removal of an offending mass or member from the human body, because of its having become, *per se* and by reason of disease, an intolerable burden or a formidable menace to life. It is by the extirpation of an offending organ, endowed with a peculiar and essential function, the performance of which function has become morbid and destructive of health, or endangering life, we seek to abrogate the function itself, and thus do away with its pernicious consequences.

Whilst it is undoubtedly true that, in the execution of the operation, we remove, in the great majority of instances, ovaries which bear upon their face the visible evidences of structural disease, still it is equally true that these diseases, were it not for the function of ovulation, would be of little matter, so far as the general organism is concerned, and many of the chronic inflammations would entirely disappear, as they are known to do after the climacteric. Whoever, in the future of medical progress, shall discover a medicament endowed with some peculiar and subtile potency to abrogate the function of the ovary and do away with ovulation, will have plucked from the brow of the operative surgeon all laurels which he may have won by the performance of this operation. It should be borne in mind, then, that the great object of the operation is not to do an ovariotomy, or an oöphorectomy, but *to establish by art the change of life.*

The surgical procedure by which it is sought to produce the artificial change of life has received, from different writers, a variety of designations, no one of which has as yet gained the general sanction of the profession. In America it was at first introduced under the name Normal-ovariotomy, a name badly chosen and very soon abandoned. Spaying was suggested, but generally condemned as being inappropriate, repulsive to refined taste, and especially offensive to the subjects of the operation, who have earnestly protested against it. Oöphorectomy was considered, and rejected upon the ground that the term had been introduced by Dr. Peaslee in his work upon "Ovarian Tumours," and accepted as a synonym of ovariotomy, and therefore calculated to produce confusion. The term "Battey's operation" was suggested by Dr. Marion Sims, and is now very generally, though not universally, received. In Germany the operation is known as the castration of women, a term as objectionable as spaying.

If the suggestion of Simpson, that the ovaries perhaps need not be removed but only ligatured, should be justified by experience, could the operation be properly termed either an oöphorectomy, a spaying, or a castration? Would its essential function—*i.e.*, the production of the change of life by art—be still altered?

History.—The extirpation of the healthy ovaries as a remedial measure seems first to have been contemplated by Dr. James Blundell, of London, as early as 1823, as has been pointed out by Dr. Aveling.* Blundell, however, does not appear to have regarded his own suggestion as likely to lead to any practical

* *Obstetrical Journal of Great Britain and Ireland*, vol. vi. p. 617.

results, for he remarks that the operation "can scarcely in any instance be necessary."

As early as October, 1865, Battey* conceived the idea of producing an artificial menopause for the remedy of disease. Briefly recounting a fatal case, he remarks: "There was reason to believe that the cardiac malady in this case had its origin in the amenorrhœa. Nature had provided ovaries performing their function in full vigour, but no womb to respond by the proper issue of the menses. I said to myself, 'If she could be relieved of her ovaries the balance would at once be restored, the menstrual molimen would cease, the violent strain upon the heart would be at an end; there might be hope for her. I searched, but searched in vain, for an authority, for a precedent. How dare I to make one?' . . . . Inwardly, I resolved that another such case should not perish in my keeping without my reaching out a friendly hand in the hope of rescue."

On the 27th of July, 1872, Hegar† did his first operation at Friburg, with fatal result. August 1, 1872, Lawson Tait‡ operated at Birmingham, also with fatal result. August 17, 1872, Battey§ operated at Rome, Georgia, with success, and published his case in the following month. In April, 1873, he discussed the operation before the Medical Association of Georgia.‖ He operated again in March, 1874, and a third time in June, 1874, with like success. December 18, 1872, Gilmore,¶ of Mobile, also followed Battey in a successful case, which he published in 1874. On the 2nd of August, 1876, Hegar did his second operation, with success.

Is there a proper field for the operation?—During the Civil War in America, in a rural district in the State of Mississippi, where competent medical aid could not be had at that time, Mrs. H., aged twenty, was confined of a first child. The labour was protracted and difficult, and was terminated at last by the obstetric forceps unskilfully used. Great pelvic inflammation, accompanied by sloughing of tissue, ensued, and the patient narrowly escaped death, getting up with complete occlusion of the whole utero-vaginal canal. She had no return of her menses, but in lieu thereof monthly sufferings, depicted by her physicians as being both "intolerable and indescribable."

Five years subsequent to the labour, she was subjected to operation by an eminent surgeon of New Orleans, with a view of restoring the lost canal and giving outlet to the menses. Failing of relief, the operation was repeated the following year by another surgeon of the same city, but with a like result. She now went from hand to hand the round of the profession in her section of the country, and took counsel at Louisville, Kentucky, but obtained no relief.

On the 8th of February, 1877,** both ovaries were removed by abdominal section, and her fourteen years of suffering—suffering by reason of an unrelieved menstrual molimen—was completely remedied. Was this a case for the operation? Having lost for ever all her uterine functions, of what possible utility could be her ovaries?

Dr. Grange Simons,†† of Charleston, S.C., reports a case of "Atresia vaginæ:

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* "Normal Ovariotomy." Atlanta, Georgia. 1872.

† "Die Castration der Frauen." Leipzig, 1878.

‡ *Brit. Med. Jour.*, 1879, p. 813.

§ *Atlanta Med. and Surg. Journal*, Sept. 1872, p. 321.

‖ *Ibid.*, April, 1873, p. 1. ¶ *Ibid.*, 1874, p. 321.

** "Trans. Med. Soc. Virginia," 9th session, p. 432.

†† "Trans. South Carolina Med. Assoc.," 1872, p. 56.

death from tetanus at menstrual period, with account of post-mortem appearances."

M. E., a negress, primipara, was delivered by craniotomy and evisceration, May 5, 1869. Atresia vaginæ followed; and in December of the same year her sufferings had become so great that an unsuccessful attempt was made to re-establish the vaginal canal and reach the uterus. Dr. Simons says:—"The patient continued under my care, suffering intensely at each recurring menstrual period, but never showing any appearance of an accumulation." November 19, 1870, another attempt was fruitlessly made to remedy the difficulty. At this operation, the cul-de-sac of Douglas was opened unwittingly, and one of the ovaries prolapsed through the wound. It was replaced, and stitches taken to prevent its return. The report continues:—"On the 14th of July, 1871, I saw her suffering intensely with pains in back and limbs, also some rigidity in recti abdominales, pain and rigidity in masseters. These I deemed to be hysterical phenomena." The tetanic symptoms became well pronounced. There was no history of wound or injury. "July 27th, I saw her again in the evening, and found her much worse. The remedies now seemed of no avail to relieve her of the pain or relax the severity of the spasms. Jaws tightly locked, but still able to swallow fluids; opisthotonos. She remained in this state of fearful suffering until the morning of the 30th, when death closed the dreadful tragedy, sixteen days from its commencement."

In this case had Dr. Simons removed the prolapsed ovary, in place of returning it to the abdominal cavity, and removed the other ovary also, the result might have been as happy as in the previously cited case of Mrs. H.

Were these proper cases for the operation? To deny it is but to assert the absurd proposition that agonizing suffering, ending in miserable death, is to be preferred to health and continued life.

Indications.—It was foreseen from the very inception of this operation that its sphere of applicability, in exceptional cases, must be as widely extended as the very diverse effects which are known to follow upon perversions of the function of ovulation. Hence, the writer attempted to mark the boundaries in a comprehensive way by proposing:* "Ovariotomy to determine the change of life for any grave disease which is incurable without it, and which is curable with it." More enlarged experience and much study of the subject have not modified, in any essential particular, the views then entertained upon this point. Perhaps no safer rule can be laid down to-day by which one may determine in any given case the propriety of the operation than by asking himself three questions—namely: 1st. Is this a grave case? 2nd. Is it incurable by any of the resources of the art short of the change of life? 3rd. Is it curable by the change of life? If all three of these questions can be answered affirmatively the case is a proper one, but if not the operation is not to be justified.

Attempts have been made to group the indications with a view to the existing menstrual conditions under three heads—namely, dysmenorrhœa, menorrhagia, and amenorrhœa. However convenient and satisfactory such classification might be viewed in general, it may fairly be questioned whether it is expedient to have an operation of so grave and dangerous character, and one so open to abuse, go out to the world as a recognized and legitimate remedy for either dysmenorrhœa, menorrhagia, or amenorrhœa, however carefully we may qualify the proposition.

* *Atlanta Med. and Surg. Jour.*, 1873, p. 67.

That the operation, in its very essence, opens the door for widespread abuse was foreseen from the first, and has been recognized and pointed out by judicious minds everywhere. Its future usefulness to the world depends much upon the zealous care with which the door is guarded. This operation cannot, therefore, in any case be viewed as an alternative for other means of cure, but must be held, as it was originally offered, for a *dernier ressort*.

Operation.—There are but three points connected with the execution of the operation which need call for special attention.

1st. Mode of access. In America operators have been somewhat divided in choice between the abdominal and vaginal route, both being in use. In Europe the abdominal has been almost exclusively chosen.

Practically, the vaginal method has been carried out, in a number of instances, with great ease and success. A difficulty, however, has presented itself in the rather frequent occurrence of cases wherein the ovaries were found to be more or less firmly bound down by adhesions to surrounding structures, and their removal difficult, uncertain, and unsatisfactory. In a few favourable cases the results obtained by this method, and without any antiseptic precautions, have been all that Mr. Lister himself could desire. In view of its rational advantages and the excellence of the results in advantageous surroundings, it is believed that it should not be hastily and wholly abandoned, but reserved for such cases only as may, by the free and ready access to the ovaries, be found suitable for it. So frequent, however, are the cases in which adhesions are to be encountered, which can be more conveniently and satisfactorily dealt with through the abdominal opening, and in cases also where the ovaries are much elevated by the growth of uterine fibromata, the abdominal method is to be preferred.

2nd. Dealing with the pedicle. Two plans have been pursued in practice.

(*a.*) The ligature has to commend it security against hæmorrhage, and, when carbolized, comparative innocuousness. It may however be objected to as a possible source of local irritation. In the first case of Battey the ligature was used, and it was observed that very great pain was experienced by the patient at the seat of the ligatures for hours after—a pain out of all proportion to any complained of by others when the ligature was not used. Moreover, this case was followed by occasional and very protracted uterine hæmorrhages for several years after, which had not previously existed, the case being one of long protracted amenorrhœa, and which could not be explained. It was feared that a sustained irritation of the trunks of the ovaric nerves by the encysted ligatures might be a cause for the hæmorrhages. In another case an abscess formed at the site of an encysted ligature, which emptied itself into the abdominal cavity and produced speedy death.

(*b.*) The écraseur suggested itself as an efficient substitute, and has been relied upon in thirteen operations. It is found expedient to constrict the pedicle by temporary ligature so as to form a groove for the chain, or wire, which is placed behind and brings the ligature away with the ovary. In practice no difficulty has been encountered in placing the chain with precision where it was practicable to so place a ligature. The use of the écraseur has not met with approval of other operators; and chiefly because of apprehension of troublesome hæmorrhage. As far as practised no such complication has occurred in any case, but how far it may be safely trusted a more enlarged experience can alone determine.

3rd. *Removal of the ovaries.*—In May, 1879, Professor Simpson* suggested the

* *Brit. Med. Journal*, 1879, p. 766.

inquiry whether it be necessary to remove the ovaries after strangulating their pedicles with the ligature. This suggestion led to practical test in a case operated on at Mount Morris, Michigan, August 9, 1880, by Drs. Bardwell and Rumer,* who report—"May now be said to be a perfect success, as the patient is perfectly comfortable and able to attend to her duties." In the event that this procedure should be proved to effect the change of life, and none of the possible objections suggested by Simpson obtain—as the ovaries removed in these cases usually show structural disease, it would be interesting to know what would be the result of leaving the diseased ovaries after strangulation, and especially whether they might be likely to take on malignant disease, as in the case of an undescended, or a dislocated testis.

Results.—The results of the operation may be considered in two classes—namely, the proximate result, as regards the mortality, and the ultimate results, in the effect upon the menses, the aphrodisiac powers, the female graces, and the general health of the survivors.

(1) *Mortality.*—The loss of life which has followed upon this operation has been much larger than *à priori* might have been expected, as will appear from the appended table of cases, which show a death-rate of 18 per cent. That enlarged experience begets increased skill, and with increased skill comes a greatly lessened mortality, is a fact which has been abundantly shown in the history of ovariotomy. Thanks to the fruitful labours of Mr. Lister, we seem to be upon the eve of greatly improved statistics. Indeed, the late report of Lawson Tait of twenty-six consecutive cases with but a single death, and the still later list of twenty-six cases by Savage without a solitary broken link, testify that the new era is already fairly inaugurated.

(2) *Menses.*—It is a well-known fact that double ovariotomy in exceptional cases has been followed by continued appearance of the menses,† and this not only occasional and erratic, but in some instances normal in its regular periodicity, normal in colour, in quantity, and in duration.‡ These curious cases may be in part explained upon the hypothesis of the existence of a third, or supplemental ovary. A fragment of an ovary, containing healthy Graafian cells, has been found by the writer after ovariotomy embedded in the pedicle of an ovarian tumour close to the point of section. Even small fragments of the ovary left behind have been shown§ to be quite sufficient to keep up regular menstruation after incomplete removal of the organs. In none of the cases recorded of continuance of the menses after double ovariotomy is the evidence of the absence of a supplemental ovary, or of a fragment of stroma hidden in the stump, satisfactorily shown. These cases, however we may account for them, are admitted to be exceptional, and the rule that the menopause follows the removal of both ovaries is well established by the teachings of experience. But it is not necessary for the justification of this operation that the monthly discharge of blood from the uterus should be done away. It is the vascular and nervous perturbations attendant upon the function of ovulation which we are seeking to arrest, and this we can surely accomplish by removing the ovaries. Indeed, it has been shown by the writer that very satisfactory results have been obtained in cases wherein the ovaries were but imperfectly removed, and this notwithstanding the continuance of the menses In this connec-

* *Obstetric Gazette*, Cincinnati, Sept. 1880, p. 120.

† "Ovarian Tumours," Peaslee, p. 526. ‡ "Ovarian Tumours," Atlee, p. 37.

§ *Brit. Med. Journal*, April 3, 1880.

Table of Cases of Battey's Operation. (*Complete.*)

	Operator.	Date.	Age.	Married or Single.	Indications for the Operation.	Method.	Treatment of Pedicle.	Primary Result.	Ultimate Result.
1	Alberts	Oct. '78	34		menorrhagia	abd.	lig.	died	
2	Battey	Aug. 17, '72	30	S	amenorrhœa, hystero-epilepsy	„	„	recovered	cured
3	„	June, '74	38	M	ovaralgia and mental disorder	vag.	ecras.	„	comfortable health
4	„	June, '75	30	M	ovaralgia and mental disorder	„	lig.	died	
5	„	Aug. '75	25	S	ovaralgia	„	ecras.	recovered	complete cure
6	„	Sept. '75	29	S	ovaralgia	„	„	died	
7	„	Feb. '77	34	M	occlusion of vagina and uterus, violent molimen	abd.	„	recovered	complete cure
8	„	Nov. '78	28	M	hystero-epilepsy	„	„	„	cured
9	„	Nov. '78	22	S	hystero-epilepsy	vag.	„	„	{ epilepsy cured— { health much improved
10	„	Mar. '79	37	M	menstro-mania for nine years	„	„	„	but little improved
11	„	Sept. '80	18	S	menstro-mania	„	„	died	
12	Bardwell and Bumer	Aug. 9, '80	19	S	dysmenorrhœa — dementia threatened	abd.	lig.	recovered	cured
13	Börner	Feb. '78	29	M	perioöphoritis	„	„	„	„
14	fraon	Dec. '78	36	S	hystero-epilepsy	„	„	„	„
15	Czerny							died	
16	„				hystero-epilepsy			recovered	not cured
17	„				retroflexion and hysteria			„	
18	De Witt	Nov. '80	30	S	ovaralgia	abd.		„	cured
19	Engelmann	Sept. '77	42	M	hystero-neurosis	„	lig.	died	
20	„	Feb. '78	33	M	ovaralgia and hystero-epilepsy	„	„	„	
21	„	Mar. '78	31	M	ovaralgia and dysmenorrhœa	„	„	„	
22	Franzolina	Aug. 14, '80	23	S	hystero-cramp-mania			recovered	promising well
23	Freund				myoma	„	„	died	
24	„				„			recovered	menopause
25	„				„			„	„
26	„				hystero-epilepsy			„	„
27	Frew	Mar. '77	24	S	dysmenorrhœa— convulsions-mania	abd.	lig.	„	cured
28	Gilmore	Dec. '72	27	M	hystero-epilepsy	„		„	much improved
29	Goodell	Oct., '77	33	S	myoma and ovaralgia	vag.	„	recovered	cured
30	„	„ '79	34	M	myoma and hæmorrhage	abd.	„	died	
31	„		27	S	pernicious menstruation	vag.	„	recovered	cured
32	„	Nov. '78	38	M	„ insanity	„	„	„	cured
33	„	May, '78	37	M	ovaralgia	„	„	died	
34	„	Jan. 10, '80	30	S	pernicious menstruatn. insanity	abd.	„	recovered	virtually cured
35	„	Feb. 28, '80	29	S	prolapsed ovaries, hysteria, opium eater	vag.	„	„	immensely improved
36	„	May. 7, '80	27	S	pernicious menstruation	„	„	died	
37	„	Sept. 17, '80	30	S	„ „	„	„	recovered	menses returned
38	Hegar	July. 27, '72	27	M	ovaralgia and menorrhagia	abd.	„	died	
39	„	Aug. '76	38	M	myoma	„	„	recovered	good
40	„	Aug. '76	41	S	„	„	„	„	„
41	„	Aug. '77	44	M	„	bilat.	„	„	improved
42	„	Oct. '77	25	S	„	„	„	„	good
43	„	Dec. '77	38	S	dysmenorrhœa	abd.		died	
44	„	Jan. '78	37	S	myoma and hæmorrhage	„	„	„	
45	„	Feb. '78	29	M	dysmenorrhœa	„	„	recovered	
46	„	Feb. '78	24	M	„	„	„	died	
47	„				myoma			recovered	cured
48	„				„			„	„
49	„				„			„	not cured
50	„				„			„	„ „
51	„				„			died	
52	„				ovaritis, pelvic adhesions			„	
53	„				„ „ „			recovered	cured
54	„				„ „ „			„	„
55	„				„ „ „			„	„
56	„				„ „ „			„	„
57	„				„ „ „			„	„
58	„				„ „ „			„	„
59	„				„ „ „			„	„
60	„				„ „ „			„	„
61	„				„ „ „			„	„
62	„				„ „ „			„	not cured
63	„				„ „ „			„	„
64	„				„ „ „			„	„
65	„				myoma			died	
66	„				„			recovered	not cured
67	„				dysmenorrhœa			„	cured
68	„				incurable uterine disease			died	

* Strangulation of pedicle by ligature, ovary not removed (see p. 283).

	Operator.	Date.	Age.	Married or Single.	Indications for the Operation.	Method.	Treatment of Pedicle.	Primary Result.	Ultimate Result
69	Hegar				incurable uterine disease			recovered	improved
70	,,				,, ,, ,,			,,	
71	,,				,, ,, ,,			,,	
72	,,				,, ,, ,,			,,	
73	,,				chronic pelvic inflammation			,,	cured
74	,,				,, ,, ,,			,,	,,
75	,,				,, ,, ,,			,,	,,
76	,,				,, ,, ,,			,,	greatly improved
77	,,				,, ,, ,,			,,	,, ,,
78	,,				,, ,, ,,			,,	
79	,,				,, ,, ,,			,,	
80	Kaltenbach		43	S	uterine tumour	abd.	lig.	died	
81	Langenbeck		21	M	epilepsy, uterus absent	lateral	,,	recovered	cured
82	Lusk	Jan. '80	39	S	ovarian dysmenorrhœa	abd.	,,	died	
83	McCormack	May. 13, '80	37	S	ovaralgia, bed-ridden seven years	,,	,,	recovered	great improvement
84	McGuin	Jan. 25, '78	25	S	ovarian dysmenorrhœa	,,	,,	,,	,,
85	,,	Mar. 6, '78	19	S	,, ,,	,,	,,	,,	complete cure
86	,,	Mar. 1, '81	27	S	,, ,,	,,	,,	,,	improving well
87	Malins	Aug. '79	33	M	ovaralgia	,,	,,	,,	cured
88	,,	Dec. '79	36	M	,,	,,	,,	,,	,,
89	Mann	Mar. 11, '80	44	M	bleeding fibroma	,,	,,	died	
90	Martin	Nov. '77	46	M	myoma, hæmorrhage	,,	,,	recovered	relieved
91	,,	Nov. '77	38	S	ovaralgia ,,	,,	,,	,,	improved
92	,,							,,	cured
93	,,				,,			,,	,,
94	,,				rudimentary uterus			,,	,,
95	Muller				ovaralgia			,,	,,
96	Netzel				fibroma uteri	abd.	lig.	died	
97	Noeggerath					,,	,,	recovered	
98	,,					,,	,,	,,	
99	,,					,,	,,	,,	
100	,,					,,	,,	,,	
101	,,					,,	,,	,,	
102	,,					,,	,,	,,	
103	,,					,,	,,	,,	
104	,,					,,	,,	,,	
105	,,					,,	,,	died	
106	,,					,,	,,	,,	
107	Nussbaum				myoma, hæmorrhage	,,	,,	recovered	cured
108	Orr	Oct. '79	28	M	,,	,,	,,	died	
109	Pallen	Oct. '80	28		amenorrhœa, ovaralgia insanity, catalepsy	,,	,,	,,	
110	,,	Dec. '72	33	S	dislocated ovary, ovaralgia	,,	,,	,,	menopause
111	,,	Dec. '78	24	S	hystero-epilepsy, self-pollution	,,	,,	recovered	epilepsy cured—mi
112	Peaslee	June, '70	33	M	absence of uterus, hystero-epilepsy	,,	,,	died	
113	Prince	Nov. '77	18	S	hystero-epilepsy	vag.	galv. caut.	,,	
114	Ross	Mar. 27, '81	34	M	,, ,,	abd.	lig.	recovered	epilepsy cured
115	Savage	Jan. 4, '79	31	S	ovarian dysmenorrhœa	,,	,,	,,	much improved
116	,,	Feb. 4, '80	18	S	absence of uterus, molimen	,,	,,	,,	cured
117	,,	May, 1, '80	20	S	,, ,,	,,	,,	,,	
118	,,	June, 20, '80	23	S	hystero-epilepsy	,,	,,	,,	cured
119	,,	July, 13, '80	35	M	dysmenorrhœa, reflex neurosis	,,	,,	,,	
120	,,	July, 20, '80	40	M	reflex neurosis	,,	,,	,,	cured
121	,,	July, 31, '80	34	M	hystero-epilepsy	,,	,,	,,	,,
122	,,	Aug. 24, '80	46	M	myoma	,,	,,	,,	
123	,,	Aug. 30, '80	31	S	ovarian dysmenorrhœa	,,	,,	,,	cured
124	,,	Sep. 12, '80	40	S	myoma	,,	,,	,,	
125	,,	Sep. 7, '80	23	M		,,	,,	,,	cured
126	,,	Sep. 9, '80	34	M		,,	,,	,,	
127	,,	Sep. 28, '80	18	S		,,	,,	,,	cured
128	,,	Oct. 15, '80	33	M	intense pain	,,	,,	,,	,,
129	,,	Nov. 8, '80	20	M	large myoma	,,	,,	,,	
130	,,	Nov. 10, '80	38	M	prolapsed ovaries, reflex neurosis	,,	,,	,,	
131	,,	Nov. 23, '80	40	M	ovarian pain, and reflex neurosis	,,	,,	,,	
132	,,	Nov. 30, '80	28	M	ovarian pain	,,	,,	,,	cured
133	,,	Dec. 3, '80	41	S	large myoma	,,	,,	,,	
134	,,	Jan. 7, '81	25	S	prolapsed ovary, incurable retroflexion	,,	,,	,,	
135	,,	Jan. 15, '81	41	M	deformity after puerperal pyæmia	,,	,,	,,	
136	,,	Jan. 27, '81	37	M	prolapsed ovary, dysmenorrhœa	,,	,,	,,	
137	,,	Feb. 5, '81	30	M	ovarian prolapse and pain	,,	,,	,,	
138	,,	Feb. 24, '81	30	M	old dysmenorrhœa	,,	,,	,,	
139	,,	Mar. 11, '81	22	M	ovarian prolapse, intense pain	,,	,,	,,	
140	Schreeder			S	mania and nymphomania	,,	,,	,,	mania cured
141	,,				myoma	,,	,,	,,	

	Operator.	Date.	Age.	Married or Single.	Indications for the Operation.	Method.	Treatment of Pedicle.	Primary Results.	Ultimate Result.
	Shucking	July, '79	38	M	ovaralgia and metrorrhagia	abd.	lig.	recovered	completely cured
	Simpson	June, '78	35	S	dysmenorrhœa, &c.	"	"	"	" "
	"	Jan. 1, '81	28	S	dancing fits	"	"	died	
	Sims	Feb. '76	38	M	ovaralgia	"	"	"	
	"	Nov. '79	30	M	hystero-epilepsy	"	"	recovered	cured
	"	Dec. '79	29	S	" "	"	"	"	"
	"	Jan. '80	21	S	" "	"	"	"	not improved
	"	Feb. '80	25	S	" "	"	"	died	
	H. Smith	April, '79	40		dysmenorrhœa	"	"	recovered	cured
	Spiegelburg	Dec. '78	40		myoma	"	"	died	
	"	June, '79	37	W	" hæmorrhage	"	"	recovered	cured
	"	June, '79	26	M	peri-oöphoritis and pain	"	"	"	pain continued
	"	Nov. '79	32	M	hystero-epilepsy	"	"	"	not improved
	L. Tait	Aug. 1, '72			myoma, hæmorrhage	"	"	died	
	"	Dec. '73			" "	"	"	"	
	"	Mar. '74			" "	"	"	"	
	"	July, '79	34	S	dysmenorrhœa			recovered	complete relief
	"	July, '79	26	S	" "			"	" "
	"	Aug. '79	21	S	menorrhagia			"	complete arrest
	"	Aug. '79	43	M	epilepsy and mania			"	" "
	"	Aug. '79	38	S	myoma, hæmorrhage			died	
	"	Oct. '79	28	S	dysmenorrhœa			recovered	complete relief
	"	Oct. '79	24	S	" "			"	great relief
	"	Oct. '79	47	W	myoma, hæmorrhage			"	complete arrest
	"	Nov. '79	21	S	dysmenorrhœa			"	complete relief
	"	Nov. '79	52	W	myoma, hæmorrhage			"	complete arrest
	"	Dec. '79	26	S	dysmenorrhœa			"	complete relief
	"	Jan. '80	29	M	" "			"	" "
	"	Jan. '80	34	M	myoma, hæmorrhage			"	complete arrest
	"	Jan. '80	48	S	" "			"	arrest
	"	Feb. '80	22	M	dysmenorrhœa			"	complete relief
	"	Feb. '80	34	M	" "			"	great relief
	"	Feb. '80	36	S	menorrhagia			"	complete arrest
	"	Mar. '80	52	S	myoma, hæmorrhage			"	" "
	"	Mar. '80	37	M	dysmenorrhœa			"	immediate and complete relief
	"	April, '80	42	S	myoma, hæmorrhage			"	complete arrest
	"	April, '80	17	S	epilepsy, mania			"	great relief
	"	April, '80	33	M	myoma, hæmorrhage			"	complete arrest
	"	April, '80	39	M	" "			"	" "
	"	April, '80	23	M	dysmenorrhœa			"	immediate and complete relief
	"	May, '80	46	S	myoma, hæmorrhage			"	complete arrest
	"	June, '80	38	M	abscess of ovary			"	
	"	Mar. '81	34	M	chronic ovaritis, abscess	abd.	lig.	"	cured
	Tauffer	Sept. '78	25	M	dysmenorrhœa	"	"	"	"
	Thomas		21	S	ovaralgia, epilepsy	"	"	"	somewhat improved
	Trenholme	Jan. '76	31	M	myoma, hæmorrhage	"	"	"	improved, not cured
	Tyng	Jan. '80	40	S	" "	"	"	"	cured
	S. Wells	Jan. '78	49	M	dysmenorrhœa	"	"	"	much improved
	West	Jan. '80	29		hystero-epilepsy, amenorrhœa	vag.	"	"	cured
	Wilson	Dec. '80	40	S	myoma, hæmorrhage	abd.	"	"	cured, contemplates early marriage
	De Zouche	Oct. 3, '79	30	M	chronic ovaritis	"	"	"	greatly improved
	Goodell	May 21, '80	21	S	insanity	vag.	"	"	improved, not cured

Cases. (*Incomplete.*)

Wherein one ovary alone was removed, or wherein both ovaries were but imperfectly removed.

	Operator.	Date.	Age.	Married or Single.	Indications for the Operation.	Method.	Treatment of Pedicle.	Primary Result.	Ultimate Result.
1	Battey	April, '74	32	M	ovaralgia	vag.	lig.	recovered	cured, and bore a child
2	,,	May, '75	24	M	,,	,,	finger nail	,,	greatly benefited
3	,,	May, '75	35	M	,,	,,	ecras.	,,	temporary relief
4	,,	Sept. '75	28	M	,,	,,	,,	,,	not benefited
5	,,	Sept. '75	35	M	,,	,,	,,	,,	,, ,,
6	,,	Jan. '77	[illegible]	M	hystero-epilepsy	,,	,,	,,	cured, bore child
7	Engelmann	April. '80	24	S	ovaralgia	abd.	lig.	,,	somewhat improved
8	Koeberlé	March, '69	[illegible]	M	retroversion	,,	,,	,,	improved
9	Muller							,,	
10	,,							,,	
11	Odebrecht	July 22, '79	29	M	retroflexion, dysmenorrhœa	abd.	lig.	,,	very much improved
12	Pernice	Sept. '78	27	S	dysmenorrhœa	,,	,,	,,	cured
13	,,	Jan. '79			menorrhagia, hystero-epilepsy	,,	,,	,,	,,
14	Sabine	Aug. '74	25	S	ovaralgia, dysmenorrhœa	,,	lig.	,,	much improved
15	Savage	June 29, '78	30	S	,, ,, ,,	,,	,,	,,	,, ,,
16	Sims	Feb. 75	35	W	,, ,, ,,	,,	,,	,,	cured
17	,,	Oct. '75	40	S	,, enteralgia	,,	,,	,,	,,
18	,,	Nov. '75	35	S	,, dysmenorrhœa	vag.	,,	,,	improved
19	,,	Nov. '75	20	S	,, ,, ,,	,,	,,	,,	made worse
20	,,	April, '77	20	S	,, ,, ,,	,,	ecras.	,,	improved
21	,,	May, '77	38	M	,, ,, ,,	,,		,,	not improved
22	L. Tait	Aug. '79	34	M	dysmenorrhœa	abd.	lig.	,,	great relief
23	,,	April, '80	40	M	myoma, hæmorrhage	,,	,,	died	
24	Thomas				dysmenorrhœa, mania	,,	,,	,,	
25	Trenholme	Jan. '76	28	M	chronic oöphoritis	vag.	,,	recovered	not benefited

Summary of Results.

	Number.		Per cent.
Died	40	...	18
Recovered	178	...	82
	218		
Of the ultimate results reported :—			
Complete operations :—			
Cured	88	...	72
Benefited	22	..	19
Not benefited	11	..	9
	121		
Incomplete operations :—			
Cured	6	...	26
Benefited	10	...	44
Not benefited	5	...	22
Not stated	2	...	8
	23		

Oöphorectomy.

Dr. Thomas Savage, Birmingham.

The history of oöphorectomy, or Battey's operation, is now well-known; its facility of performance and its safety are completely established, and its justifiability in certain cases is also recognized. It is an operation which has had, like almost all new procedures, to encounter a large amount of hostile criticism. It is

very right that this should have been so, for, from the nature of the operation, it is one which one might believe to be easily capable of abuse, and one the indications for performing which may not always be clear to others.

In discussing oöphorectomy three considerations have chiefly to be borne in mind—viz., the safety, the facility, and the selection of cases likely to be benefited. As regards the safety, I think it may be said that, with the present results of peritoneal surgery in this country, a simple abdominal section, where nothing else is done, is an almost quite harmless proceeding, and that ligaturing the ovaries is a very slight additional risk. In a certain, not yet ascertained, proportion of cases the ovaries, one or both, are adherent, and attempts at removal of these must add also to the danger.

I take it that the abdominal method of operating will be the usual one, rather than the vaginal.

Many cases incapable of removal per vaginam could be readily accomplished per abdomen; while any case incapable of removal per abdomen would, I feel certain, fail per vaginam. I have had no personal experience of the lower method.

It has been considered to be a *sine quâ non* for the benefits to be derived from oöphorectomy that both ovaries should be removed; but there are some few cases where the disease is strictly limited to one side, and where I believe the removal of the affected ovary would be effectual. In suitable cases, such, for instance, as one of prolapse of the ovary only, I intend to put this plan to the test; for this purpose, I take it, the local symptoms should be very clearly defined and limited, while at the same time there is an absence of prominent general symptoms.

There can, I think, be no doubt of the benefit to be derived from oöphorectomy in the case of a bleeding or rapidly-growing myoma of the uterus, where means adopted for relief have hitherto failed, and the patient is gradually, though surely, losing her ground, with a prospect of a not far distant death staring her in the face—*i.e.*, if the operation is done before the menopause. By taking away the ovarian influence we also cause menstruation to cease, and thereby diminish, at all events, the activity of growth in the tumour, and so stay the downward progress. But more than this is done, for in many cases the tumour actually diminishes to a considerable extent, and I believe has been known to entirely disappear.

Though I myself have not seen so much as this, I have seen them very much diminish. In one of my cases, six weeks after the operation, the abdominal circumference was reduced by at least four inches, and the tumour was to eye and touch clearly smaller. A few weeks ago I saw the patient, and in her case the same result was most manifest.

The operation for the removal of myoma by abdominal section has hitherto been attended with such fatal results in most cases that it must be long ere it will be frequently performed. I have been fortunate, out of a total of nine cases, to have had six successful ones; but if I continue to perform the operation I can scarcely hope to keep up this at present high average of success. Oöphorectomy offers itself as a very safe substitute for many such cases.

Prolapse of one or both ovaries is a prominent condition for which the operation may be performed. I am of opinion that this state of things has not hitherto been considered sufficiently in the ordinary textbooks, only a very short space usually being given to its consideration. I believe it is a condition which will ere long be regarded more in the light of a disease of itself, than, as hitherto has been the case, as a symptom or accompaniment of other conditions. When it is present I presume there must be some amount of relaxation of the broad ligament, and probably an

increase in the weight of the ovary itself. So far as I have yet been able to observe, the position of the dislocated ovary is an inverted one—*i.e.*, the broad ligament is acutely bent, causing the superior margin of the ovary to look directly downwards. This acute bending of the broad ligament would lead in time to an interference in the vascular (venous) distribution, causing œdema, enlargment, and probably areolar hyperplasia. In such cases the prolapse would be primary—*e.g.*, as a result of flexion of the uterus, and the enlargement of the ovary follow as a consequence.

I have been led to associate the predominance of reflex neurotic symptoms with the shrivelled ovary and local pain as more frequently the accompaniment of ovaries which are abnormally increased in size.

The cases of ovarian dysmenorrhœa appear to be the most difficult to come to a sound conclusion upon. They are more frequently to be met with in single women, and are attended with so many vague and indefinite nervous symptoms, often mixed up with more or less of hysteria.

The effects of oöphorectomy in this class of disorders are well known to be not always immediate, and hence we must allow a certain time, say twelve months or so, before we can form a reliable opinion as to its benefits or otherwise in a particular case; and it is not difficult to conceive of the continuance of painful impressions and other abnormal sensations, even for some time after the cause which produced them has been taken away.

I do not wish to lose sight of the hysterical or emotional element in many of the cases, nor am I forgetful of the case from abroad, recently published, where a cutaneous incision in the abdominal wall was said to have the happy effect of curing a patient, who had been recommended, and had agreed to submit to oöphorectomy as her only hope. The sequel to this case of sham castration was given by Hegar, under whose care the patient afterwards came, not having been relieved by the operation.—See the *London Medical Record*, Feb. 15, 1881.

From time to time an instance may be met with in which the symptoms are entirely subjective—there being no objective symptoms that we can lay hold of. The patient may be, so far as our examination of her reveals to the contrary, a perfectly healthy woman, with the exception that she complains of intense pains. Careful watching will eliminate the hysterical or shamming element, and we have to take on trust the reality of her sufferings, the cause for which may be revealed by an abdominal section. Recently, Mrs. E. S. was sent to me by Mr. Bethel, of Bridgenorth; married four years, never pregnant until last autumn, when she aborted at seven months. She had an illness after her abortion which she calls "inflammation." Since this illness she has not been free from pain. The pain was at first worse in the right groin, and afterwards it came in the left groin, at the same time affecting the left breast. Rest in bed and remedies had no effect whatever in easing her, and her own statement of the persistent pain was all I had to rely on, as on examination nothing wrong could be detected. I decided, after a consultation with my colleagues, to make an exploratory incision, and I found each ovary and tube to be glued down to the pelvis, so firmly indeed as to require picking away. The tubes were wormlike, filled with inspissated pus, and sacculated.

I have commenced the operation for oöphorectomy forty times in all. In five of this number I was unable to do anything beyond the simple abdominal section, on account of the strength and character of the adhesions which were present. Of the remaining thirty-five cases, I removed both ovaries—*i.e.*, performed the com-

plete operation—in thirty-one. I removed only one ovary in one case, and in three others I was obliged to leave behind a part of one ovary at least.

In the first case I did, where I removed but one ovary, I think no good at all resulted from the operation. But of the remainder, with one exception—viz. No. 14, I am convinced that the greatest benefit has resulted. In the majority of instances it may be looked upon as a complete cure. I hope ere long to be able to publish the results of each case, so far as I can get them, but it is at present too short a time after the operations to speak of the cases with anything like certainty, and I can therefore only give the impression that has been formed in my own mind, which is one of the most unqualified praise.

It must not be forgotten, in estimating the advantages and disadvantages of oöphorectomy, its facility and its safety, that there must always enter into our calculations the elements of danger that will be likely to arise in a certain but unknown proportion of cases. I mean such dangers as a septic peritonitis from an unknown or preventable source; obstruction of the intestines, as from adhesion of the gut to the raw surface of the head of the stump; or even the very rare and remote danger of tetanus, which occasionally follows all operations, and seems more especially prone to be a sequence of ovariotomy, involving as it does the encircling of living tissue with a tight ligature.

DISCUSSION.

Dr. MARCY, Boston: I have yet seen only two cases where I have thought proper to do this operation. Both these were operated upon by the vaginal method, and recovered, but not very satisfactorily. One is of sufficient interest to briefly narrate. Mrs M——, aged 30, well until four years ago, when she had a severe labour, followed by pelvic inflammation and abscess; ever since she has been a severe sufferer. Menorrhagia very pronounced, and anæmia marked; great pelvic pain; uterus retroflected, but movable; both ovaries dislocated downwards, enlarged and always very sensitive. No treatment was of the least benefit. After careful consideration oöphorectomy was performed. Every stage of the operation was directly under the guidance of the vision. The right ovary had commenced to undergo cystic degeneration, and was bound down by adhesions; the right Fallopian tube was sacculated and adherent. Both were removed, as well as a small cyst of the broad ligament. The left ovary was removed, and had undergone much less marked changes. The operation was done antiseptically and the results have been observed for one year. Menstruation continues, but in much less degree. There is still pain in the left ovarian region. Aphrodisia has not been in any degree lessened, and the patient's general condition is much improved. She wears a pessary, which retains the uterus in position, and adds to her comfort.

Dr. PRIESTLEY, London: I desire to offer the thanks of the Section to Dr. Battey for coming to this country to lay this subject before his *confrères*, and for the very able and fair way in which he has stated his case. When I recollect the revolution which has taken place in reference to the propriety of performing operations entailing the opening the abdominal cavity, I am amazed. In my earlier life ovariotomy was considered as quite unwarrantable, and one authority proposed that the operator should be indicted for manslaughter. Medical men are often taunted with being slow to adopt innovations, but the favour with which Dr. Battey's proposal has been received, and the able imitators he has found, prove that medical men

are ever ready to adopt whatever gives fair promise of being useful. The conservative sense, which we ought all to posess, suggests a note of warning, lest one should be induced to perform the operation too early or too frequently, and before all other means are fully exhausted. It is so tempting to take a short cut to cure, rather than patiently persevere in less radical methods, which are attended with less hazard. In adopting Battey's operation for dysmenorrhœa, it must be recollected that it is a confession of failure, and in some sense reminds one of the Eastern physician, who, failing to relieve his patient, recommended that he should be decapitated. I may point out that the ovaries are liable to great variations in size, and, like the male testicle, may be almost as large as the fist, and yet subside again. The mere enlargement of the ovary within certain limits, therefore, must not be taken as indicating the operation. In cases of myoma attended with alarming hæmorrhage, the course is clearer, and one need have less hesitation about operating. If the able lead of Dr. Battey is followed, and his operation is resolutely regarded only as a last resort, practitioners are not likely to be far misled and relief may be afforded to many suffering women.

Mr. KNOWSLEY THORNTON, London: My experience has been small, and the results of my operations have been as various as the published records of others would lead one to expect. I find myself, therefore, in a doubting condition. One operation performed to check uterine hæmorrhage has been so rapidly and completely successful, that I have no doubt that there is a good field for the operation in this direction. The evidence of Dr. Battey and Dr. Savage is so conflicting as to the mortality of the operation, that I think we must wait for further information. The former gives it a mortality of 18 per cent., and the latter says that its safety is completely established. All my cases recovered, but I must say that those in which the operation was undertaken for the relief of ovarian pain, &c., have been somewhat disappointing; at best they are but partial cures. I think with the abdominal section performed antiseptically it ought to be a perfectly safe operation. I am surprised to hear Dr. Savage, one of our best and most successful operators, admit failure to complete the operation in 12½ per cent. of his cases; and if there is a risk of such experience being repeated, I think it must be a great blow to the future of the operation. The theory as to the abrogation of the function of the ovary seems to me to be hardly proved, and much that we have heard this morning rather tends to shape my belief in its correctness. I think the question of how much the change in the circulation has to do with the results obtained is a very important one; and my own belief is, that it has much more to do with it than any special physiological process.

Dr. MARTIN, Berlin: The removal of the ovaries should be undertaken but very rarely and only when absolutely indicated, because these organs should be extirpated only when all other means have failed, and because the operation is not only difficult but dangerous. I have performed it fourteen times, and I have hesitated to publish my experience at length until now, as I believe it necessary that the cases should be under observation for a lengthened period before the ultimate results can be accurately known. The conditions in my cases which called for operation were disease of the uterus, or disease of the ovaries themselves. I took out both ovaries by laparotomy seven times for fibromata, with satisfactory results in two cases, temporary improvement in one, and one was done three months ago, apparently with a favourable result. In two cases I removed the ovaries after having taken out

large fibroid tumours, with a view to arrest the growth of small fibromata left behind. In one case of œdematous bleeding the removal of the ovaries was without success. Twice I operated for malformation of genital organs with perfect success. In five cases chronic oöphoritis called for removal of the ovaries; the result did not prove satisfactory in all.

Dr. Heywood Smith, London: I wish to speak on two points—first, with regard to the question of the removal of one or both ovaries. It is of course a very important question, as upon it depends the child-bearing power of the woman. Yet, in spite of this, I am decidedly in favour of the removal of both ovaries, because the objective signs in some ovarian disease are so slight that the cursory examination of the ovary during the operation may fail to reveal a condition of disease that might at some future time call for interference. Then, on the question of the vaginal or abdominal section, though many distinguished operators have advocated the original operation, I must defend the abdominal section. In a case where there are no adhesions it is comparatively easy to remove the ovaries through a small opening in the vaginal wall; but in cases where adhesions exist—and their existence is not always easy to diagnose before operation—it is almost imperative that the operation should be performed by the supra-pubic method. And this is specially necessary where the adhesions are firm, for then it requires some firmness as well as delicacy and precision of manipulation in order to free the ovary from its bed.

Mr. Lawson Tait, Birmingham: I must express my gratitude to Dr. Priestley for the extremely just and logical manner in which he has stated both sides of this most difficult question. Dr. Priestley alluded to confession of failure in many instances; but this would have to be applied to all kinds of surgical practice if applied here. When a surgeon undertakes the treatment of a diseased joint, the moment he entertains the question of excision of the joint, or when amputation is performed, he not only confesses a failure, but he confesses one which involves serious mutilation of the patient—a mutilation sending him halting through life; whilst removal of diseased ovaries does not in any way interfere with the patient's personal appearance, or with ability to engage in any kind of active business or amusement; but if successful, it completely relieves her of terrible suffering, perhaps even may save her life. It is worthy of notice that the critics of this operation have subjected it to the very crucible of statistical examination, through which they dare not pass their own work. Those who speak of normal ovariotomy, and who are under the impression that in these cases they can with as much ease remove the ovaries of a woman as a sow-gelder can remove the organs of a young pig, are under a delusion, and evidently know nothing practically of the work. It is in almost all instances a difficult operation, sometimes one of exceeding difficulty. The percentage of incomplete cases is by no means a heavy one, for if the records of any other operation in surgery had been as completely kept as had those of the one now under discussion, it is perfectly certain that in many branches a very much higher percentage of incomplete operations would have to be confessed. Thus, in excision of the knee, I have been informed by surgeons who have performed this operation on a large scale, that it is by no means an unusual thing for them to begin an operation for excision, and then have to end it as an amputation. I am told that the percentage is probably as high as between forty and fifty. But it must be noticed that the proportion of incomplete operations in removal of ovaries is, in my own experience, rapidly diminishing; for I now can and do remove ovaries

which a year or two ago I should have left unremoved. A statement has been made that the ultimate results of such cases have not been published. To such a remark I would say that it is the fault of the speaker, and due entirely to his ignorance of the literature of the subject. I submit a long list of seventy cases, with five incomplete operations and six deaths, and show that some of the earliest of these cases, after the expiration of a sufficiently protracted period, have been published; and in the case of one which had been performed December 10, 1879, I appeal to Mr. Nason, of Nuneaton, who happens to be in the room, and whose patient the woman had been, as to whether or not it is a fact that the operation has completely relieved the woman from a life of absolute misery, and whether she is not now able to gain her livelihood as a nurse?

Dr. Bantock, London: I conceive the subject admits of division into two great classes—viz., the one including those conditions classed under the head of the neuroses, and the other in which the operation is performed for the arrest of hæmorrhage. I shall touch lightly on one division, for the reason that I am unable to bring any experience to bear on it. But if I have not had that experience which entitles me to offer an opinion in decided terms, I can say that the subject has occupied a great deal of my attention. First, I must say that as yet the records do not appear to me to be very satisfactory as regards those cases which may be classed under the neuroses. We find a few cases reported in which the result is said to be a permanent cure—though with regard to some of these it may be open to doubt whether a sufficient time has been allowed to elapse between the operation and the announcement of the result. In others the result is stated to be "good"—not even the little word "very" to qualify it. In a few cases no relief has been obtained; in no inconsiderable number the condition of the patient has been aggravated; and still further, death has resulted, and that too in a large proportion of the cases. Far be it from me to say that the operation already stands condemned; but I will say that it is even now passing through a very severe ordeal with considerable doubt as to the verdict. I have been informed of one case, not yet published, in which the result appeared for a time to be most brilliant, but it was only for a time, for the patient ultimately became worse than ever. For my part, although I have seen several cases in which this operation appeared to be called for, yet I have never been able to bring myself into that frame of mind in which I could urge the patient to submit to it. On a very different footing stands the operation of oöphorectomy for the arrest or relief of hæmorrhage, whether due to the presence of a fibroid or other chronic condition of the uterus. Of this I have had some experience, and that of the most favourable kind. I will mention the case of a lady over forty years of age, who, for ten years, had been the subject of severe hæmorrhage, resulting in the most profound anæmia. She had been confined to her bed or bedroom for four months previous to operation; the systematic use of ergot subcutaneously had failed to give any relief. Yet within two months she was able to drive out. The patient called on me a week ago, and her condition was one of almost complete restoration to health. In another case, a young woman, twenty-six years of age, upon whom all the resources of our art had been lavished in vain, has been restored to complete health. In this case the ovaries were very much enlarged; the left was prolapsed, and tender and painful, and there was chronic metritis. Time fails me to speak of the difficulties of the operation, difficulties far exceeding those of nine out of ten of the ordinary run of ovariotomies. In two cases the operation was undertaken for the arrest of the

wth of fibroids; in one with undoubted success, in the other with, at most, a ibtful result. With regard to the removal of the tubes, I think it quite a matter indifference.

Dr. WILLIAM GOODELL, Philadelphia: I have performed oöphorectomy eleven es, and have always begun the operation by the vaginal incision. But I ll hereafter give up that mode of operation, because twice I had to complete it the abdominal incision, and once I did not succeed in removing all of the rian stroma, which broke down under the traction of the forceps. I believe t the reasons for the greater mortality attending this operation than that from riotomy are that in the former the peritoneum is healthy, and therefore more nerable, and that the subjects of it are in a very low condition of health, and, in t, usually opium eaters. In two of my fatal cases the women were bedridden, aciated to the last degree, and constantly under the influence of this narcotic. hink further that, in another class of cases which have not been adverted to by previous speakers, this operation would be found successful, and that class is which embraces insanity with monthly exacerbations—in other words, ovarian anity. In one such case, which had been an inmate of several insane lums, I have been rewarded with a complete cure. In another, although the y was not cured, all violent symptoms ceased, and there is a prospect of her toration to health. In a third case, which belonged to that ill-defined border- d between hysteria and insanity, the lady, from her violent temper and suspicious ure, had become alienated from her friends. But the removal of her ovaries ught about a complete mental cure. All these cases showed a marked ovarian gin, otherwise the operation would not have been performed. If the operation not followed by a cure, the surgeon can console himself with the thought t he has brought about a sterility in a woman who might otherwise have given th to an insane progeny. In fact, I am not sure but that, in this progressive , it may not in the future be deemed sound political economy to stamp out anity by removing the ovaries of insane women.

Mr. SPENCER WELLS, London: The startling proposal of Dr. Goodell, to remove ovaries of all insane women, will not receive either support or the reverse, from own experience of the relations of ovariotomy to insanity. In one very re- rkable case, where an insane patient had been tapped many times—because it was t thought either morally or legally right to perform so serious an operation upon patient incapable of herself estimating sanely the risk she ran, when it was only formed at last in the forlorn hope of prolonging the life of a dying woman—the tient not only recovered good bodily health, but also became, and has remained, sound in mind as in body. In other cases, the result has been less satisfactory. e patient, who for nine years had been in various lunatic asylums, died a week er ovariotomy, under acute cerebral excitement. One, who was operated on in ucid interval, had again to be put to an asylum, where she still is. In two other es, members of insane families who had not previously shown symptoms of in- nity, were attacked with acute maniacal excitement during convalescence, but n recovered. In none of the cases had both ovaries been removed. Then, with re- rd to Battey's operation, my experience is very small, as I have only performed nce, and that was three years ago. The result was perfectly satisfactory to the tient. Full particulars of the case are to be found in the "American Gynecolo- al Transactions." I have not operated again, simply because suitable cases have

not been brought to me. I have advised its performance in two other cases. One was a young lady, whose clitoris I had removed, after consultation with Sir W. Jenner and Dr. Wilson Fox, in the hope of stopping such continual masturbation as led to mental imbecility. This operation, and subsequent applications of the actual cautery to the nymphæ and labia, having proved quite useless, Battey's operation was considered, but not performed, as the patient is a Ward in Chancery. In one other case of frequently recurring hæmatocele and pelvic cellulitis, Dr. Battey saw the patient, with myself, and concurred with me in advising the operation; but the patient and her husband were unwilling to give up almost the last hope of having a child—a very large property depending on the result. Thus, having in three years only operated once, and only having seen two other cases in which the operation had to be considered in consultation, I am surprised at the extreme frequency with which the operation is performed by Mr. Tait and Dr. Savage. I trust that more full and accurate details will be given hereafter of the conditions which led to the performance of the operation, and of its results, after a sufficient time elapsed, to enable us to estimate them fairly. I must add a word of caution to those who expect to find the operation a very easy one. It is really more difficult than an ordinary ovariotomy. There is much more danger of wounding intestine in the first incision, more liability to escape of intestine, and more difficulty in returning it after escape—and fully as much, or more, difficulty in removing the ovary and securely tying its pedicle, or attachments. With regard to the danger of removing both ovaries in cases of uterine tumours—as compared with the danger of amputating the tumours—more facts must be accumulated before a sound opinion can be formed, either on the question as to immediate risk to life from the operation, or as to the permanent benefit to the patient.

Dr. Pallen, New York: I have made three operations since December, 1872, with two fatal results, the successful issue being in the second case, and it is decided testimony in favour of the applicability of the operation for the relief of reflex disease in consequence of ovarian disturbance. The interest in the third case is that it was one of catalepsy, and the removal of the ovaries proved that the patient was thoroughly cognizant of her surroundings pending her affection, though powerless to move or speak, and that she knew all that had been done beforehand and after she passed out of the cataleptic stage, which she did two days after the oöphorectomy. With regard to Prof. Goodell's proposal to remove the ovaries of all insane women, I can only state that I have been the most strenuous of advocates for gynecological special treatment in that class of diseased women; but as yet the question is *sub judice*, because Dr. Putzel, the pathologist of the New York Female Insane Asylum, in more than a hundred post-mortems failed to find the least trace of disease in the uterus and ovaries. In a personal communication to me, Dr. T. Gaillard Thomas relates three cases of mania and insanity following oöphorectomy! I cannot agree with the statement of Mr. Spencer Wells, that oöphorectomy is more difficult in performance than is ovariotomy. It depends entirely on the mobility and freedom from peri-ovarian adhesions.

Dr. Matthews Duncan, London: I am in the disagreeable position of differing from the general tenor of the remarks of the speakers before me. I am not a decided opponent of oöphorectomy, but I regard it as an operation which is in the crudest experimental stage. I have been all my life in contact with women's

diseases, and recently I have often been consulted as to oöphorectomy, and have sanctioned it in one case only—my experience being so far like that of Mr. Spencer Wells—and this case has given me satisfaction in every respect. Dr. Battey has described as the essential feature or basis of the operation, that it produces the effects of change of life. Now, we have had this day abundant evidence from operators, that this result is not only not constant but, according to some, rather the reverse. Menstruation often continues, tumours continue to grow, sexual desire or pleasure continues, or is even increased. If the ground on which oöphorectomists stand is swept away, where are all their arguments? But I will not insist on logical conclusions in a practical matter. Despite this failure of the logical basis, the operation may yet be found to be a useful one. I regard the sufferings of the neurotic cases as habitually greatly exaggerated. Dr. Battey has spoken of these sufferings, from ovarian neuralgia, or dysmenorrhœa, or such like, as ending in death. I have never seen nor heard of any such case. Lastly, I will point out, with reference to oöphorectomy for bleeding uterine myomata, that a score of cases has been laid before the meeting; twenty-six cases, with five deaths. I know of no such bad results under any kind of treatment, and am sure that such disasters are unequalled in the history of the subject.

Dr. De Montbrun, Venezuela: I would like to propose to give to the operation the name of castration of women, in order to put in the minds of practitioners the moral gravity of his responsibility in performing a difficult and very dangerous operation, such as the removal of ovaries, and involving the sterility of women, and to teach the people that if the patients may escape from the danger of the operation, they will not be capable of reproducing themselves. The operation ought, or rather must, be limited to a very few cases, and in amenorrhœa and dysmenorrhœa must be completely banished, to prevent its being performed, as in one case I witnessed in one of the hospitals of London, in which a young woman, married only two years, was operated on to cure a dysmenorrhœa. The ovaries were perfectly healthy, and if the patient recovers she will be deprived of fulfilling the most blessed of her functions.

Dr. Battey, Georgia: I desire, in reply, Mr. President, to express my deep sense of the exceeding leniency and kindness with which this Section, and especially that eminently conservative body—the British Medical Profession—have received my suggestions and my work; and permit me to add also, Sir, the most generous hospitality with which they have, personally, grasped my humble hand. By way of reply, I have only to correct a misapprehension upon the part of my friend Prof. Matthews Duncan. I have distinctly held that the arrest of ovulation, not menstruation, as he misconceives me to say, is the essential object aimed at in the operation. I may also state, that the case of death which I have brought forward, and which Dr. Duncan has not seen in his own experience, is not of my own knowledge, but upon the authority of Dr. Grange Simons, in the "Transactions of the Medical Association of South Carolina," for 1872.

Dr. Thomas Savage, Birmingham: I wish to remark, in reply to Dr. Goodell, that the mortality after oöphorectomy ought to be very small indeed, certainly not more than three or four per cent., and that it is not due to the profound emaciation, &c., spoken of by Dr. Goodell, as in a very large majority of my cases the pulse and the temperature after the operation did not go up above 100°.

[illegible] Study of Hysteria and Hystero-Epilepsy.

By [illegible], London.

[illegible] subject which has often and repeatedly attracted the attention [illegible] cannot be said to have been thoroughly elucidated. [illegible] wide and important a subject, and in offering a co[illegible] the settlement of certain vital questions relating to hysteria, I [illegible] a preliminary.

In the first place, I desire to present a synopsis of the subject of hysteri[illegible] that I may be able more readily to define the position which my observa[illegible] in relation to the etiology of hysteria.

[illegible] the etiology of hysteria and hystero-epilepsy:—

I. Irritation of nervous centres.
  1. Directly impressed.
    a. Mechanically.
    b. By other media.
  2. Actually diseased.

II. Presence of peripheral irritation.
  1. In uterus.
  2. In ovary.
  3. Elsewhere.

[illegible] might be [illegible] this symptomatical view, either "centric" or "[illegible] [illegible] right. It is also evident that there is no absolute incompatibility in [illegible] two.

It [illegible] been a matter of belief—dating, in fact, from the Hippocratic e[illegible] that irritation in the generative organs plays a very important part in the pro[illegible] of hysteria.

Physiologists, who have most knowledge of the truth of this idea, have [illegible] up to the present time been able to define precisely the *modus oper*[illegible] and the exact nature of the irritation in question.

Assuming that the hysterical manifestations, convulsions, &c., seen in wo[illegible] are brought about by the irritations of the sexual organs, the initial diffic[illegible] presents itself as to which of the two organs, the ovary or the uterus, is mo[illegible] responsible.

So far as hysterical convulsions and hysterical symptoms are concerned, [illegible] held by some authors—Negrier and Charcot—is in favour of the view [illegible] the ovaries are the *point de départ*.

The observations of Charcot are most interesting, and the phenomena of hysterical paroxysm have been described by him, and still more recently by Rich[illegible] in a manner which leaves little to be desired so far as the outward manifestati[illegible] convulsions, spasms, anæsthesia, paralyses, temporary intellectual disturbar[illegible] &c., are concerned. And these delineations are also most complete in regar[illegible]

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* "Etudes Cliniques sur l'Hystéro-épilepsie ou Grand Hystérie." Par Dr. Paul Ric[illegible] Paris: Delahaye. 1881.

the manner in which the manifestations in question are capable of being modified or influenced by the action of *external* agencies. The ebullition, as it may be termed, has, in short, been pictured in a most graphic manner.

Circumstances have led me to investigate the various hysterical manifestations observable, from an etiological point of view. I had no predisposition to take any particular view of the matter, and it was only by repeated observation that I became convinced that the uterus is generally in a state of irritation in cases where these manifestations are observed; thus, in fact, confirming the more ancient theory of the subject. And I was induced to take this view of the influence (etiologically) of the uterus from the circumstance that in cases where the two conditions were conjoined—viz., uterine irritation and liability to attack—the attacks always appeared to cease on removing the irritation. In fact, experience revealed to me that in the course of treating the disorder of the uterus, the liability to hysterical attacks ceased. Further observation showed that the peculiar irritation productive of hysterical symptoms and attacks was always one and the same—viz., a flexed and distorted state of the uterus. Since I first became aware of this relation I have omitted no opportunity which has occurred to me for verifying and repeating the observation. Cases of this kind now referred to do not present themselves with great frequency; cases of marked hysterical paroxysms, so far as my experience goes, are not very common, but during the last ten years, during which I have been testing the matter in question, several instances have fallen under my notice; and as yet the facts I have collected are strictly confirmatory of the truth of the above generalization.

There appear to be two classes of cases:—

1. Those in which the attacks are induced primarily by some strong emotion—the reception of distressing news, a fright of any kind, a severe mental shock, &c. Here the operation of the causes is a direct action on the central nervous system, which in such cases may or may not be weakened in some way, and predisposed, or not, to be affected by an excitement acting from without. These cases are undoubtedly met with in practice, but they seem to be rather rare.

2. Those in which the attacks are induced primarily by a reflex disturbance from within, and quite distinctly so. This class of cases is numerically far more frequent than those classed in the foregoing list. They include cases in which the hysterical manifestations are severe, and more or less constantly liable to occur.

Now, the evidence which I have been able to collect, to me convincingly shows that the reflex irritation causing these attacks and other hysterical manifestations is an irritation having its seat in the uterus, and that the particular irritation most potent in producing the reflex disturbance is flexion of the uterus. This view is one which I expressed about twelve years ago.

In the course of my professional experience, I have only met with cases which seemed to be cases of hysteria produced in the reflex manner, and I have seen none in which hysteria of a severe character has been brought about emotionally. I do not deny the existence of the latter class of cases (certain of M. Charcot's cases, for instance), but it so happens that I have seen none. On the other hand, I have met with many cases coming under the former category, and in such cases the uterus was found to be the cause of the symptoms; the facts of the cases, the results of treatment and the whole phenomena of the cases in question, indicating in what has seemed to me a most unmistakable manner that this view of the case was a correct one.

What the precise nature of the condition of the uterus is which is capable of

giving rise to such remarkable manifestations is a matter of great interest. The results of my observations have led me to the conclusion that in these cases the uterus is in a condition of what may be termed traumatic congestion, by which is meant that the blood current is forcibly arrested in the tissues of the uterus. The common cause of such arrest in these cases is compression of the organ at its centre by the bending or flexion of the uterus. There occurs as a result acute congestion of the body of the uterus, which becomes aggravated by certain movements and diminished by others. Whence it happens that exertions capable of increasing the flexion are found to bring on the attacks or other hysterical manifestations, while, as a rule, rest and the horizontal position are equally potent in removing them or in preventing their occurrence.

The word "traumatic" seems suitable as explaining the nature of the congestion present under these circumstances.

The intensity of the traumatic congestion present in different cases appears to vary, but its main characteristics seem to be the same in all instances that I have observed. And the worst and severest cases of hysterical convulsions have been those in which the degree of traumatic congestion of the uterus was actually greatest.

There is another etiological moment present—viz., the compression of the nervous filaments of the uterine tissue at the precise spot where the flexion compression is greatest. When the uterus is forcibly flexed, such compression occurs.

I have here the model of a section of the uterus constructed in sponge.* The model is six times the normal length of the uterus, but the thickness of the walls and the due relation of the parts are carefully preserved. The model is constructed in order to exhibit the effects of acute flexion of the uterus on the uterine tissues. It will be observed that when the sponge uterus is bent so as to imitate the change of shape observed in acute flexion of the organ, the sponge is greatly compressed and squeezed together on the concave side of the bend. The model thus enables us to understand that the centre of the uterus is the seat of great compression in cases of flexion, which compression is increased by increase of the flexion and relieved by removal of the flexion. The sponge model also serves to illustrate the production of traumatic congestion, for the compression due to flexion is the cause of the interference with the circulation of the uterus.

Whether the traumatic congestion of the uterus or the flexion compression is the more important in giving rise to reflex hysterical manifestation, it does not appear to be easy to determine. Possibly both moments are important. And it may be that the presence of traumatic congestion operates in inducing hysterical phenomena by virtue of the compression of the uterine nerves in those parts of the uterus which are actually the seat of the congestion.

The accompanying drawings represent flexions of the uterus severe in degree. Fig. 36 shows a third degree of anteflexion of the uterus. Fig. 37 represents the uterus in a case of retroflexion in the third degree. The seat of the compression is principally the wall of the uterus on the concave side of the flexion.

I adduce in support of the views now enunciated a series of eighteen cases, arranged in chronological order, observed by me during the ten years from 1870 to 1880. I have observed other cases also, of which records have not been kept. The following series are all of which I have kept records. There are six cases in which the uterus was retroflexed and twelve in which anteflexion was observed.

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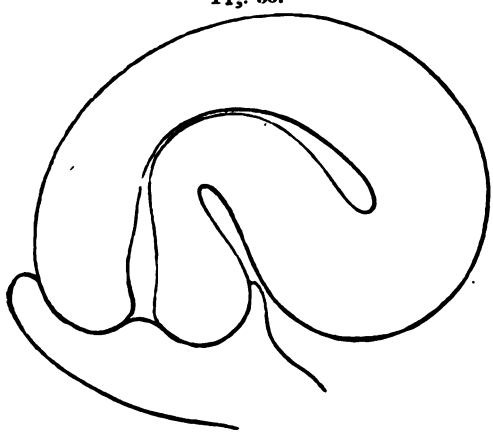
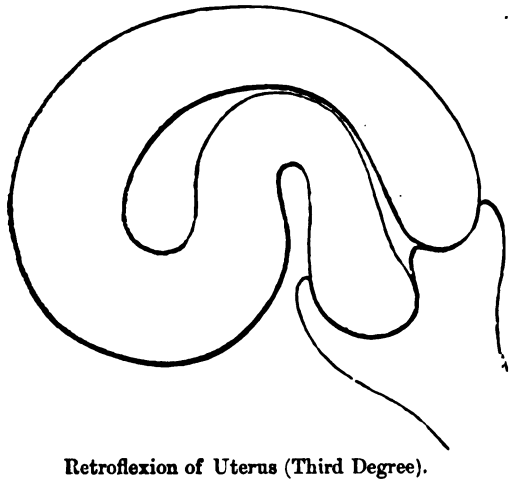
* The model was here exhibited by the Author, and the action of flexion on the uterus illustrated by its means.

Fig. 36.



Anteflexion of Uterus (Third Degree).

Fig. 37.



Retroflexion of Uterus (Third Degree).

CASE I.

Chronic retroflexion. Severe hysterical attacks.

Mrs. —— had been liable to frequent severe hysterical attacks, after which she usually remained in a state of quasi-insensibility for some time, ever since her first confinement, which occurred upwards of twelve years previously. Latterly severe sickness had occurred. The uterus was found acutely retroflexed. There was an absolute cessation of the hysterical attacks from the time the treatment of the retroflexion was commenced.

CASE II.

Acute anteflexion of the uterus. Almost entire suspension of menstruation for two years. Severe hysterical attacks.

The following is an abstract of the case which is more fully published in the third edition of my work on "Diseases of Women." The patient was single, æt.

nineteen, a dressmaker. Two years ago became attacked with "hysterics," at first severe, afterwards less so. On one occasion she lost her voice for five months. Has had lately a peculiar cough. Menstruation only once in the two years. While in the hospital had several severe hysterical attacks, strong convulsive action and attempts to beat her head on the floor, sometimes several in the day, and a peculiar cough resembling that observed in laryngismus stridulus. The uterus was found to be in a state of acute anteflexion. A stem pessary was employed. The attacks at once became less frequent. In a month she was made out-patient, fits ceased. Two months later pessary removed, complete cure and return of menstruation.

Case III.

Acute anteversion of the uterus. No menstruation. Severe epileptiform attacks.

For fuller particulars of this case see third edition of my work on "Diseases of Women." The patient was single, æt. seventeen; never menstruated. For ten weeks has had fits, as many sometimes as twenty in the day. In service since age of ten years. Pains in hypogastrium and frequent micturition for four months. Uterus anteverted. Sound easily introduced. Cradle pessary introduced. A month later the fits had become reduced in frequency and she left the hospital. Menstruation appeared about two months after commencement of treatment and was followed by a complete cessation of the attacks.

Case IV.

Acute anteflexion of the uterus probably of one year's duration. Convulsive attacks occurring frequently during that time.

The patient was married, thirty years of age, had one child four and a half years ago. Health tolerably good till one year ago. Six weeks nursing a sick child appears to have made her ill. The illness began with an attack consisting of slight loss of consciousness for a moment, then convulsions. Since that time has had attacks—two or three a day as a rule; the attacks last a short time, are not accompanied with loss of consciousness as a rule, and during the last three months have become more intense; menstruation had also ceased for three months, but has just occurred again once. The uterus was found acutely anteflexed. A cradle pessary was applied, the sound used to straighten the uterus. The attacks became at once reduced in frequency and intensity. During the first four days had altogether eleven attacks; during the succeeding ten days only five attacks; altogether she was under observation for seven weeks: the attacks latterly only occurred once in two or three days, and were very slight, while menstruation had occurred a second time rather profusely.*

Case V.

Retroflexion of the uterus. Hysterical attacks following exertion.

Mrs. ——, æt. nineteen, married fourteen months. Has had no child. Suffers from hysterical attacks, and her medical attendant believed her to be affected with retroflexion of the uterus.

It appears that four years before marriage she had a severe attack of scarlet fever, which left her so weak that she did not walk for one year, and then began with crutches. Since recovering from this attack she has been liable to what are termed hysterical attacks, following any exertion.

* Fuller particulars of this case in *Lancet*, August, 7, 1875.

Menstruation is profuse and too frequent. The uterus is soft to the touch, very distinctly retroflexed. A pessary was applied.

The patient completely recovered, and had a child two years afterwards.

Case VI.

Acute anteflexion. Severe hysterical attacks.

Mrs. ——, æt. thirty-four. Has been married fifteen years; has had no children. Menstruation always painful. Has had bearing-down for years. Ten years ago had St. Vitus's dance, not severely; but has occasional symptoms on and off, such as nervousness for an hour or two when excited. Six months ago had been nursing, for five months severely, and began to feel excessive bearing-down and strangury, became insensible for a week, and urine had to be drawn artificially. Had also acute pain in abdomen and hypogastric region, the difficulty in passing water continuing. She had severe convulsions at intervals during the time. Ever since this time she has had severe attacks of what are termed "strong hysterics" after any slight fatigue. Uterus in a state of acute anteflexion. A cradle pessary was applied. Relief. Later history not known.

Case VII.

Retroflexion of the uterus. Hysterical attacks.

E. J., a cook, single, æt. twenty-six. Three years ago was under treatment for uterine affection. Has suffered for some time now from hysterical attacks, which last for about twenty minutes, and during which she becomes unconscious. The last attack came on during the singing in church, and she had to be carried out.

Uterus retroflexed. A pessary applied. Cure.

Case VIII.

Slight anteflexion of the uterus. Attacks of convulsions.

Mrs. ——, æt. thirty-three, had four children, the last born six years of age. Six months ago had a convulsive seizure, following a course of nursing and over-exertion. The convulsions produced a kind of opisthotonos. She was conscious throughout, but could not move for ten days. Since this attack has occasional twitchings. No sickness. Easily tires from short walks. Uterus a little anteflexed. Sound enters with difficulty. Treatment, rest. Result favourable.

Case IX.

Acute anteflexion of the uterus. Suppression of menstruation. Severe hysterical attacks.

Miss ——, æt. twenty. Has always been weak and delicate. Menstruation began at twelve.

Two years ago she bathed in the sea just before the time for the period, and it did not consequently occur. She became very ill, and menstruation did not occur for three months. Since that time she has been liable to severe hysterical attacks, and to frequent threatenings of attacks. There was a further catching of cold five months ago, and the menstruation has not occurred since, with one exception.

The uterus was found very low down in the pelvis and anteflexed. A cradle pessary was employed. The hysterical attacks ceased, but the patient remained for some time in a weak condition. Finally, restoration to health. The hysterical symptoms did not recur.

Case X.

Retroflexion of the uterus. Hysteria.

Miss ——, æt. forty-one. Had a fall from a horse twenty years ago, and has been ill ever since. Treated for hysteria for a long time. It was discovered, nine months ago, to be a case of retroflexion, by Mr. Palmer, of Nayland, Colchester, who has nearly succeeded in restoring the uterus to its proper place, and she is now much better.

Case XI.

Acute anteflexion of the uterus. Severe hystero-epileptiform attacks.

Mrs. ——, æt. twenty-one. Married three years. Ill since six months after marriage. Is subject to severe hystero-epileptic attacks. These chiefly occur after sitting upright, as at meals. They are very severe, and the general disturbance is very acute.

The uterus is in a state of acute anteflexion and much tilted forwards. There is very great tenderness of the epigastrium and of the back, particularly at three special spots.

The flexion and displacement were treated by the sound and a cradle pessary. The attacks were relieved at once, and have not returned since.

Case XII.

Acute anteflexion of the uterus. Severe convulsive attacks.

Miss ——, æt. thirty-eight. Out of health one year. Had an attack of bronchitis, on recovering from which she had a succession of severe nervous attacks, on one occasion being for five or six hours unable to speak, move, or show any sign of life, but was all the time conscious. There were many other severe attacks. For three or four months could not sit up one hour, though she could walk a little. Has not improved the last three months. To quote the patient's own description:— "There is constant pain in the back, almost constant sickness or nausea, occasional violent retching brought on by walking or even talking. Any exertion of mind or body produces clinching of the hands and a horrid feeling all over the back and back of the head. Menstruation regular, but extremely painful, and inability to move at these times increased. Feels very often faint, and a sensation then begins in the brain. She feels that she cannot speak, and is very unlike herself. On recovering feels as if she had been some one else all the time, or as if she had two selves, one quiet and sane, the other idiotic."

Severe anteflexion of the uterus. Treated by a cradle pessary. Complete cure.

Case XIII.

Acute anteflexion of the uterus. Severe convulsive attacks just previous to menstrual periods.

Miss ——, æt. twenty-eight. Has suffered from severe convulsive attacks since menstruation commenced. These attacks appear generally just previous to menstruation. They have been considered due to disease of the brain.

The attacks are of the following kind:—The eyes become fixed on the ceiling, the teeth clenched, the back arched and rigid, the limbs also contracted and set. There is incapability of speaking, but the patient knows what is going on. The skin is deadly cold. The attacks last from an hour to an hour and a half. The patient was found to be suffering from acute anteflexion of the uterus. She was

reated for this by a cradle pessary and occasional use of the sound. After three months the attacks had become greatly lessened in frequency. Half a year elapsed before the patient was next seen. The attacks had disappeared. A slight sensation of faintness only was occasionally observed at times. A year later still free from attacks. The anteflexion of the uterus was difficult to cure in this case, but the final result satisfactory.

Case XIV.

Retroversion and slight flexion of the uterus. Convulsive attacks about menstrual periods.

Miss ——, æt. twenty-nine. Four years ago began to suffer from convulsive attacks which always came on about the second day of the menstrual period. She remains insensible about half an hour (once in two days) after the attack. Has had five attacks. Has had much exertion in lifting and nursing. Uterus markedly retroverted and a little flexed. Treated by pessary. Cure.

Case XV.

Acute retroflexion of the uterus. Severe hysterical attacks.

Mrs. ——, æt. thirty-eight. Has had no children. About one year ago began to have severe hysterical attacks, with screaming and much excitement. Occasionally every word excites the sensation of an attack coming on. Formerly could walk well. Walking power now very much more limited.

Uterus acutely retroflexed, extremely sensitive to the touch. Treated by the sound and by a Hodge pessary. One year afterwards she stated that she had had no more attacks, and was in all respects feeling quite well and strong.

Case XVI.

Uterus anteverted. Hysterical attacks.

Mrs. ——, æt thirty. Four children. Hysterical attacks and pain after exertion. Uterus anteverted, wearing a Hodge pessary, the overaction of which has produced anteversion of the uterus, or exaggerated it.

Case XVII.

Anteflexion of the uterus. Hysterical attacks.

Mrs.——, æt. twenty-four. Three children. Two years ago began to have hysterical attacks, with pains in the head, and dulness. Since last confinement, five months ago, the attacks are more frequent. The patient has a frequent choking sensation. She is obliged to stand a good deal.

Uterus low down, anteflexed; fundus close to symphysis pubis.

Treated by a cradle pessary. Cure.

Case XVIII.

Anteflexion of the uterus. Hysterical attacks and severe sickness.

Miss——, æt. thirty-three. Five years ago lifted a heavy weight, and fell ill in consequence. Two years ago began to suffer from sickness. The sickness has been almost incessant ever since. Dysmenorrhœa also of late. For the last four months has been subject to fits of insensibility. The head feels strange; she lies down and knows no more for some time—once for as long as twenty-four hours. When she

returns to herself has much aching of the jaws. Uterus very low down, larger and anteflexed. There is great tenderness over right ovarian region. Very severe and troublesome sickness almost constantly present. Treated by a cradle pessary. Great improvement, sickness subsided, attacks ceased. Pessary removed one year and nine months later, when patient seemed well. Five months later return of symptoms and re-employment of cradle pessary.

The cases above related, coupled with others which I have seen, but of which I possess no sufficiently good records, have induced me very decidedly to come to the conclusion that it is the uterus which is the seat of the irritation, which issues in the hysterical attack. The manner in which the attacks originated, the circumstances attending the subsequent occurrence of them, the relief, and in many cases the instantaneous manner in which the attacks ceased when the uterus was straightened and put into its proper position in the pelvis—these facts and observations, repeated over and over again, have forced this conclusion upon me.

The occurrence of hysterical paroxysms was, in the large majority of cases which I have witnessed and investigated, apparently brought on by some physical exertion. This is a most important circumstance. The importance of it arises from the following considerations:—When the uterus is in a state of flexion, either forwards or backwards, the act of lifting, or stooping, or over-walking, or standing, has the effect of intensifying the flexion; the uterus is pushed lower in the pelvis, and its curvature becomes exaggerated. This is a fact abundantly borne out by clinical observation. The result of increase of the flexion of the uterus is to increase the congestion; there is in such cases congestion to begin with, but the physical exertion leads to its very considerable aggravation, and when the aggravation reaches a certain point the hysterical attack appears.

On the other hand, by taking measures such as are adapted to prevent the aggravation of an existing flexion—that is to say, by keeping the patient in a horizontal position—the attacks are not found to occur, or, at all events, become much diminished.

Observation shows that the dorsal position prevents hysterical attacks due to anteflexion, but that the prone position is most effective when the case is one of retroflexion. These facts are most interesting. Out of the eighteen cases related, twelve were cases of anteflexion, from which it appears that the most common cause of hysterical attacks is presence of anteflexion of the uterus. One of the principal reasons why the mechanism of the production of the hysterical paroxysm has so long escaped recognition is, I believe, the fact that anteflexion of the uterus has, up to quite a recent period, been hardly allowed a place in nosology. I cannot stay here to explain this latter circumstance; but I take the opportunity of saying that, having for many years closely observed and investigated the mechanical diseases of the uterus, I have long been impressed with the grave nature and frequency of the symptoms to which this variety of distortion and displacement of the uterus is capable of giving rise.

I may be permitted, in conclusion, to make a few remarks on the ovarian theory as to the origin of the attacks, which has of late been so warmly advocated by Professor Charcot.

It is well known to gynæcologists that the ovary is sometimes found to be prolapsed, and can be readily felt in the Douglas' pouch. It is there subjected to great pressure and irritation, and much pain and suffering is found to be present in such cases. These cases would therefore be supposed to be of all others cases in which

hysterical attacks should occur, supposing that the ovaries are the principal point of origin. I do not deny that such dislocation of the ovary may cause hysterical attacks; but I have, at all events, not seen attacks of hysteria in such cases of dislocated ovary, unless accompanied also by acute anteflexion of the uterus. Retroflexion and dislocation of the ovary are not seldom associated.

Further, in the cases of hysteria above related, where flexion of the uterus was undoubtedly present, the ovaries were not found to be particularly sensitive, nor was there evidence of ovarian disease.

The fact that pain is frequently felt in the ovarian region in cases of hysteria, on which much stress has been laid by those who adopt the ovarian theory, is explained by the flexion of the uterus. Having made many observations on this subject, I am able to state that pain in the ovarian region is a very common symptom in cases of uterine flexion. It appears to be due to the fact that the flexion is generally a little to one side, the uterus not being usually bent directly backwards or forwards, but most usually a little to one side or the other.

Two series of facts described by Professor Charcot are adduced by him to support the theory that the ovary is the *point de départ* of the paroxysm in hysteria and hystero-epilepsy.

In the first place, Charcot states that pressure over the lateral hypogastric region has the following effect:—"Pressure there produces not only pain, but a sensation accompanied by all or some of the phenomena of the aura hysterica. Thus, methodical compression of the ovary determines the production of the aura, or sometimes even a perfect hysterical seizure."

In the next place, Charcot states that a more energetic compression is capable of stopping the development of the attack when beginning, or even of cutting it short when the evolution of the convulsive accidents is more or less advanced.

The method adopted by Professor Charcot to effect the more severe compression is as follows:—

"The patient should be horizontal in dorsal decubitus on the floor, or a mattress. The physician then kneeling on one knee, presses the closed hand, or fist, into that iliac fossa which he had previously learned to regard as the habitual seat of the ovarian pain. At first much force is required to overcome the abdominal muscles. Pressure then produces numerous and noisy attempts to swallow. Consciousness returns almost at the same time. Now the woman moans and weeps, says she feels relief, or that you are hurting her. By continuing the pressure two, three, or four minutes, you are almost certain to find all the phenomena of the seizure to disappear as if by magic. When the abdominal resistance is overcome, pressure by the two first fingers is sufficient."*

It may be desirable to consider how far the results of Professor Charcot obtained by pressure, as above described, over the ovarian region, are antagonistic, or the reverse, to the uterine theory above formulated, as to the cause of the paroxysm in hysteria and hystero-epilepsy.

The pressure employed by Professor Charcot is a very forcible pressure made in the hypogastric lateral region, calculated, first of all, to abolish the resistance of the abdominal muscles—a resistance considerable in many cases; and secondly, to produce a real compressing influence on the organs which lie in the pelvis. The incidence of this pressure, which is effected by the fist, or by an apparatus specially contrived for the purpose, is rather widely spread, and it is such that it must

* See New Syd. Soc. trans. of Charcot's "Lectures," p. 27.

almost of necessity affect not only the ovary, but first the uterus, and secondly the ovary. Doubtless when the resistance of the abdominal muscles is overcome, the pressure can be more particularly pointed on, or directed towards the ovary, or concentrated on this latter organ. But at the same time it is almost inevitable that the uterus should be greatly affected by this pressure, and must receive a considerable portion of it. Considering for a moment the operation of such pressure on the uterus, the effect might be different, according to the position of the uterus at the time. Thus, if the uterus were much anteverted, the result would, or might be, to push it still lower in the pelvis, and to increase the anteversion; but the action of the pressure would be further to express the blood from the uterine vessels. and to diminish any congestion of the organ existing at the time. If the pressure were made directly behind the pubic bone, the effect might, on the other hand, be such as to push the uterus backwards, and, in the next place, to drive the blood out of its tissues. A further effect of the pressure would be, in any case, to diminish the flow of blood to both uterus and ovaries alike, by the general action of the compressing power on the blood of the pelvis.

So far as I am able to judge, therefore, it would appear that the operation termed ovarian compression is really entitled to be denominated "uterine," quite as much, perhaps even more, than it is to be described as ovarian compression.

But this is not all. Professor Charcot states that slight pressure of the kind above described often brings on pain and symptoms of the hysterical aura—that is to say, the attack is capable of being brought on by slight pressure, and relieved by severe pressure. All this is quite in unison with the argument which I just advanced, for supposing a version or flexion to exist, the slight pressure above the pubes, such as Charcot describes, would undoubtedly at first intensify the displacement. The slight pressure would temporarily thus so act on the uterus as to induce the attack.

In conclusion, I would express my conviction that the escape from the indefiniteness of view, which up to the present time has characterized the various opinions entertained as to the nature of "hysteria" is to be found in the frank adoption of the term "hysterical" in its most literal sense, and that in the future the uterus will be held to be in the main responsible for those various manifestations and disorders denominated "hysterical."

The Curability of Uterine Displacements.

Dr. PAUL F. MUNDÉ, New York.

Finding that the textbooks either entirely omit all mention of the possibility of permanently curing displacements of the uterus by any of the methods in use, or give but vague statements on the subject, and, impressed with the importance of having some positive conclusions on this matter, both for the sake of the patient and the satisfaction of the physician, the author has analyzed the numerous cases of displacements which have come under his care (895), and arrives at the following conclusions:—

1.—Displacements of the uterus are permanently curable in the large majority

of cases only when recent, or when a complete tissue metamorphosis, as occurs during pregnancy and after parturition, takes place.

2.—Chronic cases (of more than a year's standing) are but rarely curable permanently, except occasionally under the last-named circumstances. Apparent cures reported by some authors and witnessed by many physicians, soon show themselves to have been but temporary.

3.—Pessaries form unquestionably the most practical, rational, and (temporarily) the most efficient means of treating uterine displacements. Cures are but rarely accomplished by them.

4.—Medicated, chiefly astringent, tampons, intelligently applied every day by the physician, give the best chances for permanent cure. This is particularly true of prolapsus, but holds good for all forms.

5.—Electricity locally applied deserves more extended application.

6.—All methods should be persevered in for months and years before success is to be expected.

On the Influence of Uterine Disorders in the Production of numerous Sympathetic Disturbances of the General Health, and Affections of Special Organs.

Dr. Arthur W. Edis, London.

A practical acquaintance with the diseases of women justifies the statement, I think, that where there is any latent or hereditary tendency to neurosal affections, the presence of uterine disorder constitutes an important factor in the production of migraine, megrim, or sick-headache. I am fully aware that numerous conditions met with in patients suffering from uterine disorders may conduce to this end. A patient with a bulky, inflamed, retroflexed uterus may have her powers of locomotion so seriously interfered with, and her general health so deteriorated by the menorrhagia and constant discomfort, want of refreshing sleep, aversion to food, constipation, &c., as to materially impair the tone of the nervous system, and thus call forth any latent tendency to neurosal affections which might not otherwise have occurred. Still, so long as the uterine condition is overlooked or disregarded, treatment directed specially to the relief of the sick-headaches is of little or no avail. I have seen repeatedly the most inveterate sick-headaches gradually lessen in frequency and severity, and ultimately disappear entirely, on the removal of some uterine disorder which had not been even suspected until a local investigation revealed it. It is a curious fact, which has been observed by many, that patients throughout the period of pregnancy remain perfectly free from the headaches which invariably return after delivery, often with renewed severity—thus, as Dr. Liveing remarks, lending additional support to the general impression that there is some real connection between the attacks and the catamenial function.

In young women, just when the menstrual function is becoming established, how often do we find nausea, and even vomiting, with possibly an absolute distaste or disgust for food, so much so as not infrequently to seriously impair the general health, or even to develop any latent tendency to consumption, due to some marked

flexion of the uterus. This, as Dr. Graily Hewitt has shown, produces a species of strangulation of the uterus, and interferes materially with the circulation in the organ.

The distressing nausea and vomiting, so often noticed during the early months of pregnancy, is frequently due to the combined effects of the increasing distension of a congested uterus, associated with flexion of the organ, to granular erosion of the cervix, or some other morbid condition.

Cases could be cited where every conceivable remedy had been tried, including soda, bismuth, oxalate of cerium, belladonna, ipecacuanha, creosote, nux vomica, morphia, lime-water, lemon-juice, ice, &c., without allaying the sickness in the least degree, and where the application of the nitrate of silver, or other similar agent, to an intensely granular cervix uteri, the insertion of a morphia suppository in the vaginal cul de sac, or the adjustment of a suitable pessary to support the uterus and obviate any flexion, has at once arrested the nausea and allowed food to be retained. In severe cases Dr. Copeman's plan of inserting the tip of the finger just within the cervix, so as to dilate slightly the os uteri, produces a similar result.

Epilepsy depending upon uterine disorder is a well-recognized form of the malady. It was for this variety that the late Sir Charles Locock introduced the use of bromide of potassium as a remedy, and the increased experience of succeeding practitioners has fully confirmed the value of the drug.

Goodell refers to cases of epilepsy which seem to come wholly from the sexual organs—cases with an ovarian aura, so to speak. The fits begin at puberty, generally last through life, and end in impairment of the mind. Often the first convulsion is ushered in by the first menstruation, and ever afterwards it is around ovulation as a storm-centre that future eclamptic attacks revolve. In very many of our asylums incurable patients may be found whose mental infirmities seem to depend wholly upon the act of ovulation, or at least to become greatly exasperated by its recurrence. Dr. Sutherland examined carefully into the menstrual history of upwards of five hundred inmates of an asylum, and tabulated the following general conclusions :—

In epileptic insanity the fits are generally increased in number, and the patient frequently becomes excited at the catamenial period.

In mania exacerbations of excitement usually occur at the menstrual period, and a state of intense excitement is almost continuous in patients suffering from menorrhagia.

Esquirol long since pointed out the influence of the painful evolution of puberty, pregnancy, parturition, and the cessation of menstruation at the climacteric period as the determining causes of insanity.

It is a question whether Battey's operation for removal of the ovaries would not be perfectly justifiable in many of these cases.

It is a curious fact that in the event of pregnancy occurring the epileptic seizures disappear during its course, recurring at the time of, or shortly after, parturition has taken place.

Neuralgia, often of a severe and intractable kind, is in many cases due to some uterine disorder. Dr. Barnes has shown clearly the relation between these two conditions. Excessive secretion from the uterus, whether in the form of menorrhagia or leucorrhœa, deteriorates the general health, degrades the quality of the blood, impairs nutrition, and lowers the tone of the nervous system, so that the nerves become keenly sensitive to external impressions of cold, and to what are called the

ıpathetic impressions brought from internal organs, neuralgia being the ex-
ssion of the culmination of these conditions.

Mastodynia, or pain of a dull aching character in the left mammary region,
ling to the supposition that the patient is suffering from heart disease, is com-
nly met with in patients the subject of dysmenorrhœa from ovarian or uterine
order. Numerous cases of this description could be cited where much needless
riety had been caused by attention being specially directed to the heart when the
aries or uterus were alone in fault.

Pain in the right hypochondriac region, due to some uterine disorder, is not
frequently mistaken for congestion of the liver. Patients have been invalided
ome from India, supposed to be suffering from hepatic congestion, with threaten-
g formation of abscess, the symptoms being in reality due to some uterine
ıflammation or displacement. A few weeks of appropriate uterine treatment
ffectually removed all traces of the presumed hepatic mischief, when several
ionths of treatment directed specially to the liver had failed completely in afford-
ıg relief.

Asthma, when occurring in females, has frequently been noticed associated with
terine derangements. Attacks of spasmodic asthma are more likely to occur
the menstrual epochs, and where any functional disorder exists: the attacks
otably diminish in severity when a healthier condition of the uterus has been
tablished.

Chorea, although essentially a disorder of childhood, is yet liable to be repro-
ıced later on in life from dysmenorrhœa, or during the course of pregnancy, when
e affection proves most difficult to deal with.

The influence of uterine disorders in the production of inflammatory and other
nditions of the eyes, is well recognized by most oculists, but does not seem to have
et with that general recognition which the importance of the subject demands.

Numerous French authors have recorded well-marked cases of amblyopia, or
naurosis, due to optic neuritis or retinitis, dependent upon uterine disorders, and
here the affection of the eyes undergoes a marked aggravation at the recurring
enstrual epochs, or at the climacteric age.

Disturbances of the menstrual function, such as amenorrhœa, dysmenorrhœa,
ucorrhœa, and the climacteric period, hold the first rank as exciting causes of
eflex asthenopia.

Soelberg Wells noticed that the insufficiency of blood supply, which gives rise
the anæmic amblyopia, is often due to some excessive discharge from the
terus, to a prolonged and very exhausting confinement, or to over-suckling.
oss of sight may come on rapidly from uterine hæmorrhage, leading to complete
indness in the course of a few days, affecting both eyes, and proving incurable.

Dr. Dennis alludes to spontaneous irido-choroiditis occurring in women at the
imacteric period, at the time when the catamenia commence to be irregular, there
ing a certain correlation between the condition of the uveal tract and that of the
erine system.

Retinal asthenopia, due to hyperæsthesia and irritability of the retina, accom-
nied by hyperæmia of the optic nerve and retina, is frequently met with in
ble, nervous, and excitable females, where the menstrual function is disordered;
e ocular phenomena ceasing only when the regularity of the catamenia has been
ected.

Aphonia, spoken of in many instances as hysterical aphonia, is more frequently
pendent upon disordered uterine function than many are aware of.

Examination of the larynx shows no evidence of any pathological lesion of that organ. Galvanism may effect a transient relief to the aphonia, but unless the uterine disorder be recognized, and dealt with, no permanent restoration of the function of the larynx is established. So evident is the mutual dependence of the one condition upon the other, that even when appropriate uterine treatment be adopted, relapses of aphonia occur at the menstrual periods, until the local disorder has been overcome.

A curious spasm of the glottis is noticed frequently in patients the subject of dysmenorrhœa, more especially in cases of retroversion and retroflexion complicated with congestion of the uterus.

In some instances it is described as a mere sensation of choking, in others as if the throat were tightly gripped by a hand, amounting almost to a feeling of strangulation. These symptoms I have repeatedly seen relieved at once by reinstating the uterus in its normal position and sustaining it there by means of an appropriate support.

Instances are not infrequent where laryngeal phthisis, mediastinal tumours, thoracic aneurism, or other serious organic malady has been suspected, when the symptoms were in reality due to some uterine affection, and disappeared rapidly when the uterine disorder was relieved.

Barnes has pointed out the fact that paraplegia is in some cases met with as a reflex phenomenon in connection with some uterine affections, such as imperfect involution, catarrh, hæmorrhages, and, most frequent of all, retroversion or retroflexion of the uterus. When these are relieved, the paraplegia often disappears in a most rapid and extraordinary manner, showing clearly the relation between the two affections.

Palpitation of the heart, with mitral bruit, dyspnœa, œdema of the feet, and a general condition of anæmia, due entirely to menorrhagia, is very liable to be mistaken for advanced cardiac mischief. In these cases it is especially important to ascertain the condition of the pelvic organs, as the menorrhagia is too often regarded as an evidence of mitral disease, when in reality the hæmorrhage is the cause of the palpitation and other symptoms of cardiac mischief, relief to these being obtained when the menorrhagia is checked.

Perfectionnement dans la Construction et dans l'Application de l'Appareil pour le Redressement des Déviations Utérines.

Dr. Verrier, Paris.

Après avoir rapporté les travaux des auteurs anglais, allemands et américains qui s'étaient occupés des diverses sortes de postures à donner à la femme pour le redressement des déviations de l'utérus non-gravide, M. Verrier examine dans son travail les avantages que l'on peut retirer d'un appareil à l'aide duquel le médecin pourrait non-seulement donner à la patiente la position désirée, mais surtout obtenir le redressement de la malade sans la moindre secousse pour elle ni le moindre effort de sa part, ni de celle de l'opérateur.

Expliquant le mode de suspension anatomique de l'organe dans le petit bassin il fait comprendre l'utilité de son appareil pour redresser les déviations. L'on peut dire d'une façon générale que toute déviation cède à la manœuvre et à la

position, grâce à la simplicité de l'appareil, sauf bien entendu les cas où il y a des adhérences pathologiques de l'utérus avec les organes voisins. De même il reconnait que le redressement ne se maintient pas sans un bâton intra-uterin dans les flexions de l'utérus.

Les cas auxquels convient l'appareil de M. Verrier, sont:

Les antéversions.

Les rétroversions.

Les chutes ou prolapsus.

Pour la construction de l'appareil il a d'abord employé des échelles à ressorts et un ou deux coussins.

Puis il a fait construire un appareil fixé dans le plafond d'une chambre avec des ressorts à boudins.

Enfin il a fait construire un portique mobile et consacre un chapitre à la description de l'appareil perfectionné.

Quelques gravures dans son travail font comprendre le mécanisme de l'appareil qui, tel qu'il est, ne saurait être remplacé par des facteurs à mécanique d'aucune sorte.

Il renvoie, pour des statistiques, à une communication qu'il a faite à l'Academie de Médecine de Paris, le 18 Mars, 1880.

On the Mechanical Treatment of some of the Displacements and Diseases of the Uterus.

Dr. R. Beverly Cole, San Francisco.

Without entering into an elaborate description, giving the etiology, symptomatology, varieties, degrees, or treatment in detail of the displacements of the uterus, all of which are familiar to gynæcologists, and to most general practitioners, I propose to deal with this class of derangements, at this time, simply with reference to their mechanical treatment, whether complete or auxiliary.

From time immemorable, so to speak, pessaries of great variety of forms and material have been used in the treatment of displacements of this most important organ—the uterus; but Dr. Hugh Hodge, of Philadelphia, was the first to properly appreciate the true principle of shaping and adapting the instrument to the axes of the vagina and straits of the pelvis.

Until his work, "Diseases of Women," was given to the profession, no instrument was extant that fulfilled the indications in this class of cases.

The Hodge pessary, so familiar to us all, has however undergone many modifications, and the reasons for this are easily explained.

No gynæcologist who has enjoyed a large practice has failed to encounter cases in which the Hodge pessary, perfect as it is, has not fallen short of meeting every indication; hence he has felt himself not only warranted, but called upon, to so change the original shape as to enable him to adapt it to the case in hand.

In 1862, conceiving the idea that the pessary would be better retained in position by depressing its sides, so that the circular muscular fibres of the vagina

would apply themselves to the centre of the instrument, and thus fix it, and obviate the slipping that occurred when the original was used, I gave to the profession the results of my ideas and experience. And later on, in 1863, discovering that in certain cases, particularly if the instrument happened to be a little too long, the inferior bar had a tendency to compress the urethra, and give rise to trouble in the act of micturition, I depressed this portion also, which entirely overcame the difficulty.

On visiting London, however, in 1864, and meeting my friend, Dr. J. Marion Sims, and comparing notes, he assured me that he had adopted the same expedient; and I, therefore, never attempted to establish my claim to the improvement.

Before and since these modifications, as we have already said, many others have been made by different gynæcologists. Among those who have given special attention in this direction, I will mention Prof. James Y. Simpson, of Edinburgh; Dr. Albert H. Smith, of Philadelphia; Drs. T. G. Thomas, and Montrose Pallen, of New York, each from his standpoint either taking from, or adding to, the original instrument of Hodge in accordance with his peculiar views as to improvement. The former of these gentlemen was in the habit of modelling his instruments in gutta-percha for each individual case, which plan has its advantages, inasmuch as it enables the practitioner to take cognizance of the peculiarities of indications, and adapt, or model, the instrument accordingly.

This method consists in simply softening the refined gutta-percha by placing it for a few minutes in hot water, and when of a doughy consistence rolling it on a table, or other smooth surface, until of a proper thickness, then bringing the two ends together, so as to convert it into a ring. This done, you can give it any curve or curves required, and, while holding it in proper shape, immerse it in cold water, which immediately hardens it, and gives you the instrument perfect.

Dr. Smith adopted the special features of both the Hodge and the Simpson pessary, producing, in some particulars, a superior instrument to either, and yet not without its faults. But, as has occurred to each of us, cases present themselves from time to time in which neither of the instruments extant seem to fulfil the requirements, and of these there is a class that perplexes and annoys us more than all others; it is that which is attended with great sensibility, due ocasionally to a neurotic condition, but more frequently to a subacute or chronic inflammation of the parenchyma of the organ itself or the tissue surrounding it.

Under the latter condition no one doubts that we should tarry before resorting to mechanical supports, and address our remedies to the end of subduing the inflammation; and yet, when this is accomplished, for many months the tenderness is liable to continue, while the displacement contributes to its maintenance; therefore, we consider it to be of the first importance, whether the case be one of neurosis consequent upon retroversion, or an inflammation, subacute or chronic, due to the same or other cause, that we should replace and retain in position the displaced organ.

To meet this indication and avoid pain and irritation, we have devised and used for many years the instrument here shown (Fig. 38); it is simply Smith's pessary, shorter than usual, with the upper almost angular curve or arm so straightened as to conform merely to the curve of the floor of the vagina, while springing from the lateral bars we have two segments of watch-spring, supporting at their upper extremity a bar of hard rubber, which is designed to rest in the fornix of the vagina and uterus posteriorly; in a word, the instrument is so constructed as to

be of the general form of Smith's, only less angular and provided with two springs, which give it an elasticity that enables the wearer to tolerate its presence without pain or inconvenience.

Fig. 38.

Modified Smith's Pessary.

All of us have observed that in the mechanical treatment of *anteversions* and flexions, whatever be the cause or attendants, we are much more liable to encounter this extreme tenderness or sensibility that has been described, than in *retro*-versions and flexions. This increased sensitiveness is in many instances confined to the bladder, through which the pressure in the employment of all mechanical supports must be exerted upon the uterus; hence any fixed support or point of pressure, such as is furnished by all anteversion and flexion pessaries that have fallen under my observation, is unbearable. And what applies to the bladder will also hold good as to the uterus itself.

Suppose that from long-continued displacement irritation is set up, aided as it is liable to be by many other factors; or from the presence of a small fibroid in the anterior wall of the uterus the organ is bent forward and its fundus carried downward; the structures are liable to become the seat of great irritability and tenderness, and the sympathy of contiguity becoming great, there is no relief except through mechanical support, so adjusted and directed as to secure the elevation of the fundus of the displaced organ from exerting pressure on the bladder; and a replacement of the uterus.

In order to accomplish this most desirable end, I have devised this instrument, Fig. 39, which in construction is similar to the retroversion, except that the direction of the watch-springs is reversed, and the ordinary Smith pessary is employed as the bed or support of the springs, without any modification of the superior arm or angular curve of the original instrument. I should have mentioned before, that the springs in both of my instruments are coated with rubber and baked, so as to render them imperishable. After nearly twenty years of experience in the use of this and the original instrument from which this has been taken, and which was made much more clumsily of silver, I have no hesitation in saying that while it may not, and does not, in some cases, answer the purpose, yet it is far, very far, the best I have either used or seen used.

Fig. 39.

Modified Smith's Pessary.

As a rule it can be well borne by the most sensitive uterus, and nervous patient, giving great relief and comfort, and serving as an invaluable auxiliary in the treatment of these most troublesome and common of all the disorders of women.

The next subject to which we invite your attention for a moment, although not falling exactly within the scope and limit of the title of this paper, is one upon which our authorities have so little to say, that this is my apology for alluding to it here. I refer to displacements of the ovary.

Whilst it is possible for this organ to be dislodged, so to speak, from its natural position in any direction, a mere glance at the relation it holds to the broad liga-

ment will serve to establish the fact that this displacement is most liable to occur backward and downward into the cul-de-sac of Douglas.

When in this position it may, as a rule, if healthy, be easily detected as a small ovoid body about the size of an almond floating or loose (unless fixed by cellular inflammation), occupying in part the space behind the uterus high up, and above the fornix of the latter organ and vagina. Retroversion, and even flexion, is extremely liable to accompany it, and, in my judgment, constitutes its most prolific cause.

When detected, it will be found sensitive to the touch, and if at or immediately before the menstrual molimen, considerably enlarged.

In treating these cases, the practitioner is destined to encounter great difficulty; the only immediate relief is in mechanical support, and this is best effected at first by placing the patient in the genu-pectoral, or knee-chest position, through careful digital examination determining if there be adhesions between the displaced organ and the surrounding tissues, or if indeed there is present (as in a majority of cases there will be) retroversion or flexion; making clear your diagnosis, push up the ovary as far as the finger will reach, and if displacement of the uterus is present, this organ with it until it has resumed its normal position; then there should be a pledget of cotton so packed at the upper extremity of the vagina anteriorly to the cervix as to push this portion of the organ backward towards the concavity of the sacrum, and thus carry, which this accomplishes, the fundus upward and forward; the first pledget should be followed by another and another, until the vagina is reasonably filled, removing these applications daily, whilst the patient is in the same position—viz., knee-chest, washing the vagina with carbolated warm water, and using fresh pledgets. Continue this treatment so long as any irritability of the ovary is present, and then introduce the retroversion spring pessary we have described, selecting one the superior bar of which is very thick, say half an inch, so as to have it occupy fully the space behind the uterus, and thus prevent the re-displacement of that organ and the accompanying ovary.

This treatment should be supplemented by a tonic constitutional course, and I know of none so potent as the "rest treatment" suggested by Dr. Weir Mitchell, which consists, in brief, of placing the patient in bed, and in these cases keeping her as much as possible in the prone or face position, giving iron in some of its forms, malt, and light but very nutritious diet, thus increasing the deposit of fat in all of the tissues, particularly the cellular, furnishing a packing which will materially aid in maintaining the organs in their normal position. This "rest treatment" should be continued for a period of at least two months, and will be greatly assisted by the daily application, for thirty minutes, of the Faradic current, one pole of the battery being applied over the sacral region, the other over the abdominal and pelvic regions. At the expiration of two months, exercise and douche bathing should be prescribed, *inductively* as I choose to call it. To speak in detail, every morning on rising the patient should be required to sit on a stool in a bath-tub, with warm water enough to cover the feet, a towel wet with cold water folded and applied to the head, the body inclined forward, elbows resting upon the knees, while the nurse, or female attendant, pours a small stream of water (from a pitcher), which shall have an ascertained temperature of 84 degrees for the first day, 83 degrees for the second day, 82 degrees for the third, and so on—reducing the temperature one degree daily, until the natural standard of the water is finally reached.

This stream should not be larger than the small finger, and directed along

the spine from the nape of the neck to the sacrum, while the pitcher should be held at a height of three or four feet from the patient, and caused to be swayed to and fro, that the water may fall on every part of the spine successively. On the first day it should be continued for five seconds of time and no more, lest it exhaust the patient; the second day it should be given for seven seconds, the third for nine, and so on, increasing the duration of the bath two seconds each day. After the bath, friction should be instituted by means of coarse towels, including every part of the body, particularly the spine, and continued until the whole surface becomes reddened, and perfect reaction has been induced.

The patient should now dress quickly, drink half a cup of cold water, eat a small crust of bread or a cracker, to stay the stomach, and take a walk, the first day of half a block, and return; the second day, three-quarters of a block; the third day, one block; and so on, increasing the length of walk one-fourth of a block* daily.

On returning she should lie down on a lounge for twenty minutes or more, and then eat her breakfast.

The next condition of the uterus of which we shall speak is one which, though met with occasionally, either escapes detection, or is considered of too little importance to receive attention at the hands of authority. I allude to *double curvature of the cervix uteri*, consequent upon *atony* of the organ and its annexæ.

As we announced at the beginning of this paper, we do not design entering into the etiology and symptomatology of these troubles, but merely to call your attention to the fact of their existence, and perhaps, in this instance, to the diagnosis, whatever the symptoms leading to physical examination be. On introducing the finger per vaginam, the os uteri will be found looking forward, the cervix elongated and resting on the posterior wall of the vagina, and so far as a diagnosis can be made through this medium, the case is one of prolapsus with curvature. When the speculum is introduced and the sound employed, the latter will be arrested in its progress at a point about one-half to three-quarters of an inch from the external os; by changing the direction of the handle, either depressing or elevating it, according to the judgment of the operator, the instrument makes another advance for the same distance, and is again arrested; changing for the third time the direction of the sound, and perhaps reversing the position of the concavity of the instrument, it will finally pass the internal os, into the cavity of the body of the organ.

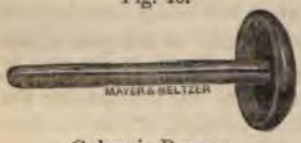
This class of cases was first described by me in the *Medical Press* of San Francisco in 1859 or 1860, since which we have met with this condition at least twenty or thirty times, and, as has been said, although it must have been met with by others, I have no knowledge of any written authority on the subject. The treatment of these cases consists in tri-weekly introductions of the sound or stiff English elastic bougie, as large as the canal will admit, assisting its introduction by straightening the organ either by pushing the fundus up with the index-finger of the left hand, a sponge-holder, depressor, or other such means, or drawing the organ down with a volsellum forceps through a Sims' speculum, allowing the bougie to remain ten or fifteen minutes each time, which, being a foreign body, acts as an irritant, inviting an afflux of blood to the parts, inducing active congestion, and through the distension of the vessels overcoming the curvatures, rendering patulous the cervical canal, and relieving the dysmenorrhœa that always accompanies the case.

* A block is about five hundred feet.

While this treatment with the sound frequently accomplishes the end in view, a much more reliable means is the "galvanic intra-uterine pessary," first introduced to the profession, so far as I know, by Prof. James Y. Simpson, of Edinburgh. This instrument consists of a staff, the upper half of which is zinc, while the lower half is copper. The supposed action of this instrument is as a small "galvanic battery," inducing a feeble current which, in its effect upon the uterus, as you can well understand, is excitant, attracting blood to the part and imparting tone to the organ; but, as it is well known to all who have given study to the subject of "galvanism" and electricity, the current always takes the shortest route from the zinc-plate or generator to the copper or keeper; hence, as the staff is divided, or rather, is united in the centre at its short diameter, the current amounts to but very little.

An improvement upon this instrument I made some years ago, which I have found to render the pessary (as a battery) much more powerful and efficient; it consists, as I here show you, in running the plates of zinc and copper longitudinally, separating or insulating them by the interposition of a thin sheet of hard rubber; by this arrangement we obtain action along the entire length of the staff, instead of, as in the other instrument, at one small point, and consequently it must be more effective (Fig. 40).

Fig. 40.



Galvanic Pessary.

This instrument is for the most part easily introduced by drawing the uterus down through the Sims' speculum, which straightens the canal, and when employed should, for the purpose of cleansing it, be removed at least once a week.

During the use of the galvanic pessary it is needless for me to say that a properly directed constitutional treatment, as in most uterine troubles, is indispensable.

Before leaving the consideration of this little and, as I think, invaluable instrument, allow me to trespass upon your time briefly, while I describe another condition, in the treatment of which I assert it has no equal, and in which, so far as my experience goes (it having been large), it never fails. This is a broad assertion to make, but I feel that my observation justifies it. I speak of *super-involution*, or that condition which sometimes follows delivery at full term, in which the physiological process through which the enlarged uterus is absorbed and restored to its original and normal state is carried too far, and atrophy results.

These cases, in my experience, have been invariably attended with cachexia, and an indefinite suspension of the menstrual function. So frequently have I met with these cases that now, when a woman consults me with the following history—namely, that she was delivered at full term of a healthy child (at a period variously remote, from one to many years), and that her recovery, or "getting up," was entirely satisfactory, and that since she has weaned her child no return of menstruation has occurred, and that she is growing weak, pale, or anæmic, with cold hands and feet, and a tendency to hysteria, no unusual indisposition having expressed itself during the time, I always suspect, and am usually right, that she is the subject of super-involution. An examination and the employment of the sound soon determine the fact.

The treatment of these cases consists distinctly in the persistent use or employment of the "galvanic intra-uterine pessary," beginning with one of such length as will agree with the measurement of the uterus as ascertained through the sound,

and gradually increasing the length as the organ develops in size, which, as has been said, will result, and that, in from two to three months. I have in this way, after an absence of the menstrual functions for five years, restored the uterus, which at first measurement was but one and a quarter inches in length, till finally, at the end of three months, it measured two and three-quarter inches; the menstruation returning, and in due time pregnancy supervening.

The notes of a number of such cases justify me, I repeat, in commending the galvanic pessary in their treatment.

We now come to the consideration of an affection familiar to all of you—viz., *menorrhagia* and *metrorrhagia*, due to granular inflammation or degeneration of the endometrium, ulcer, cancer, and fungous degeneration of the mucous lining of the uterus. Without entering into detail as to the means of diagnosis at our command, and presuming that this has been arrived at, whether it be one or other of these diseases that gives rise to the excessive hæmorrhage, the indication is distinctly the same in each—viz., to remove the cause, so far as may be possible, by destroying the bleeding, diseased surface, and establishing a healthy action in the tissues beneath.

To generalize, the curette, Vienna paste, chloride of zinc, and other escharotics and astringents of every kind, and finally the actual cautery, have each had their advocates, and doubtless each has been productive of good in the treatment of these cases; without descanting on their relative merits, suffice it to say that where granular or fungous degeneration, ulcer, or cancer, exists, and can be brought into view, even though the sponge tent be necessary as an auxiliary aid or means, most authorities agree that the actual cautery is invaluable, and perhaps the most reliable agent in the treatment of these diseases.

When this is the conclusion of the practitioner, the question arises how best to employ it.

We have the ordinary brutal instrument, the iron heated by means of the flame or fire, and blowpipe, which generally cools before it can be applied. We have the thermo-cautery of Paquelin, of Paris, which, although a good instrument when it works, *never* works when it is wanted. We have the gas jet, which has been recommended by Thomas and others, but this requires a double speculum, between the walls of which a stream of cold water is kept running, at great inconvenience to all parties. And we have what I now introduce to you, an instrument which is easy of manipulation, and never fails to work (Fig. 41).

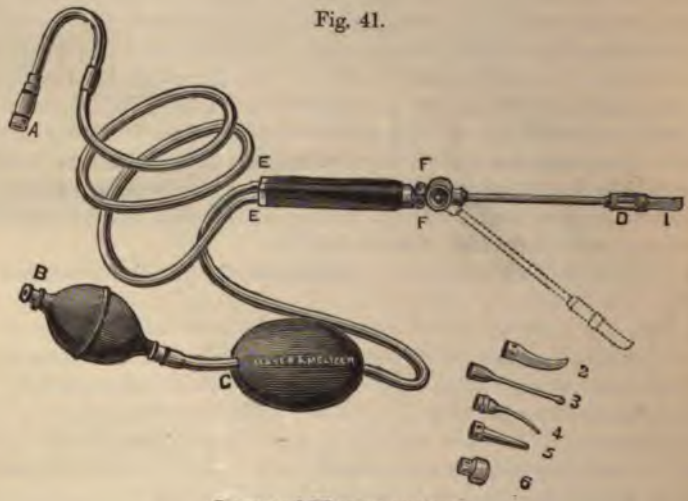
The principle involved in its construction is not new, but the application of it in these treatments, and the improvements I have made, *are new*, and I am prepared to say that under every circumstance, so long as you can command a supply of ordinary illuminating gas it will work to the entire satisfaction of the operator. It consists of a goose neck, A, which is to be applied to an ordinary gas fixture. Through a rubber tube connected at one extremity to this goose neck, and at the other to a metal tube at the handle, E, the gas is conveyed to the instrument. To another metal tube at the handle, E, we have another rubber tube attached, which conveys the atmospheric air from the reservoir, C, the reservoir having been previously filled by the bulb-pump, B. The supply of gas and of air is governed by two screw stopcocks, FF, at the upper extremity of the handle, so as to give the operator complete command of the supply of both air and gas, thus enabling him to regulate the proportion of each in such a manner as to secure perfect combustion. These metallic tubes traverse the handle, and are merged into one, so that when the flame reaches the cautery point, D, you have the highest degree of heat

attainable. Ten seconds of time is all that will be required to produce a beautiful red heat, which experience teaches is the most desirable, since with a white heat we are liable to have hæmorrhage.

The instrument is provided with a joint just above the handle, by means of which any angle may be given to it, thus obviating the hand of the operator coming into the field of vision, when being employed through the speculum; or it may be, in abdominal or other surgery, used straight.

To facilitate its use in every direction, it is fitted with a variety of points—viz., 1, is a straight knife; 2, is a curved knife; 3, is a point of such size and length as to be used through the cervical canal of the uterus; 4, is a curved point, for application to fistulas; 5, is a point designed to cut down granulations of the endo-cervical membrane; whilst 6 is a button, suitable for general use when counter-irritation is indicated. Should the air-bag become exhausted, it may be refilled easily by employing the bulb-pump, without interrupting the operation.

Fig. 41.



Improved Thermo-cautery.

In the employment of this cautery any ordinary speculum may be used, or the speculum of Sims, which enables you to bring the uterus down, thus straightening the canal, avoiding danger, and facilitating its application.

The after-treatment of all cases in which the cautery may be employed, whether local or constitutional, will of course receive the attention of the attendant.

Before concluding this paper, I claim your indulgence whilst I describe two more instruments, which I now present for the first time to the profession: the first is a rubber (velvet) elastic self-retaining catheter (Fig. 42).

Fig. 42.

Self-retaining Catheter.

This instrument, you will observe, has its eye or opening at the extremity instead of at the sides, as ordinary ones have; it is provided on either side with a barb

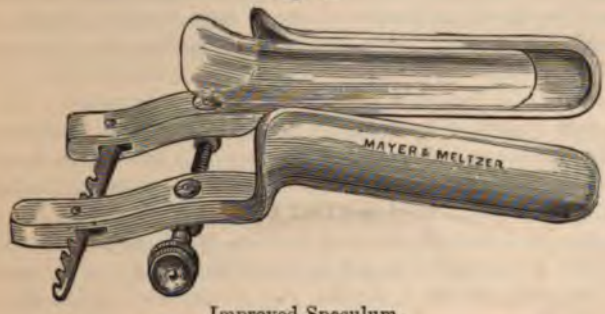
ne quarter of an inch in length and placed one quarter of an inch from the xtremity.

In the treatment of cystitis it is a well-established fact that perfect rest shall e secured to the bladder, and the most efficient means of obtaining it is through the use of a self-retaining catheter.

Metal instruments corrode and gather the deposits from the urine, hence in their removal they scrape and tear the delicate and possibly inflamed membrane of the urethra. This material is free from these objections; it never corrodes and never harbours the inorganic constituents of the urine. The barbs open on the introduction of the instrument and retain it in the viscus, whilst being of soft rubber they offer no serious impediments to its removal. It may be retained with perfect safety indefinitely.

The next, and last, instrument I shall exhibit to you is this my improved speculum.

Fig. 43.



Improved Speculum.

It is made of hard vulcanite, bi-valve in general form, but each valve so arranged as to accommodate a concave glass mirror; by this arrangement and selection of material the instrument can be always kept perfectly clean, and is indestructible from the action of acids, &c.—a great desideratum, as all metallic specula will corrode and finally be destroyed by these agents.

Another pecularity of my speculum is its movements; as you observe, we can diminish the field of either extremity, and shorten either blade or valve at will, by means of the simplest possible mechanism; and lastly its entire weight is scarce more than two ounces and a half.

DISCUSSION.

Dr. Heywood Smith, London: I wish to speak first with regard to retroflexion, and specially with regard to Dr. Graily Hewitt's statement, that if we remove the cause of the mischief we cure the resulting disease. Now, retroflexion is followed by a train of maladies and their symptoms, and it is more logical to begin with the last of the series and work back, rather than at once attack the flexion itself. The most logical way of attacking these conditions is to begin with the inflammation and subdue it, and afterwards use mechanical means for the replacement of the uterus. It is the practice always at the Hospital for Women to allay chronic inflammation and all uterine tenderness by leeches or puncturing, and by the use of tampons of wool and glycerine; and, after the uterus is free from tenderness, mechanical means are had recourse to for the rectification of the flexion. I have seen mischief, and even death, follow the replacement of the uterus and introduction of a pessary,

the uterus being in an inflamed state. In treating retroflexion, intra-vaginal pessaries are more frequently used, and if care is necessary in their use, how much more necessary is such care when anteflexion has to be treated, for such flexions are with great difficulty treated with intra-vaginal pessaries; as there is no *point d'appui* from which the uterus can be straightened, and as intra-uterine stems have to be used, it is more important that all inflammation should be first subdued. Then as regards time, I am sure that in many cases we ought fairly to state to our patient that her cure may take many months; and this is specially so where there are adhesions binding down the fundus into the cavity of the pelvis.

Dr. JOHN A. BYRNE, Dublin: Dr. Hewitt has brought forward a table containing eighteen cases in which symptoms of a most aggravated nature for the most part are attributed to displacement of some kind, and in which the symptoms disappeared on the replacement of the uterus in its normal position. Now, in the first place, this is a very small number of cases for a gentleman of the well-known reputation of Dr. Hewitt to furnish as a basis upon which to found a theory such as that forming the title of his paper; and, on looking closely into the table, I cannot fail to remark that, in my opinion, the symptoms were hysterical or hystero-epileptic, and depended most probably on other causes; and that their removal or cure was caused by the other skilful general treatment adopted by Dr. Hewitt. Can it be conceived to be possible that such formidable symptoms could depend upon the displacement of the uterus, and that they vanished at the introduction of a pessary? I must say that whilst I think anteflexion and version extremely rare, and that the normal position, which is one of anteflexion, has frequently been mistaken for it, yet that retroflexion is a very common displacement, and frequently occurs in virgins, and is very often unaccompanied by any indicative symptoms such as hæmorrhage.

Professor KUSTNER, Jena, described Schultze's figure-of-8 pessary for retroflexion and its advantages, and observed: That displacements and flexions of the uterus often produce reflex nervous and neuralgic affections of different parts of the body, and more or less pronounced mental aberrations, can hardly be denied. As Dr. Hewitt has related, the removal of the effect by the rectification of the displacement proves this statement. But other diseases of the uterus play quite as important a part, perhaps a more prominent one, in the production of these hystero-neuroses, and none more common than that known as "areolar hyperplasia," or the old "chronic metritis," either following sub-involution of the uterus or attending a displacement. The reflex neuroses accompanying these cases of chronic uterine enlargement are so variable that scarcely any organ is exempt from them; and hysteria, with its peculiar psychical phenomena, is almost uniformly met with. Whether the hysterical symptoms described by Dr. Hewitt do not depend, at least in part, on this hyperplastic condition of the areolar tissue of the uterus, and the pressure thereby on the nerve-filaments disseminated everywhere throughout the uterus, is a matter of question. But there is another lesion which may produce peculiar mental symptoms if subject to irritation without such excitement. I mean laceration of the cervix. I will relate a case in point. A patient came to me last winter with her husband, who complained chiefly that she always went into a faint whenever he had connection with her; otherwise she was perfectly well. She had had one child four years previously, and since then had been ill. On examination I found a large

aceration of the cervix with complete eversion of the lips. While touching the everted surface I asked the patient if she experienced pain, but received no answer. On looking closely at her I found her to be apparently unconscious. No shaking roused her from her trance. It occurred to me that Charcot's method of pressure over the ovarian region might rouse her. I applied firm pressure over the left abdominal ovarian region, and the patient at once gasped. A few more steady pressures brought her to consciousness. This manœuvre was repeated a number of times at each treatment, and always without fail. Only after the operation for cure of the laceration and the union of the edges did these undoubtedly hysterical phenomena cease. A marked case of mental aberration depending on retroflexion of the uterus in a young unmarried lady came to my non-professional notice a few years ago. The lady, previously healthy, fell downstairs sharply on her buttocks, and soon after became subject to melancholy, for which she was six months in an insane asylum, and for several months under distinguished neurologists. A notorious female quack finally chanced to make the long-needed examination, discovered the displacement, rectified it, introduced a proper pessary, and in a week the patient was entirely well, and has so remained, and is now a wife and mother.

Dr. ROSEBRUGH, Hamilton, Canada: described an anteflexion pessary which he had found useful. It consists of two parts—(1) a crescent-shaped Hodge which stretches the vagina and upholds the uterus; and (2) the anterior two-thirds of an Albert Smith pessary attached to the Hodge by means of a spring in such a manner as to tilt up the posterior end of the front part so as to keep up the anteflexed uterus and the anterior wall of the vagina and bladder.

Dr. GRAILY HEWITT, London: In reply to Dr. Byrne I would say that anteflexions are, according to my view, very important, and produce most troublesome symptoms and effects. I cannot go over the whole ground of the paper again, and must refer to it for answers to Dr. Byrne's objections. One thing, however, I would say, that I throw out the challenge to Dr. Byrne to produce a case of marked hysteria not accompanied by uterine distortion or displacement. In the paper I have described "congestion" as one of the exciting causes. This covers the effects of congestion—viz., enlargement and hypertrophy, described by Thomas as hyperplasia.

Gedankengang der Eröffnungsrede über Total-Exstirpation des Uterus.

Prof. WILHELM ALEXANDER FREUND, Strassburg.

Meine Erfahrungen von drei Jahren, welche seit meiner ersten Publikation "über die Total-Exstirpation des Uterus" verflossen sind, und die in dieser Zeit veröffentlichten Arbeiten von Collegen über denselben Gegenstand, haben mein Urtheil über die wichtigsten Punkte dieser Angelegenheit zur Klarheit gebracht.

Es ist wohl unbestritten, dass bei carcinomatöser Erkrankung des Uterus, die sich über einen grösseren Theil dieses Organes erstreckt, die Total-Exstirpation die richtig indicirte Operation ist.

Seit langer Zeit haben die Chirurgen immer wieder diese Operation unter-

nommen, und bei der Ausführung beide Wege, die man auch heute einschlägt, nämlich den Weg durch die Scheide und den durch die geöffneten Bauchdecken versucht. Die Erfolge dieser Versuche waren so schlechte, dass man lange Zeit von weiteren abstand.

Als ich nun meine Methode sofort am ersten Falle mit gutem Erfolge erprobt hatte, als die Herren Collegen Dr. Martini in Breslau, Dr. Kocks in Bonn, Prof. Dr. Olshausen in Halle, Prof. Dr. Schröder in Berlin, Dr. Veit in Berlin, Prof. Dr. Spiegelberg in Breslau, Dr. Kuhn in St. Gallen gerade mit ihren ersten Fällen dieselbe Freude des Gelingens erlebten, so war die Hoffnung und ein gewisses Uebermaass der Zuversicht, diese grosse Aufgabe der Chirurgie endlich sicher gelöst zu haben, wohl verständlich und gerechtfertigt.

Als aber schlimme Erfolge sich erschreckend häuften, als auch die traurige Thatsache klar wurde, dass nach gelungener Operation Recidive nicht ausblieben, da trat von vielen Seiten Entmuthigung und Opposition in dieser Angelegenheit hervor, und diese Stimmung beengt heute fast ebenso sehr den Blick, wie ihn die entgegengesetzte vorher über das Maass erhoben hatte.

In der That—unter den Umständen, welche in Bezug auf Genesung nach der Operation ungünstiger, in Bezug auf Radikalheilung nicht günstiger waren, als die bei der Amputation der erkrankten Portio vaginalis und der supravaginalen Amputation des erkrankten Corpus uteri, würde man nicht mehr berechtigt sein die Total-Exstirpation zu unternehmen, wenn diese allerdings richtig indicirte Operation nicht mit grösserer Lebenssicherung als bisher ausgeführt werden könnte.

Diese grössere Sicherung gewährt nach neuesten Erfahrungen die vaginale Total-Exstirpation (Sauter) nach Czerny-, Billroth-, Schröder-, Martini'schen Grundsätzen ausgeführt—und die abdominale Total-Exstirpation nach den Angaben von Bardenheuer, Breisky, Rydygier, Kolaczeck, M. B. Freund modificirt.—Die Offenhaltung und Drainage des Peritoneums, die einfache Unterbindung der Gefässe der durchschnittenen Ligamenta lata Schritt für Schritt machen die Operation von kürzerer Dauer, weniger mühsam und beschleunigen die Heilung. Ich habe mich durch Versuche an Leichen überzeugt, dass es nach Abtrennung des Cervix uteri vom Laquear vaginæ leicht gelingt, den Uterus durch die Bauchwunde bis über die Symphyse hervorzuziehen. Rydygier und ich haben diese von Breisky zuerst empfohlene Operationsweise an Lebenden ausgeführt, und ich kann dieses Verfahren dringend loben. Zieht man den Uterus mittelst der von mir angegebenen Klemmzange hervor, so erreicht man zugleich damit relative Blutleere des Uterus.—Die Umschneidung des Cervix von der Vagina aus habe ich unmittelbar vor der eigentlichen Operation an der nicht chloroformirten Kranken ausgeführt.

Die Erfolge der vaginalen Total-Exstirpation in Bezug auf Genesung von der Operation scheinen sehr gute zu sein,—und ebenso kann Kolaczeck bereits sagen, dass nach der von ihm und Martini geübten Methode der letale Ausgang zu den Ausnahmen gehört.—Damit wäre denn derjenige Punkt bei dieser Operation erreicht, den ich für den erwünschten hingestellt habe: dass die Operation als eine nicht sehr gefährliche in den Anfangstadien des Carcinoms und Sarkoms ausgeführt werden dürfe, wo sie Radikalheilung verspricht.

Ob nun die vaginale oder die abdominale Exstirpation gemacht werden soll, das muss sich nach dem einzelnen Falle richten. Ist der Uterus sehr gross, die Scheide sehr eng, so wird man immer die abdominale Total-Exstirpation ausführen müssen. Bei kleinem Uterus, weiter Scheide ist die vaginale Operation vorzuziehen. Immer aber wird man in's Auge fassen müssen, worauf auch Kolaczeck

hinweist, dass die abdominale Operation durch die Erleichterung und Sicherung der Schnittführung im Gesunden von der vaginalen einen grossen Vorzug hat.*

Ich beurtheile meine Methode der Total-Exstirpation als ein Stadium in der Entwickelung dieser Operation, das nach den Aufgaben, welche die damalige Erkenntniss aufstellen liess, d. i. Verhütung der Blutung und Deckung des Peritonealdefekts (Hegar-Kaltenbach) geradezu nothwendig gegeben war, das aber jetzt überholt ist, nachdem eine geförderte Erkenntniss die Gefahr der Blutung als geringer, die des Peritonealdefekts als nicht bestehend, vielmehr die Offenhaltung der Peritonealwunde als höchst erwünscht erwiesen hat.

Ich erachte es als einen hohen Vorzug unserer Zeit, dass solche Förderungen der Erkenntniss und des Handelns sich so schnell vollziehen, und ich bin erfreut zu dieser Förderung eine Anregung gegeben zu haben.

DISCUSSION.

Professor G. CORRADI, Florence: History teaches us that extirpation of the uterus affected with cancer, though proposed at various periods, and performed by different methods, has not yet gained the acceptance of surgeons. We may hope, however, that this operation may find more favour at the present day. It is true that it is still to some extent experimental; in fact, while it was brought back into practice by Freund with the abdominal section, other surgeons have practised it with the vaginal, thus returning to the views of Bellini, Récamier, and others. This method shows that both present real advantages; but it is also evident that neither is free from practical inconveniences. The abdominal incision presents these principal advantages:—1st. It enables the operator to ascertain the degree of mobility of the uterus, the extent of its adhesions, the progress of the disease, and the local glandular infection. 2nd. It enables him better to avoid hæmorrhage, and, if hæmorrhage does occur, it can be more easily arrested. 3rd. By this method the bladder, rectum, and ureters are less exposed to injury. 4th. It renders the operation possible even in cases where considerable adhesions have already formed. But the abdominal method, however modified, obliges us always to make a large incision, which has serious drawbacks—*e.g.*, 1st, the cooling of the abdominal viscera and long-continued exposure of the peritoneum and intestines; 2nd, the extensive and prolonged injury to the peritoneum and intestines; 3rd, if the patient is very stout, the operation may be troublesome, difficult, and perhaps impossible. The vaginal method has not these great inconveniences; but, 1st, it exposes the bladder and ureters more to injury; 2nd, if there should be bleeding, the operator would find great difficulty, not having free access to the whole field of the operation; 3rd, for this method the vagina ought to be capacious, the uterus movable and not much enlarged in volume; 4th, if there are uterine adhesions the operation may be very difficult, extremely dangerous, and finally the surgeon may be obliged to leave the operation unfinished. Thinking about these two methods, I have convinced myself that in most cases we can utilize the best part of both, and avoid their

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* *Anmerkung vom 7. Juli.*—Am 5. Juli habe ich den carcinomatösen Uterus durch Laparotomie total extirpirt, trotz mässiger Grösse des corpus uteri und hinreichender Weite der vagina, behufs Entfernung mehrerer, bedeutend vergrösserter, interabdominaler Lymphdrüsen (glandulae iliacae), die ich für carcinomatös degenerirt gehalten habe und die bei späterer Untersuchung sich als solche erwiesen haben. Diese Entfernung der glandulae iliacae ist bis jetzt, so viel ich weiss, noch nicht unternommen worden, und erwies es sich durchaus nicht schwierig. Ueber den weiteren Verlauf des Falles werde ich zur Zeit berichten.

chief dangers. In a word, I propose a mixed method, which I will now shortly describe. As it is necessary for the operation that there should be a capacious vagina, we must prepare it a few days before, and if it is impossible to dilate it by any means, then we must renounce this vaginal method. We may perform this dilatation by means of india-rubber balls distended with water, and just before the operation we can take advantage of the anæsthesia to increase the dilatation to the highest degree. For the sake of brevity I omit the other preliminaries, and will begin by saying that the patient must be placed on a table with a pillow under her buttocks, and in Simon's position. A small incision is then made in the linea alba just sufficient to allow the hand to pass into the peritoneal cavity. This done, the dilator is extracted from the vagina in order to introduce my speculum, or a large-bladed retractor, so as to enable us to see perfectly the neck of the uterus. The operator introduces his left hand through the abdominal incision, and places the tips of the fore and middle fingers in the peritoneal fold, which lies between the uterus and bladder, and at the same time pulls down with the forefinger of the right hand the anterior cul-de-sac, being thus able with both hands to recognize the exact limit between the uterus and bladder. The operator passes a needle into the middle of this cul de sac, close to the uterus, with a strong double silken thread, leaving the ends out of the vagina and the loop out of the abdominal incision. With the left hand he turns the body of the uterus into Douglas' fold, so as to put it in a position of forced retroversion, and pulls it outwards. He next divides with a knife the vagina from the posterior part (now anterior) of the neck of the uterus, and, keeping always close to it, he cuts the corresponding fold of the peritoneum. This incision must be semicircular and transverse, and lies near the uterine insertions of both utero-sacral ligaments. By pressing gradually with the left hand on the body of the uterus, it is made to pass through this incision, and the operator seizes it with two strong-toothed forceps near the insertion of the two Fallopian tubes. This done, the abdominal end of the silken thread must be passed through the wound in Douglas' fold, and between the two forceps, so that it forms a loop, which goes round the uterus in the median line. The thread is then drawn through, and the wire is made to take its place. In the looped end of this double thread must be fixed one end of a platinum wire, one mill. in diameter. The two ends of this loop must be put into a wire-holder. The operator withdraws his left hand from the abdomen, and having placed the loop of platinum well and straight, he renders it incandescent by means of a galvanic pile, and so divides the uterus in its median plane. It must be remembered that the uterus is very much retroverted and flexed, and that it is drawn downwards and outwards, so that the circulation of the blood in it is arrested. If, therefore, the universally known electro-thermic rules are followed, we may be almost certain to have no bleeding from this incision. But even if bleeding should occur, it would be very easy to arrest it if a provisional ligature were placed on each half of the uterus. This division of the uterus might even be effected by the écraseur or the scissors, for, as I have said, the surgeon might command any hæmorrhage, with provisional ligatures; but if a battery is at hand I think it better to use the galvanic loop. The two halves of the uterus thus divided and drawn by the two forceps to the left and right, separate one from another. Then the operator carries his left forefinger into the anterior peritoneal fold, and with this sure guide he cuts from the neck of the uterus with blunt-pointed knife, or with scissors curved on the flat, the left half of the anterior cul-de-sac and of the corresponding peritoneal fold. The same is to be done on the right side of the anterior cul-de-sac, so that each half of the uterus remains

attached only by its own ligaments. In this way it is quite easy to secure the bladder and ureters from injury. The division of the uterus is also very useful for securing the ligaments. and the extirpation of the ovaries if it should be necessary. We may take one half of the uterus—suppose the left—and turn it on its own axis, so that the divided side is now turned outwards, and the side where the ligaments are attached, inwards. The operator may then seize and bring out, if necessary, each ovary after having it well strangulated; he can do very high ligatures of precaution on the Fallopian tube, on the round ligament, and on the utero-sacral ligament, and finally he seizes with a forceps the Fallopian tube and the round ligament, and draws them up in order to unfold the broad ligament, which must be pierced by a needle carrying a strong silken loop, to be used for the two ligatures; the first so high as to surround the broad ligament and the tube, the second lower, to surround the utero-sacral ligament near its uterine attachment. It is hardly necessary to say that the two loops ought to be passed into each other, and that the ends of one of them should closely strangulate all the tissues. We must next tie the same ligatures on the other side—that is, on the ligaments of the right half of the uterus. Then keeping always both halves of the uterus apart one from another, and forcibly drawn outwards, the operator unites by means of the interrupted suture the edges of the transverse vaginal wound, including the peritoneum. Each half of the uterus is then removed separately, the tissues being divided close to the ligatures, and thus the ligaments and the ligatures, which strangulate them, are brought out at the angles of the wound in the vagina. After having carefully cleaned out the vagina, the operator sews up the abdominal wound, and so finishes the operation, which I submit for your judgment. I trust that you will excuse me from either writing its eulogy, undertaking its defence, or comparing it with other methods. I should be glad if any of you would perform this operation on the dead body, and then either write to me privately or state in print your opinion of it.

Dr. A. Martin, Berlin: Only a very small number of cases of cancer of the uterus can be subjected to the operation of total extirpation of the uterus, because any cancerous infection of the broad ligaments, or even of the lymphatic vessels therein, renders all these efforts surely futile. I have operated according to Freund's plan six times; the results were throughout fatal. I attribute this principally to the extent of the incision which is made in the abdominal walls and at the bottom of the pelvis. In this respect the vaginal method is much less formidable. But the vaginal method is a very difficult one, by reason of the narrowness of the parts. The objections to this method are that the control of bleeding is difficult. With this I cannot agree. I have done vaginal extirpation in seventeen cases, and the difficulty was met with in a few cases only, and in those the hæmorrhage came from cancerous deposits outside the uterus, which could not be entirely removed. I ligate all vessels before dividing them, and in this way I lost less blood generally than in simple plastic operations. The bladder and ureters I have never injured in vaginal operation. As to recurrence, I have seen patients operated on in October, November, and December, 1880, quite well, though this question can be settled only after some years' longer experience.

Prof. Czerny, Heidelberg: I was induced by the bad results which I had at first with Freund's operation to put again in practice the vaginal method. As it so often happens, I had at first good results, until I, with operations somewhat too thorough-

going, had also mishaps. As well as I can remember, I have performed the vaginal operation seven times, with three deaths. Of the four which recovered after the operation one died of recurrence, one was operated upon six months afterwards for a recurrence in the vagina; two, so far as I know, have remained free for a long time. From my present experience, as well as from the statement of Freund, I believe that, for movable tumours which do not involve the vagina, and in cases in which the vagina is capacious, the vaginal method is the best. When the uterus is fixed, and when the dragging down of the same fails, we can forthwith have recourse to the abdominal incision, which, according to the experience of Bardenheuer, promises better results. Moreover, we can bring about a radical cure in cases in which the carcinoma is limited to the external orifice by means of the supra-vaginal incision, as I can affirm from my own experience (two cases are still free from recurrence two years after operation), while for deep-seated carcinoma which has involved the parametrium and the pelvic walls, palliatives (scraping and cauterization) are to be recommended.

Dr. C. Hennig, Leipzig: In the year 1875, before Professors Czerny and Billroth recommended the vaginal method, I laid before the Association of Naturalists and Physicians a cancerous uterus which I had taken out by Sauter's method from a woman forty-three years of age. In this case the womb was totally fixed and could not at all be dragged down, not because of disease of the surrounding tissues, but because of a layer of peritoneal exudation at the fundus uteri. This impediment for operating proved an advantage: the organ could be removed without any wound being inflicted on the peritoneum, this being protected by the false membrane. This person lived a year more, and died in the summer of 1876, of diarrhœa, as was reported to me by her physician. After this I saw a couple of Sauterian extirpations, but all the patients had relapses and are dead. Nevertheless, even if but one or two per cent. should be saved, I would recommend the extirpation of a womb with malignant disease, according to the indications just given by my friend, Dr. Martin. We ought not to shrink from the difficulties of the operation. These are, indeed, greater in Freund's method, which seems to be fit for fewer cases than the vaginal one.

Dr. Marcy, Boston: I rise to discuss but a single point in relation to the very valuable paper of Prof. Freund. First, I believe the disease arises *in loco*, and is not constitutional, and it has been my habit for many years to operate upon cancer in every part of the body, as early as possible, and remove all neighbouring glands, fat, &c., very thoroughly. I have never extirpated the uterus entire for cancer, but am prepared to believe it highly advisable in a considerable class of selected cases. From a number of pelvic operations per vaginam, I am inclined to think this the preferable method when possible, and, when thus done, I would not close the opening as a rule, but use the drainage-tube. For this purpose I have devised double rubber tubes of various calibre, which I take pleasure to exhibit. They are of equal value for simple drainage with any tube, and have the especial benefit of rendering it easy and quite safe to irrigate the pelvic cavity, if desired, with hot water, carbolic acid solution, &c. This appears to me of the first importance in certain cases, in washing out clots and decomposing material. These tubes are manufactured in moulds, and have the perfection of finish only recently attained in the making of catheters.

Dr. NELSON, Chicago: It is generally admitted that in other parts of the body cancer should be removed as soon as discovered, if we are to hope for good results. Then if the cancerous mass discovered proves to be the primary development of the disease, and this has been thoroughly removed before it has been transplanted into other organs, we may confidently expect it will not return. In the uterus also it should be removed at once on discovery, and completely removed. But can any form of cancerous disease—epithelioma, sarcoma, or carcinoma—be partially removed from the uterus without its total ablation? I believe the cases are rare where it can be, and these only such cases of epithelioma as implicate only a small portion of the cervix. And even in such cases, does not the greater risk of the return of the disease more than counterbalance the greater danger of total extirpation of the uterus? Except, perhaps, in the very few cases seen before the disease can have extended to the body of the uterus or the adjacent glands, I would extirpate the entire uterus by abdominal section, and along with it many of the adjacent tissues, whether glandular or vaginal, which showed suspicion of disease, or were even near those already diseased.

Dr. BEVERLEY COLE, San Francisco: I have never seen this operation but twice, one done by a friend, the other by myself in New York. Yet as it is an operation of quite recent date, and statistics are necessary, an account in brief of this operation as done by me may have some interest for you. I must say first, that I am in sympathy with the conclusions of Dr. Freund—viz., that some cases can be better operated on through the abdominal walls, whilst others can be more easily and safely done through the vagina. The case I operated on was one of commencing carcinoma of the body of the uterus, which was not yet materially enlarged—the vagina was as relaxed as is the vagina of matrons generally. A Sims' speculum was employed, the uterus seized and brought down, an incision one inch and three-quarters in length transversely was made through the vagina into the cul-de-sac of Douglas, at the fornix of the latter, and the uterus. A tenaculum was then passed through the wound, buried in the fundus of the organ, and the uterus retroflexed and brought down through the incision. The volsellum forceps was now substituted for the tenaculum, and strong traction resorted to, which had the effect of bringing not only the body of the womb down, but with it the ligaments lateral and round; the bladder was carefully dissected from its attachments to the uterus, the ligaments were secured by strong carbolated silk ligatures on either side and seized, the clamp forceps applied to the uterus as near the fornix as possible, and by means of the knife the amputation so to speak was effected. Whilst holding the stump in the grasp of the forceps my gas cautery was applied to the cut surface, and thus all hæmorrhage was arrested. The ligatures were now brought through the incision and vagina and served as a syphon or drainage-tube. No sutures were applied, the wound remaining open until closed by granulation. Within fifteen days the ligatures were cast off, and in twenty-one the patient was declared well, the incision or opening into the cul-de-sac having closed entirely; the only after-treatment instituted in this case was the use of injection, per vaginam, of carbolated warm water three times a day, keeping the nervous system quiet by means of morph. sulph., &c., and not an untoward symptom presented itself from first to last. The second case alluded to was operated upon by a friend, the details of which being in the main the same as my own, it is not necessary to give them; this latter died one year after the operation from a reproduction of the disease, whilst my patient is still living, but in my judgment must sooner or later succumb to the

ravages of the most horrible of diseases. In a word, whilst I believe that the operation of extirpation is justifiable because it prolongs life and enhances the comfort of the patient, I do not believe cancer curable through any means at our command or known to us.

Dr. Wynn Williams, London: Although I have paid a good deal of attention to cancer of the uterus, I have never removed it with the knife; I consider that I can produce better results by the means which I employ. In treating epithelioma where there is a large growth from the neck of the uterus, I remove it by means of the écraseur and the scooping away any parts that can be removed in this way. Should there be much hæmorrhage it is arrested by the application of the actual cautery. I then pack the parts within and around the uterus with cotton wool, saturated with a solution of bromine, one part of bromine to three of spirit of wine (in mixing, the bromine must be added to the spirit of wine on account of the great increase of temperature that takes place); the surrounding parts must be well protected by cotton wool, saturated with a solution of carbonate of soda. Bromine causes a much larger destruction of the diseased structures than any other escharotic. A cup may be placed over the diseased neck with wool saturated with bromine, or it may be covered with oiled silk. In this way the uterus may be completely excavated, leaving only a mere shell. Again and again patients have been treated in this way with the result of being restored apparently to perfect health, living eight or nine years without any return.

Mr. Spencer Wells, London: I have never removed the entire uterus affected with cancer, because I have never seen a case where so serious an operation appeared justifiable, and where also the disease had not extended into the surrounding tissues to such an extent as to render the operation, if not fatal, of no permanent benefit. In cases where diseased structure could be scraped or cut away, and bromine, chloride of zinc, or the actual cautery applied to the raw surface, I have seen cases where bleeding and offensive discharge have been stopped to the great comfort of the woman; many months, and in a few cases years, have passed before there was any return of disease. In two cases patients have died after five and six years of some other disease without any return of the uterine growth. If the entire uterus were removed through the vagina, in a case where the surrounding structures were healthy, bleeding might probably be arrested in a simpler way than by the series of ligatures described by Dr. Martin. A number of pressure forceps might be applied before dividing the tissues closely around the neck and body of the uterus, or directly after division, and the forceps left hanging out of the vagina until all fear of bleeding is over, and they might also be used to keep together the opposite sides of the divided vagina.

Prof. Simpson, Edinburgh: To me the course of the discussion has been specially interesting from one point of view. At the meeting of the British Medical Association, nine years ago, I read an account of some cases illustrative of the value of removal of portions of the cancerous uterus. One of the fathers of gynæcology then alleged that it was only youthful enthusiasm and inexperience that would attack a cancerous uterus. Throughout all the debate to-day much has been said as to the best way of attacking the cancerous uterus, and even when many said that not only the cervix, but the whole organ, should be removed, there was no man who said that no operation should be done.

Dr. I. N. Quimby, Jersey City, N. J.: I think that it is impracticable to remove the uterus for cancerous disease after the surrounding tissues have become much involved, and especially if adhesion has taken place to the neighbouring parts. But if the patient is seen in the early stages of carcinoma, and the disease is confined to the anterior portion of the uterus, and the organ is still movable, I believe the removal of the cancerous portion of the uterus to be eminently proper. But the best method of removal is by écraseur. I have operated on two such cases with entire success by the écraseur. After controlling the hæmorrhage by the use of compression forceps, I applied the pure carbolic acid to the cut surface, being careful not to allow any of the acid to flow upon the vagina or any of the uncut surface.

On a Point in the Diagnosis of Ovarian Tumours.

Dr. Jastrebow, St. Petersburg.

That it is often difficult, and sometimes impossible, to determine from which side an ovarian tumour grows is well known. I think, however, that there is a symptom which may help us on this point; and as I have but a small personal experience of it, I bring it before the profession in the hope that others may turn their attention to the subject and test its value for themselves. The symptom to which I would direct attention is impairment of sensibility in the inguinal region (inguinal triangle), which is supplied by the genito-crural nerve. I think that if the sensibility of the two sides be compared, it will be found to be less acute on the side of the affected ovary. I shall now give a brief report of five cases from Professor Slawjansky's Clinic, in which I have observed this symptom.

Case I.—*Multilocular cyst of the left ovary—Ovariotomy performed by Dr. Slawjansky—Recovery—Impairment of sensibility in the left inguinal region.*—The patient, aged 26, came to the Clinic on the 27th of September, 1873. Six months before she had suffered pain in the lower part of the abdomen, and noticed a tumour of about the size of a fist. The tumour grew rapidly, and when I saw her it was about the size of an adult head. On investigating the cutaneous sensibility of the whole body with Weber's circle, I found impairment of sensibility only in the left inguinal region. In the right inguinal region the distance was 4 centimetres, while in the left it was 7, showing a difference of 3 centimetres. During a menstrual period the distance on the right side was only 2 centimetres, but on the left side it remained 7 centimetres. Ovariotomy was performed on the 1st of November, 1878, and the tumour was found to come from the left ovary—that is, the ovary on the side in which the sensibility was impaired. After the operation, the following data were obtained by investigation with Weber's Circle:—

Day after operation.	Right side.	Left side.
6th	3·	1·5
9th	4·6	1·3
20th	3·5	3·5
32nd	3·5	4·

Case II.—*Multilocular proliferous glandular cyst of the right ovary—Ovario-*

tomy by Dr. Slawjansky—Recovery—Impairment of sensibility in the right inguinal region.—The patient, aged 38, came to the Clinic on the 11th of November, 1879. Two months previously she noticed a painful tumour in the lower part of the abdomen, which grew rapidly. On admission the upper limit of the tumour reached to midway between the umbilicus and the scrobiculus cordis. The distance of distinct tactile perceptions in the left inguinal region was 2 centimetres, in the right 5. The sensibility to pain, weight, differences of temperature, and electricity was less on the right than on the left side. The diagnosis of a tumour growing from the right side was verified at the operation.

Case III.—*Multilocular proliferous glandular cyst of the right ovary—Impairment of sensibility in the right inguinal region—Partial ovariotomy by Dr. Slawjansky, and death.*—The patient, aged 23, came to the Clinic on the 22nd of November, 1879. For a year and a half she had noticed that the abdomen was increasing in size. On investigation a very large tumour was found, bound down by adhesions. The sensibility to touch, weight, and electricity was less in the right inguinal region than in the left. The operation and subsequent post-mortem examination proved that the tumour sprang from the right ovary.

Case IV.—*Multilocular cyst of the right ovary—Ovariotomy by Dr. Solsky—Recovery—Impairment of cutaneous sensibility in the right groin.*—The patient, aged 24, was admitted on the 23rd of February, 1881, with an abdominal tumour the size of an adult head, which presented all the signs of a multilocular ovarian cyst. On investigation with Weber's circle tactile sensibility was found to be diminished on the right side, and the diagnosis of right ovarian tumour was confirmed at the operation on the 26th of February.

Case V.—*Ovarian cyst of the left side—Ovariotomy by Dr. Slawjansky—Recovery—Impairment of sensibility in the left inguinal region.*—The patient, aged 16, was admitted on the 10th of December, 1880. She first menstruated at the age of 15, and six months later noticed a tumour in the abdomen, which on admission was the size of an adult head. Weber's circle showed impairment of sensibility in the left inguinal region. The correctness of the diagnosis was proved at the operation of the 6th of April, 1881.

From these cases it would seem that with ovarian tumours there is present a difference of sensibility in the right and left inguinal triangles, and that the impairment of sensibility is always present on the diseased side, and that discovery of this impairment enables us to diagnose the side affected.

Our present knowledge of the innervation of the uterus and its appendages does not enable us to explain this symptom, and a greater number of cases is required to establish its diagnostic value.

DISCUSSION.

Professor Kustner, Jena: I can recommend a method of diagnosing large ovarian tumours which was published some years ago by Schultze, which has proved most valuable. The patient, deeply narcotized, is moved with the buttocks to the edge of the table; two fingers, or half the hand, are introduced into the rectum, the thumb into the vagina, and an effort is made to grasp the fundus of the uterus from the rectum. If this fails, then the cervix of the uterus is seized with volsellum forceps, and the organ pulled down until it succeeds. Then the abdomen, distended by the tumour, is grasped firmly above the pelvis by an assistant with the two hands, and the tumour is lifted upwards and forwards as if it were about to be torn from its connections in the pelvis. Then the two fingers in the

rectum will, in most cases, be able to feel on which side of the uterus the pedicle is situated, and also estimate most accurately the nature of the pedicle, whether long or short, wide or thin.

On the Operative Treatment of Extra-Uterine Pregnancy.

Dr. A. Martin, Berlin.

The propositions for the treatment of extra-uterine pregnancy have only within a short time assumed a definite shape. The unfortunate subjects of this form of gestation were left to take their own chances, and measures were only taken when elimination had commenced in some direction. Then either the death of the fœtus was attempted, afterwards waiting to see what becomes of the ovum, or the immediate emptying of the sac was essayed, at least in so far that external communication was established with the sac. Gradually it was proposed to attempt the extraction of the living child. In course of time three methods of procedure have been accepted—first, interruption of pregnancy; second, the extraction of the living child at full term; third, the opening, clearing out, and rendering harmless of the sac, in whatever stage of its development, whenever the general symptoms assume a dangerous character.

The questions as to the advisability of these various proceedings I will not discuss to-day. I wish simply to point out a method of treatment of the sac after gastrotomy put in practice by myself, which may perhaps aid in lessening the dangers of this operation, and thereby insure the only rational avoidance of the difficulties of those cases of extra-uterine pregnancy in which the ovum cannot be extracted *in toto*, as in those cases in which adhesions with surrounding organs are slight or absent.

Cases of the ovum lying free in the abdominal cavity are by far the rarest. I have operated on one of these cases with success, but they do not come into discussion here. When in a laparotomy we have opened the completely adherent sac, we always encounter two difficulties—*i.e.*, the treatment of the placenta and the walls of the sac. The first, even if the child has long since died, can cause immediate danger to life by profuse hæmorrhage, as is shown in a case reported by Schröder; the second by their decomposition endanger convalescence.

The difficulties pertaining to the removal of the placenta were considered so great that its immediate removal was abstained from, its elimination being left to suppuration through the external opening during convalescence. It is, however, evident to all, that convalescence is considerably endangered by this procedure, for even if the sac as such were closed, and the diligent injection of the sac should prevent a stagnation of the secretions in the bottom, still it is clear that the recovery is thus retarded greatly, and the patient liable to such dangers as are wont to accompany these processes, even with the strictest antiseptic treatment.

In the case which I now report, as well as on a former occasion, I have treated the placental insertion on rules different from those previously acted on, in so far that I ligated the base of the placenta. The ligation of single bleeding points is possible only in exceptional cases. By the ligature *en masse*, on the other hand, large bleeding surfaces can be tied quickly and safely, without danger from the hæmorrhage, so that we need not fear secondary hæmorrhage during recovery.

In my last case—one of tubal pregnancy—I ligated the connection with the

uterus in three parts—the placental insertion was situated at the uterine end of the tube—and the placenta could at once be removed without the loss of a single drop of blood. In a former case—an abdominal pregnancy—the placenta was inserted on the walls of the pelvis. Here, likewise, the placental insertion could be ligated and loss of blood avoided. I believe that a large proportion of similar cases could be treated in a like manner, and that the cases in which the placenta need be left behind would be few, and exceptional ones in which the base of the placenta could not be ligated.

The treatment of the sac as pursued heretofore consisted in uniting its upper walls with the abdominal wound. Drainage was made either by carrying the drain through the abdominal wound or by perforating the bottom of the sac from the vagina, and combining the latter with the former. The abdominal wound always remains open, of course with closure of the peritoneal cavity; the sac walls are often friable, tearing easily from unskilful manipulations, and frequently in a large sac diverticuli remain, which, in spite of all irrigation, can easily retain secretions. The sac remains large, however much it may be compressed by the intestines. Considering these evils, I pursued a course in my last case of extra-uterine pregnancy which had rendered me good service five months ago in an operation for extra-peritoneal hæmatocele of immense size.

In this case of extra-uterine pregnancy, after having introduced two thick drainage-tubes through the bottom of the cavity from the vagina, and having assured myself that the injections flowed off freely, I resected the sac walls so far as the numerous and extensive intestinal adhesions allowed it, and sewed them together above, partly by means of interrupted silken sutures, and partly by means of glovers' sutures, also of silk.

In this manner a sac was formed which was thoroughly closed off from the abdominal cavity, and which allowed free exit and entrance of fluids. By these means the abdominal cavity could be carefully cleansed, and the wound closed. The convalescence of the patient fulfilled completely the most sanguine expectations, for no reaction whatever followed.

The case in which both of these methods, differing from former ones, were pursued for the treatment of the placenta and the sac, was the following:—

Mrs. Günther, æt. thirty-nine, washerwoman. Has always been well; menstruated regularly, but scantily, since her eighteenth year; was delivered of a child at term fifteen years ago. Four years later had an abortion. Her menses in the middle of June of the present year were noticeably scant. Since four weeks has complained of radiating pains in the abdomen, especially on the left side, which interfered with her work, and robbed her of sleep. At the same time she steadily lost flesh and strength. Appetite and stool regular.

Present condition, June 6, 1881.

Tall, moderately strong woman. In the dorsal position a large mass was found to rise above the symphysis from the true pelvis, causing a considerable prominence of the abdominal walls. This projected higher on the left side, and could be easily palpated, showing on the right side an additional elevation, which was not intimately connected with, and was softer, than the larger mass. The mass itself seemed to present a smooth but irregular flat surface, and was of firm consistency. Percussion not dull; auscultation of the tumour gave a dull bruit, synchronous with the radial pulse. Vagina wide and stretched upwards and forwards. The vaginal portion was thickened, and lay high up immediately behind the symphysis. Behind this was a tumour, reaching to the bottom of the pelvis, pressing the bottom

of Douglass cul-de-sac low down, elastic to the touch, and not movable to any degree. This mass lay close to the uterus, and seemed to be intimately connected with it, pressing it upwards and forwards out of the true pelvis.

Here the surface of the tumour was smoother, and seemingly also of firm consistency. The uterus, on the contrary, felt decidedly soft. By palpation the body of the uterus could be readily separated from the hard tumour. The sound entered 9 cm., but by means of it the uterus could not be moved. The bladder lay above and in front. The large os could not be readily brought into the field of the speculum; the colour of the vaginal portion and the fornix was bluish, and the surface of the vagina covered with considerable catarrhal secretion. The diagnosis of the tumour was confirmed by an examination of the patient under the influence of chloroform. As to the nature of the tumour, I could ascertain nothing. I thought of an ovarian tumour fixed by adhesions in the pelvis, or of a subserous fibroid tumour, as I had operated on two similar ones shortly before. For the development of an abscess the anamnesis contained not a single hint, and the patient denied at that time every possibility of pregnancy. I decided to make an exploratory incision, to try how the poor starving patient might be delivered from her evil.

Operation, July 7, 1881. Chloroform.

The abdominal incision bled but slightly, the arteries only requiring to be caught up. Panniculus adiposus moderate, intestines strongly injected, and partly covered by the omentum, the latter being exceedingly vascular. The surface of the tumour was of a dark reddish blue colour; the upper part showed an extensive adhesion, with a portion of the small intestine, and a larger one with the sigmoid flexure. The uterus was covered with superficial false membranes, and was intimately connected with the tumour, being thick, soft, and of a bluish colour. Upon attempting to ascertain the exact position of the tumour, the uterus as far as the cervix presented itself at the abdominal opening, the bladder being invisible.

The tumour proved to be adherent on all sides, filling the whole true pelvis from the left side to the right, on which the finger could not enter freely Douglas pouch. In front, and to the left, the anterior surface of the left lateral ligament, as well as the sheath of the ileo-psoas, was blended with the tumour.

Upon separation of the intestinal adhesions the surface of the tumour tore slightly and about a tablespoonful of tolerably clear fluid escaped. The tumour had previously felt as if immediately below the surface. It contained a bony structure, and had given rise to the suspicion of extra-uterine pregnancy. Upon enlarging the small opening with a view of emptying the sac, the occiput of a six months' fœtus appeared, the face showing muscular contraction; then the child began to breathe. The intestines being pushed up and covered with a warm damp cloth, the fœtus gradually followed, pushed out by the contraction of the sac walls. The long, thin, and but slightly convoluted cord was still pulsating. Immediately upon the expulsion of the child the sac collapsed, allowing a large quantity of dark blood to escape, especially from the anterior wall. The application of Péan's hæmostatic forceps tore new vessels, the tissue being exceedingly friable, and bleeding freely and profusely. For this reason the cord was severed at once, and the child, gradually ceasing to breathe, laid aside. An inspection of the sac now showed that the placenta was inserted over a great extent of the anterior wall, and that the proviously mentioned hæmorrhage originated from it. The hæmorrhage, coming in spurts, filled the sac until the communication with the uterus was cut off by means of three deep ligatures, one reaching from the bottom to the centre, and the others

from here over the tube and embracing the side of the uterus. The placenta could then be loosened and removed, appearing to be entire, although considerably torn. It was friable, and its inner surface, as well as the fœtus, was covered as if with dust. No hæmorrhage followed its removal. The bottom of the sac was now searched for and perforated by means of long blunt forceps towards the vagina, and a thick drainage tube with a cross piece and numerous openings at its side introduced. As carbolized water could not freely ooze out after injection by the single tube, a second opening was made, and a similar tube without side openings introduced. The sac could now easily be washed out. Now the whole part of the sac walls projecting over those lower parts of the sac adherent to the surrounding tissue was removed, and the remainder closed. The part towards the uterus was stitched by means of interrupted sutures lying close together; the portion lying toward the adherent intestines was closed by means of a glover's suture. Thus the adherent part of the sac was transformed into a well-closed cavity without connection with the abdominal cavity, and freely accessible from the vagina. The abdominal cavity was then cleansed until the sponges came out clean. After dropping the uterus back into the abdomen, it sank toward the median line of the pelvis. Upon its dark red surface, a characteristic picture of perimetritis, several single sinal tissue vessels bled quite profusely; the bleeding of these was stopped by laying upon them the neighbouring shreds of the perimetritic membranes. The abdominal wound was then closed.

Time of operation, 1 hour 15 minutes. Moderate collapse of the patient.

The progress of the patient showed at no time any reason for anxiety. Before the drainage-tubes fell out, a slight retention of secretion caused a little rising of temperature, which returned immediately to its course, the drainage being free. The discharge at no time was abundant or offensive. The patient was detained some days longer than necessary, as she had nothing to eat at home, and no one to take care of her. The sac is shrivelled to a small thick mass; lying beside the uterus at the pelvis, the opening still persists.

The fœtus was a rather moderately developed boy, with a large encephalocele on his occiput, and flat feet. The habitus was the normal one, the fœtus weight 185 grm., the placenta 131 grm. The resected parts of the wall proved to be, by microscopical examination, the tube with largely developed muscular elements and enormous vessels. The placenta was adherent to the uterine part of the tube, this being quite pervious.

On her dismissal the patient confessed that being a widow for ten years she had had sexual intercourse in December, 1880, and January, 1881, several times. This proves the correctness of the diagnosis concerning the age of the fœtus which we had based upon its development.

I am well aware that the development of the ovum and varieties of situations of extra-uterine pregnancy, and its connections with the surrounding tissues are of various kinds, and that the stage of development and the retrograde metamorphoses demand totally different plans of procedure. Still I think it worth while to consider in every case the treatment of the placental insertion by means of the ligature, and the removal of the upper portion of the sac, and the closure of the remainder which is to be drained through the vagina.

I am convinced that in this manner we decidedly simplify the operation for extra-uterine pregnancy and give a much better chance of recovery in these cases.

Frau Günther. Gravid. extr-uterin Tubarica.

	9/ap	10/ap	11/ap	12/ap	13/ap	14/ap	15/ap	16/ap	17/ap	18/ap	19/ap	20/ap	21/ap	22/ap	23/ap	24/ap	25/ap	26/ap	27/ap	28/ap
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Urin/Spontan

Stuhl

Irrigation des Sackes.

Irrigation des Sackes.

Blähungs-beschwerde.
Brechneigung.

Euphorie. Appetit.
Eisblase entfernt.

Ein Drain fällt aus.

Verbandwechsel. Letzte Stich-canal in unteren Wandwinkel eitert.

Zweite Drain fällt aus.

Aufgestanden

Bauchwunde festgeschlossen

Unterste Stich-canal trocknet ein

Geringe Secretion durch Vaginal loch

Bauchwunde geheilt

Geringe Secretion. Entlassung

The Reparative Surgery of the Genital Tracts.

Dr. Montrose A. Pallen, New York.

Nature of the accident or deformity requiring treatment—congenital or acquired.

Congenital causes explained by teratological facts based upon the study of embryology. An arrest of embryonic development, a fixation of a perfectly normal transitory state, results in a permanent anomaly materially affecting the future generative functions. The converse of this is equally productive of mischief, as an excessive developmental impetus results in a symmetrical, anatomical, and physiological correlation, and determines an increase of one or more of the factors of copulation, generation, or parturition evidenced by the presence of a double uterus and vagina, or a double vagina and single uterus, or the absence of the vagina with rudimentary uterus and well-developed ovaries, &c.

The faulty internections of the bladder, vagina, rectum, uterus, oviducts, and ovaries, indicate certain relations that are invariably attended with either dyspareunia, dysmenorrhœa, sterility, or dystocia.

Embryological facts cited to sustain these theories.

Study of the formation and functions of the vagina, the vagino-cervical spaces, vesico-vaginal and vagino-rectal connections, and the vagino-perineo-rectal fusions. When no increase either of vascular, muscular, or connective tissue takes place, the vagino-cervical fusions are mutually sustentative and supporting, and it is only when the correlation of place and order is destroyed by pathogenetic causes that we are called upon to treat post-pubertic lesions, whilst congenital (teratological) abnormities constantly require surgical interference.

Faulty implantation of the vagina upon the cervix uteri, the anomaly of excessive vaginal dimensions or pseudo-hypertrophy of the intra-vaginal cervix, is invariably associated with sterility, and usually with dyspareunia or dysmenorrhœa.

The converse of the above, or defective vaginal dimensions (the so-called infantile neck), is likewise inductive of dysmenorrhœa, sterility, and prolapse of both uterus and vagina. Methods of operating for the relief of these conditions with illustrative cases.

The acquired conditions resulting in deformity requiring plastic or reparative operations, ensue from parturition—some few are the sequences of pathogenetic causes unassociated with pregnancy. Illustrative cases and new methods of operation for the relief of cystocele, rectocele, uterine and vaginal prolapse and procidentia, as well as lesions associated with lacerations of the cervix or sundering of the perineum, or disruptions of the peri-uterine connective tissue.

On Laceration of the Cervix Uteri—Its Causes and Treatment.

Dr. J. Henry Bennet, Mentone.

During the last few years a new autoplastic operation has been advocated by several eminent gynæcologists, and especially by Drs. Emmet and Pallen, of New

York, for the cure of laceration of the cervix uteri occurring during parturition. This operation was not merely recommended in extreme cases, but for every form of laceration, severe or slight. Dr. Pallen had found two hundred cases of laceration among nine hundred patients examined for female disease at his New York Hospital, and appeared to think that all these cases demanded the autoplastic operation. According to these views a considerable portion of the uterine ailments to which women are liable—inflammation, menorrhagia, displacements, &c.—are to a great extent occasioned by this lesion, simple or double, and are only to be radically cured by a serious and delicate autoplastic operation. Dr. Henry Bennet quite agreed with these eminent authorities as regards the frequency of laceration of the cervix uteri during labour, and had pointed out this pathological fact long ago, in 1849, in his work on "Uterine Inflammation," second edition, page 25; and in the third, page 185, at still greater length. Although admitting that these lacerations during parturition were often the result of manual or instrumental interference, the cervix being healthy at the time, he then stated that the principal cause was previous inflammation and induration of the cervix non-softened during the latter period of pregnancy. This morbid condition at the moment of labour gives rise to rigidity of the os, of which it is the principal cause, and subsequently to laceration. He was led to this conclusion in an early period of his obstetrical career, between thirty and forty years ago, by careful observation. He was always in the habit of examining instrumentally, six weeks or two months after labour, all females who had presented morbid symptoms during pregnancy, parturition, or the puerperal state. He thus, early in life, became aware that many such morbid symptoms, including rigidity of the os and laceration, were connected with previous chronic disease of the cervix or uterus. He had attended, during a long gynæcological career, many hundred cases of laceration, slight and severe, without once operating. He always found that, under the treatment of the inflammatory state which attended them, the ulcerated edges healed, the indurated tissues softened, and a mere notch remained. He had never had irrestrainable hæmorrhage from this cause in subsequent labours, or extension of the laceration to the body of the uterus. He thought the operation a totally unnecessary one, unless in some extreme, exceptional case. He drew attention to the fact, that one generation of surgeons divided the cervix deeply to cure all kinds of uterine disease, and that now another sews up accidental divisions to cure precisely the same diseases.

DISCUSSION.

Dr. PLAYFAIR, London: I trust my colleagues will not be offended with me if I point out what has always struck me as a very remarkable fact and one greatly to be regretted, that although ten or twelve years have elapsed since Emmet brought his operation permanently before the profession, although our transatlantic brethren have published many important papers upon it, and declared their conviction that this operation is of enormous value in the treatment of many obstinate and otherwise incurable uterine diseases, it has up to this time received, I may safely say, no serious study at all at the hands of British gynæcologists. Surely the fact that men like Emmet, T. Gaillard Thomas, Pallen, Sims, Goodell, and many others, have recorded it as their deliberate conviction that in lacerations of the cervix in the puerperal uterus and their effects we have a common and fertile source of uterine disease, and in the operation of trachelo-raphé a ready means of cure, should have secured for their views an earnest and respectful study. Dr. Bennet

can hardly have forgotten how, thirty years ago, his own views were met with the most unjust and unmerited adverse criticism, how they were sneered at, and made the subject of jocular comment, of which he had just reason to complain, at the hands of those who had given them no proper consideration, and who had never tested them in practice; and surely the measure which he had meted to him he should be careful not to measure out to others. Nor can he with justice use the argument against trachelo-raphé, that it has been too frequently performed by its over-ardent votaries. The fact that a thing may be abused is no argument at all against its proper use. I observe that Dr. Bennet says, that one generation of surgeons was occupied in dividing the cervix, while another, to use his own words, "now sews up accidental divisions to cure precisely the same diseases." Nothing can possibly be more misleading than this statement. Surely there is all the difference in the world between a surgical incision of a nulliparous cervix for a definite congenital malformation, such as a conical cervix, or pinhole os, and a traumatic laceration in a puerperal uterus leading to imperfect involution, hyperplasia, and ectropion of the lining membrane of the cervix? How can any one possibly say that the two operations are intended for the cure of "precisely the same diseases." The plain fact is, and it is one rather humiliating to us, but it is one the accuracy of which I am perfectly satisfied of, that we have all of us been in the habit of overlooking these lesions, of mistaking ectropion and eversion of the lining membrane of the cervix for granular abrasion of its superficies. And so we will continue to do to the end of the chapter, until we get into the way of studying these cases with the duck-bill speculum and a pair of tenacula, which comparatively few of us are in the habit of using. Like the rest of us, I was strongly prejudiced against Emmet's operation. My first step towards conversion was having to recognize the frequency of these lesions by the use of this speculum. Even then I never tried the operation, believing, as Dr. Bennet does, that I could cure my cases by other means. About this time I was occasionally seeing an old patient, such as all of us have, who had been for many years the subject of what I should formerly have called chronic endometritis, with much cervical abrasion and hyperplasia. Under the use of the usual remedies she used to get temporarily better, but never well, and in a few weeks or months always turned up again. For the benefit of her health she took a voyage to New York, where she fell into what some might call the hands of the Philistines, had trachelo-raphé performed, and came back perfectly cured, with an apparently virgin cervix, and has remained so ever since. I then resolved to give the operation a fair and impartial trial. I have now operated some twelve or thirteen times with so gratifying results that I am quite prepared to endorse Dr. Thomas's statement in the last edition of his book, that Emmet's operation is one of the most important contributions to gynæcology ever made. I am not concerned to dispute the fact that the operation has often been done unnecessarily in cases that would have got quite well without it. I am equally convinced that there are many severe cervical lacerations which lead to no bad results whatever, and require no treatment. All I maintain is, that there are certain cases in which the operation is eminently useful, and to claim for it the serious and unprejudiced study which, I am sorry to say, it has never yet received in this country.

Dr. W. Goodell, Philadelphia: I feel under such great obligations to Dr. Bennet for his pioneer work in uterine therapeutics, that I dislike to disagree with him; but I am sure that he is wholly wrong in his deductions. I have operated

one hundred and seven times without a death, and, in the great majority of cases, with such beneficial results, that I cannot but believe in the efficacy of the operation. I admit that it is not needful to operate in every given case of cervical laceration. Whenever the rent is a mere fissure, like that of a bishop's mitre, with no eversion, an operation is generally not needed. But whenever there is a separation, and a curling over the lips of the fissure, there exists an ectropion of the mucous membrane, which cannot be cured permanently by local applications. By such treatment a thin silvery pellicle of epithelium can, it is true, be made to cover the eroded surface. But it will last for a short time only, for sexual intercourse and the vaginal attrition from locomotion will soon destroy it, and the erosion will become as bad as before. Caustics can no more cure an old cervical rent than they could a hare-lip. The true treatment is the radical operation. I contend further, that in cases of cervical laceration with ectropion of the mucosa, it is needful to operate if for no other reason than to prevent the development of cancer; for in my experience cancer of the cervix very rarely indeed occurs in women who have never borne children. But I have had repeated ocular evidence that it started from the constantly raw and irritated surface of a cervical rent. Acting upon this knowledge, I have on several occasions operated upon a lacerated cervix, when no constitutional symptoms were present, simply because cancer was hereditary in the patient's family.

Dr. Marcy, Boston: The valuable papers of Drs. Bennet and Pallen represent the antipodes of surgical treatment of cervical lesions of the uterus. Were it necessary I should be a ready defender of Dr. Emmet's operation for the laceration of the cervix uteri. Indeed I almost anticipated the operation myself. Fully fifteen years ago I denuded the granular eroded everted cervix and covered it over with neighbouring healthy mucous membrane with most excellent result. I called it a plastic operation for restoring a healthy mucous covering, but I did not discover the etiology of the disease. For this service and the placing of the operation upon a sound basis as a rational surgical procedure, our profession owe Dr. Emmet a deep debt of gratitude. The one point of importance to the inexperienced is the use of the lateral semi-prone position, the Sims' speculum, and drawing the uterus well downwards. The second is to remember that very frequently Nature has made an imperfect attempt at repair by granulation at the base of the cleft, and this must be completely removed. I have operated in about fifty cases, and have had one death caused by septic peritonitis. I have also known of two deaths in the hands of skilful operators. Thus the procedure cannot be said to be without danger, and the cases where it should be done are to be wisely chosen.

Dr. Nelson, Chicago: An important method of preventing laceration of the cervix was omitted in the paper—viz., the adjusting of the fœtal head, or presenting part, to the os uteri, or the os uteri to the presenting part of the fœtus, if it can be done; of course in many cases it cannot be done; but it should always be attempted. And further, the descent of the fœtal head should be regulated; if it descends too fast it should be restrained till the cervix dilates; if it is delayed it should be aided by the forceps before the pressure shall have produced sloughing of the cervix. I was glad to hear Dr. Bennet examined all his patients six weeks after delivery. I would suggest that he examine them also immediately after delivery; and as in lacerations of the perineum, so also of the cervix, if there are severe lacerations repair them at once. If the operation succeeds, as it has in all my cases—three—

the patient is cured of the laceration. But if the surfaces do not unite the patient is surely as well off as if no operation had been attempted, and the secondary operation may then be done in due time. If both inflammation and laceration are found to exist, the inflammation should be first removed, and then the laceration treated, by operation if needed; but never operate upon a lacerated cervix so long as there is extensive perimetritis. The patient's life may be endangered, and certainly her condition will not be improved.

Dr. PALLEN, New York: Dr. Bennet is wrong when he reiterates the opinion that inflammations and ulcerations precede rather than ensue from lacerations of the neck of the womb. Neither he, nor any other observer, ever saw an example of true ulceration of the cervix (as he would have us understand it), unless it was specific or concomitant with pregnancy. He has confounded a split cervix, having ragged edges, covered with cicatricial and papillary hypertrophic tissue, with the "ulcerated patulous os." The long cylindrical speculum, quite two or three inches longer than necessary, pushes the uterus more in retroversion or anteversion (as the case may be), and distends a lacerated cervix by crowding it into the lumen of the instrument, thereby creating a confusion which has cost many a woman the entire intravaginal cervix, in consequence of "melting" from potassa fusa. A simple bimanual palpation, and the sweep of the skilled finger, is enough to make a diagnosis of lacerated cervix; and, should one be still in doubt, the introduction of the Sims' speculum, and a couple of tenacula to draw the ragged edges together, very soon clears up all doubt about "ulcerated cervix with patulous os," which only exists in the minds of those who were indoctrinated by the honest convictions of Dr. Bennet forty years ago. I feel very positive in my views about this operation, because of a very special study of fifteen years. It was my good fortune to be the first who ever published anything about it (*St. Louis Medical and Surgical Journal*, 1866), when I believed I was the pioneer; although, as we all know, Dr. Emmet had done it four years before. Certainly, I was the first to recommend and make the "primary" operation—to stanch the bleeding vessels at the time of parturition by stitching the laceration with silver wire sutures. There can be no doubt about the utility and necessity of the primary operation—sometimes to save life by checking an otherwise fatal hæmorrhage, always to save the woman subsequent trouble and subsequent operation. I think that we should sew up all lacerated cervixes, not because of immediate necessity therefor, but because of anticipated trouble, particularly should there be any tendency to malignant disease, as pointed out by Professor Goodell. It is a good rule, almost an axiom of surgery, to always repair a solution of continuity. This is applicable to the uterine neck as well, and in all lacerations proper, not simple nicks, but in all true lacerations, even if the patient be not suffering from immediate or reflex symptoms, she will sooner or later succumb, and it is far better and much wiser to operate before her nervous system is broken down, when she can better withstand shock.

Dr. HENRY BENNET, Mentone: I thank my medical brethren warmly for the very flattering terms in which they have spoken of my labours in the early days of gynæcology. I do not, however, accept the accusation brought against me by some of the speakers, that I am now, in my later years, endeavouring to obstruct progress. My object has been, in the paper just read, principally to raise my voice against the extreme doctrines advanced by Dr. Pallen, in the essay read before the British Medical Association at Cambridge in 1880. The explanation now given by Dr. Pallen of

his views, doctrines, and practice renders it still more necessary that the profession should be warned that they lead to an unnecessary, and therefore unjustifiable, amount of severe surgical interference. In the memoir just read I have expressly reserved the class of cases with deep lobulated fissures in which I have proposed and used my potassa cum calce cylinders, to reduce inclination and hypertrophy—the class of cases more especially referred to in Dr. Emmet's work. I have not repudiated the autoplastic operation in these cases, stating that it was for younger gynæcologists to decide whether the operation is or is not the best procedure to adopt. No man would be more anxious than myself to see science in general, and gynæcology in particular, progress; and I am ever ready to accept new doctrines and new views, if founded on facts, and not on blind enthusiasm. I am ever desirous to give to other innovations, now I am getting old, the sanction I so long demanded in my youth. But I could not stand by and see such extreme surgical doctrines promulgated with authority, without making a protest, founded on a lifetime of clinical experience.

Memoir on the Treatment of Puerperal Hæmorrhage.

Dr. Barnes, London.

It will hardly be disputed that the treatment of post-partum hæmorrhage as taught in the schools and as generally practised is to a great extent empirical. It will be generally admitted that many women still perish annually from post-partum hæmorrhage, and that of those who survive many suffer from secondary or remote effects.

But whatever diversities of opinion may exist as to the foregoing propositions, we shall be unanimous in the determination to esteem deaths from post-partum hæmorrhage as preventible, and to strive to avert these sad calamities, which shock our common humanity and cast reproach upon our art.

The treatment of post-partum hæmorrhage finds its rational basis upon a careful study of the conditions which precede and attend it.

The antecedent conditions date from the pregnancy itself, and some even precede the pregnancy. Upon these I cannot dwell. Enough to call to mind the hæmorrhagic diathesis with which some persons are afflicted. Such subjects occasion the greatest solicitude when they become pregnant. Their idiosyncrasy intensifies the conditions of pregnancy which predispose to hæmorrhage.

The study of the predisposing causes proper to pregnancy naturally invites attention to prophylaxis. This also I must pass by.

It may be stated generally that so long as the several functions are executed in harmony, so long as the just equilibrium is maintained, and this presupposes healthy structure, hæmorrhage is not to be feared. But when this harmony, this right physiological co-ordination, is disturbed, hæmorrhage and other evils may occur. For example, if under excessive nervous and vascular tension the liver and kidneys are overtaxed, noxious matter accumulates in the blood, albumen appears in the urine, and one of the most powerful causes of hæmorrhage is produced. The influence of this cause has been most clearly established by Blot and Chantreuil.

It follows as a physiological necessity that the excess of blood required for the work of pregnancy must be got rid of when the pregnancy is at an end. This riddance may be called the "*physiological puerperal hæmorrhage.*" It is a form of

menstruation, or at least may be compared to menstruation. Labour takes place indeed, at a menstrual epoch.

This discharge of the excess of blood is attended, as in menstruation, by a rapid discharge of the excess of nervous energy stored up for the supreme effort of labour. The security against hæmorrhage beyond the physiological limit depends mainly upon the maintenance of the due order and just equilibrium of the factors concerned in labour.

The ovarian stimulus ascending to the spinal centre calls back to the uterus nervous energy to induce a series of muscular contractions. This nervous force comes in a succession of discharges or shocks. Presently, the work once started, the stimulus to reflex nervous action comes from the uterus itself. This organ may be said to regulate the supply needed for its own want. The uterus goes on contracting with increasing energy, calling more or less upon the auxiliary force of the voluntary muscles, until the supreme moment of the expulsion of the child. This supreme effort is followed even in the most healthy subjects by a stage of comparative exhaustion. Rest is necessary to generate new nerve force to cast the placenta, and to close the vessels torn across on the detachment of the placenta. During the expulsion of the placenta the physiological hæmorrhage takes place. This loss is necessary in order to restore the balance between the circulatory and the nervous systems. This balance effected, there is a sudden diversion of blood-flow from the pelvic vascular system. By a natural process similar to, and physiologically associated with, that which is observed in the new-born child on the establishment of respiration, by which the blood-current is all at once turned off from the hypogastric uterus and the umbilical cord, the mother's blood is turned off from the uterus. As a matter of precaution we tie the cord, and Nature does the equivalent in tying the uterine vessels by muscular ligatures.

If we analyse the conditions which most commonly lead to post-partum hæmorrhage, we shall discover that they are those which mark the disturbance of the equilibrium, or which disorder the regular sequence, of the phenomena which mark the normal parturient process. Thus, in tedious or obstructed labour the presenting part of the child has been pressing prematurely or too long long upon the lower segment of the uterus. The expulsive force exerted by the fundal polar region, overtaxed in its fruitless efforts to drive the child through the cervix, presently becomes spasmodic, and the irritated lower polar region is excited to contract prematurely. The perverted action thus induced is not always allayed after the expulsion of the child, but continues; the regular equable tonic contraction of the uterus necessary to expel the placenta and close the placental site does not take place, and so hæmorrhage results. A similar disturbance takes place when precipitate or unskilful operations are performed to extract the child or the placenta. Premature escape of the liquor amnii, permitting the descent of the presenting part of the child upon the cervix before it has undergone adequate dilatation, is a common cause of perverted polarity, and thus of hæmorrhage.

A disturbing cause that greatly disposes to hæmorrhage is the free administration of chloroform during the labour. The just correlation of the forces which concur to effect delivery, the exact physiological sequence of the several phenomena, are undoubtedly affected. Hence, whenever anæsthesia is resorted to we should be prepared to meet hæmorrhage.

In the presence of hæmorrhage we have to take a rapid survey of the attendant conditions. First, is the placenta still in utero? It is superfluous to insist upon the well-recognized law that it must be expelled or extracted. But how to do this

is a matter of great importance. I must interrupt my argument for a moment, in order to dispose of this point. For want of due attention to the physiological sequence of the phenomena of labour, a method of dealing with the placenta, excellent in its due place, has been much abused. I refer to what has been called the "expression of the placenta." If this manœuvre be resorted to prematurely—that is, immediately after the expulsion of the child, without allowing the interval of rest that Nature craves for the regeneration of nerve-force—we run imminent risk of setting up irregular spasmodic action of the uterus, and thus of causing hæmorrhage. And there is another risk—that is, of causing inversion of the uterus, the pressure upon its fundus being exerted upon flaccid paralyzed walls. This, again, disposes to hæmorrhage. I affirm both these propositions from clinical observation. The proper proceeding is, if we fear hæmorrhage, to follow the uterus in its last contractions for expulsion of the child by steady uniform pressure with the hand, or binder, to maintain gentle pressure upon the uterus after the child is expelled, and until the return of uterine contraction under restored nerve-energy.

In this manœuvre we must alike avoid attempts at forcible "expression" and "kneading." If at the end of a few minutes we find due contraction does not set in, and especially if there be any sign of hæmorrhage, or of unsteadiness of the heart's action, we may proceed to "expression" in the way to be presently described.

But this argument must not be overstrained. We must not permit it to paralyze action, to encourage a policy of inaction. Nature may be strong even in her weakness, but she may be weak even in her strength. In either case she may profit by well-directed help.

What are the natural means of arresting hæmorrhage?

1. The most universally recognized is the regular tonic contraction of the uterus, the contraction especially preponderating in the fundus and body of the uterus, including the placental site.

2. This effective uterine contraction, by closing the vessels on the inner surface of the uterus, helps to turn away from the pelvic vascular system the current of blood.

3. Coagulation of the blood in the uterine arteries especially. But this is of little avail, unless the uterine fibre contract in some degree to reduce the calibre of the vessels.

4. The lowering of the heart-impulse, thus reducing the driving-force which impels the blood to the pelvic vascular system. In some cases, as in extreme shock, or after great loss of blood, the heart-impulse is lowered to the point of syncope, and thus for the time hæmorrhage may be arrested; the temporary blood-stasis may favour closure of the uterine vessels by clotting, and when a renewal of nervous force takes place, it may be directed to the uterus, inciting this to contract. This is the history of many cases of post-partum hæmorrhage. The patient recovers under this last and desperate resource of Nature. The experience of this fact opens the way to doubt the efficacy of the treatment which the physician had employed. The question will arise, Would not Nature have been adequate without the physician? We must take this argument of doubt into account in estimating the value of our therapeutical proceedings.

As a guide to the choice of remedies, we may usefully divide hæmorrhages into stages, or degrees, rising in severity from the simple physiological hæmorrhage to the extreme degree in which extinction of life is threatened. These degrees may be defined for the purpose with tolerable precision by certain physiological phenomena,

or consequences. We cannot take as a test the quantity of blood lost, since the effect of a given loss varies with the individual.

The *first degree* of hæmorrhage is marked by the full retention of the diastaltic function, this giving a ready response to the customary excitors.

The *second degree* is marked by considerable diminution of the activity of the diastaltic function, this responding with difficulty to the ordinary and extraordinary excitors.

The *third degree*, or effect, of hæmorrhage is marked by the suspension or extinction of the diastaltic function. It is commonly indicated by vomiting, tendency to syncope, yawning, gaping, flagging pulse, rapid, occasionally intermittent, to the point of suspension, as far as feeling the radial artery is concerned, although the heart may still be felt beating. The rapid draining is also attended by loss of temperature, a phenomenon important to remember.

In the *first degree* in which the diastaltic function is in full power, we naturally call upon it to arrest the hæmorrhage by inducing contraction of the uterus. The call is made in three ways—1, by the brain, by restoring confidence, and thus getting emotional influence in our favour; 2, by appealing to the spinal centre through impressions upon the peripheral nerves, as by putting the child to the breast; by the sudden application of cold to the abdomen; by irritation of the os and cervix, a proceeding not of much value; by compression of the body of the uterus by the hand outside, a proceeding approved by experience as trustworthy in the great majority of instances; by cold or heat applied to the interior of the uterus singly or alternately, as Tyler Smith suggested and practised.

The first object to be attained before these or other remedies can act efficiently is the emptying of the uterus. This should be done by expression applied externally—rarely or never by passing the hand inside the uterus, unless the retention of the placenta be due to adhesion.

In this first degree these and allied mechanical agents are singularly aided by certain medicinal agents. Thus, where we find the hæmorrhagic pulse giving evidence of increased heart action, and colicky, spasmodic pains giving evidence of perverted uterine action, the greatest advantage may be gained from moderate doses of opium, digitalis, and bromide of potassium. In this first degree also ergot, whether through its centric virtue in increasing the energy of the spinal cord, or by its diffused virtue of inducing tonic contraction of the vessels, is often of especial value. This agent is, I believe, greatly enhanced in efficacy by combining digitalis with it. Quinine also acts in a similar manner, and may be usefully combined.

In the *second degree* the difficulties have increased. The diastaltic function is impaired. We must nurse it, not force it or overtax it, as is too often done. Ergot, quinine, and digitalis may still be of service; but it cannot be too earnestly enforced that ergot does not give strength to the spinal cord, that it only stimulates it, acting as the whip or spur to a jaded horse. If, then, ergot be pushed beyond a moderate extent, it tends to exhaust nervous power. It is not uncommon to see its use under this condition followed by vomiting, by spasmodic uterine action, or even by depressed action of the heart. Sometimes, indeed, it seems to have no effect at all. The rule I would propose as to the use of ergot is the following: Give ergot early after the expulsion of the child where there is reason to anticipate hæmorrhage. Repeat it as soon as hæmorrhage appears; but if it fail to act quickly do not repeat it. Proceed at once to more trustworthy methods.

Compression of the uterus by the hand outside will probably always be the

most trusted. The best way of applying this is to lay the patient upon her back near the side of the bed, so that the physician standing can lean well over and thus economize his strength by adding weight to muscular exertion. One or both hands are then so applied to the body of the uterus as, first, to press the anterior wall upon the posterior, thus obliterating the cavity; and, secondly, to press the whole uterus back upon the sacrum, thus compressing the aorta and large pelvic vessels. So long as this pressure is kept up it is obvious that little blood can be lost. Time is gained for the action of restoratives, as brandy and cinnamon; and thus for the recovery of diastaltic power. Then, if the uterus fails to contract firmly and to maintain the much desired ball-form, we may call upon the diastaltic function again by means of cold. A piece of ice is the best form. This may be laid for a minute upon the abdomen, to the vulva, or inside the vagina. In severe cases this is not of much use, and it should not be persevered in. A similar rule to that laid down in the case of ergot applies not less strongly to the use of cold. If the diastaltic function is disposed to respond to the excitation it does so promptly. If it be not so disposed—that is, if it be too feeble—cold can only act as a depressant, and thus will actually increase the hæmorrhage. Therefore, unless cold act speedily, abandon it. Especially is it necessary to warn against applying cold by means of water douches or wet cloths to the abdomen. Depression is sure to be induced, even if the hæmorrhage stops, and the patient runs imminent risk of septicæmic fever, peritonitis, pleuritis, or pericarditis. The temperature of the body falls enough by the mere effect of the hæmorrhage.

The most rational and the most efficacious way of applying cold has been long known: it was practised by Levret, by introducing a lump of ice into the uterus itself. If this fail, do not repeat the experiment. It is now preferable, to economize nervous power, to try the effect of heat upon the diastaltic centre. This is best applied by injecting the uterus with hot water at 98° Fahr. to 100°. The uterine tube is carried into the uterine cavity, and a stream of water is gently pumped through during a minute. Carbolic acid, in the proportion of 1 in 100, may usefully be added. Not weighing for the moment its antiseptic property, I can affirm that it adds to the particular virtue of the hot water as a diastaltic excitor some astringent action.

If the hot water fail to excite contraction, we may in like manner run a stream of cold water through the uterus, and again a second stream of hot water. During all these manœuvres the uterus should be steadily guarded by external pressure.

If the uterus still relax and the hæmorrhage go on, it is obvious that the diastaltic function is failing, that the case is merging into the *third degree*, in which we can no longer rely upon the methods which appeal to the diastaltic function. What remains to be done? At this point, in this emergency, it is not too much to say that the common practice has been to persevere blindly in the use of those means which depend for their efficacy upon diastaltic power, and which, that power lost, have become worse than useless, positively and negatively mischievous.

The first question—the condition of the patient under which iron may be usefully applied—is already answered, or nearly so. I assume that it will be granted that women do occasionally die of hæmorrhage, in spite of the assiduous use of what are called "the ordinary remedies." This granted, it follows that we are justified, more than justified, in trying to avert death by using extraordinary remedies, even if these remedies entail some special danger. The moment to select for calling up the reserved forces is a most anxious one, and not always easy

to determine. I have already insisted upon the physiological fact, that a time comes when the diastaltic function is for the time exhausted, and that with this exhaustion the time for the reliance upon remedies that depend for their action upon this function ceases. Logically this is the time when remedies which depend for their action upon properties still existing, has come. But now there arises a serious reflection. The suspension of the diastaltic function reveals an extremely critical condition. Life is already in imminent danger; and it may well be that any measure the physician may adopt may be too late; or that the shock inseparable from the use of the remedy may precipitate death. The evidence of many accomplished observers, however, demonstrates that the proper use of iron in this critical condition has been followed by arrest of hæmorrhage and recovery. Here then it ought to be used. But ought its use to be restricted to this condition? The lapse from diastaltic power is often rapid, scarcely perceptible. When on the verge of suspension, one gush of blood may precipitate the patient into fatal paralysis, and we shall have lost our opportunity. The indication then is clear to anticipate this suspension, and to call in an effective remedy early enough to give it the opportunity of working under the greatest advantage. An essential condition is that the uterus shall be empty when the iron solution is introduced. Another condition, less essential but useful, is, that the uterus during the introduction of the iron shall be firmly compressed by the hand outside. It is to the neglect of these two conditions that failure has in some cases resulted.

The second question—the form and manner of application of iron—is one of great practical interest. The iron must be used in strong styptic solution, reducing it below the caustic power. We have unfortunately experience of the caustic power. For, like every new remedy, this one has had to contend against the mishaps due to ignorance of the principles upon which its right use is based.

Can we define the strength of the solution precisely? For practical purposes we can. It should be strong. A very weak solution can have little styptic power; and safety does not lie in weakness. It is most convenient to carry the iron in the solid form. It may be carried in bottles, each containing a definite weight of the perchloride, say an ounce. An ounce may be dissolved in ten ounces of water to make the solution required.

At first, and for some years, I injected it in a gentle stream into the uterus, taking care to place the uterine tube fairly touching the fundus, first washing out the cavity by hot or cold water, for the double purpose of washing out the uterus and of giving a last chance of obtaining contraction by appeal to the diastaltic function. One death occurred. It was in consultation with a very able practitioner, who had used the ordinary means to arrest a furious hæmorrhage. The condition of the patient was critical; it seemed that her last chance lay in the iron solution. The injection was quickly followed by severe abdominal pain, shock, collapse, and death. That the death was accelerated by the injection I cannot doubt; that the woman would have recovered had the injection been withheld, I do doubt. But this question of the dangers incident to the iron will be discussed.

In the third degree it is to be noted that the diastaltic function, although suspended, should not be considered as extinct whilst life endures. If the hæmorrhage can be stayed for a while, the diastaltic power will be regained after rest. But it is dangerous in the extreme to trust to this return. It returns in feeble, fitful waves, quickly ebbing away, quite unable to produce permanent closure of the uterine vessels.

Arrived at this point, we must direct our treatment to other forces that Nature

still holds in reserve. We still may hope that the uterus may respond to appeal made to its quasi "vis insita," to that power of contraction which it holds for a while, at least, independent of spinal nervous influences. We may still hope that the tissue of the inner surface of the uterus may be constringed under styptic application. We may still hope that the blood-vessels may be obturated by coagulation of the blood at their open mouths. We may hope that if only a few minutes' respite from hæmorrhage be thus secured, we may have time to use restorative measures, and that with returning strength tonic muscular contraction may take place.

First in order of this new class of remedies which now present themselves to our service is galvanism, or faradization. This agent acts directly upon the uterine muscular fibre, exciting its peristaltic or inherent contractility more or less independently of spinal or diastaltic influence. If this agent could be depended upon to maintain steady uterine contraction, if even for a few minutes, it would render invaluable assistance. But can it be depended upon? Many years ago Sir James Simpson tried it, and obtained only negative results. The experiences of Dr. Radford and myself justify the conclusion that Sir James either did not use a fit apparatus or did not apply it fitly. Certainly faradization will excite the uterus to contract. In my experiments I never failed to elicit this result. But, unfortunately, there arose serious practical objections. Failing then the agents which provoke muscular contraction of the uterus, we are reduced to the choice of agents which directly constringe the inner surface of the uterus and the mouths of the vessels. This power we find in styptics, and amongst these we have to choose. Which is the most trustworthy? The two which compete the most forcibly are iron-salts and iodine; and it is upon their relative merits that I propose more particularly to dwell. They are so greatly superior to all other styptics of which we possess any clinical experience, that they challenge all our attention. From time to time vinegar, lemon-juice, and other agents have been suggested and tried. It is not, I think, unjust to say that they are of feeble efficacy, and not to be trusted in the serious cases we are now dealing with.

I have subjected iodine and iron to the closest clinical tests. In cases of otherwise uncontrollable flooding, I first injected hot water, the flooding persisted: I then injected strong styptic solution of iodine, the flooding persisted: at last I injected solution of iron, and the hæmorrhage was instantly and permanently stopped. If it could be proved that iodine and iron were equally efficacious in arresting hæmorrhage, the decision would have to be given in favour of the agent which was the safer. But since iodine fails in the first essential—that of efficacity—it is, I submit, excluded from the competition. We may, therefore, fix undivided attention upon the value of iron. The questions to determine are—First, the conditions of the patient under which it may be usefully applied; second, the form and manner of its application; third, the dangers incident to its use; fourth, the means of obviating these dangers whilst securing the sought-for hæmostatic action.

We enter upon the third question, What are the dangers incident to the use of iron injections?

These dangers may be divided into two classes—namely, those common to all kinds of injections, and those peculiar to iron injections.

To dispose of the common dangers. The first is shock. This is the most immediate danger attending fluid injections into the uterus, and the manipulations necessary to carry them out. We know that the simple introduction of the hand

into the uterus has been quickly followed by death. We know that women have died during the external grasping of the uterus. Women have died under the injection of cold water.

The truth is, that in the state of extreme depression of collapse, or approaching collapse, induced by hæmorrhage, and which gives the indication for treatment, any kind of physical disturbance entails some danger. The severity of the shock bears less relation to its cause than to the condition of the patient.

If we define simple shock as an overpowering, stunning effect, paralyzing the heart, produced upon the nervous centres through some sudden impression made upon the uterus, it is not easy to understand how the injection of cold water, which by itself can precipitate death under the conditions postulated, can be made more dangerous by carrying in solution iron in styptic proportion. Certainly I know of no evidence to show that it is more dangerous.

But there are other forms of shock not so simple. For example, along with the fluid injected, air may be driven into the uterus, and thence into the veins; and if air in any marked volume enter the heart, rapid death in all probability ensues. Death has in this manner followed the intra-uterine injection of water practised on the gravid uterus in order to provoke premature labour. It may be in this manner that death has been produced by the intra-uterine injection of water to arrest post-partum hæmorrhage.

Is this accident equally or more likely to happen under the injection of water plus a styptic proportion of iron? There are reasons *à priori* why it should be less likely to happen. The styptic application to the inner wall of the uterus, corrugating it and clotting the blood, tends to close the gaping vessels more than plain water can, and thus lessens the chance of the entry of air. Without denying the possibility of this accident under iron injections, we still want evidence that it has happened; and we may reasonably conclude that it is not an accident that constitutes a special objection.

There is another way in which injections may cause shock—that is, by some of the fluid being driven along the Fallopian tubes into the peritoneal cavity. Cases have been recorded, and I have seen several, in which the injection of water, or of a weak solution of zinc, into the vagina in non-pregnant women has been followed by alarming shock. In some of these cases it has been ascertained that the cervix uteri was patulous, and that the fluid was received into the uterine cavity. Thence it was driven through the tubes by a process which I have described elsewhere.* It is also probable that under violent compression and difficult exit along the tubes, some of the fluid may be forced into the tissue of the uterus and into its vessels. The famous Florentine experiment, and the experiments of Dr. Matthews Duncan with hydraulic pressure upon dead uterine tissue, suggest this hypothesis. Now inasmuch as the iron solution is more irritating to the uterus than plain water, it appears that this accident is more likely to happen when iron solutions are used. Still it must be rare. Probably there are ways of avoiding it, and it is desirable to bear in mind that the mere contact of iron in solution with the peritoneal surface is not in itself necessarily injurious.

Several cases of sudden death have followed the injection of very small quantities of iron solution into veins of the face and neck. In some of these it has been demonstrated that the iron has been carried directly to the right side of the heart, there causing coagulation of the blood. It will be noted that in these cases two

* "Diseases of Women."

conditions are present which singularly favour this transit of the iron injection into the heart. 1st. The proximity to the heart of the vessel injected, and the comparatively unobstructed state of the channel. 2nd. The aspirating or suction force exerted by the chest. The conditions obtaining in the case of intra-uterine injections are much less favourable to invasion of the heart, whether by air or liquids. Even where the uterus is in complete inertia, the sinuses lying in strata do not offer a direct channel towards the heart; and the slightest degree of muscular contraction at once opposes a barrier to centripetal advance. And the suction or aspirating force, although probably in some cases existing, cannot be so active as in the case of the veins of the face or neck. Poisseuille denies abdominal suction. I myself have no doubt from clinical observation that there is a suction or vacuum-force in operation in some cases as far as the cavity of the uterus. Beyond that cavity I am inclined to accept the negation of Poisseuille. But the iron may enter the veins or sinuses of the uterus. This seems a real danger. Iron has been found in these vessels. Wherever iron comes into contact with the blood it coagulates. If the inner wall of the uterus is fairly corrugated, the clots thus formed are limited to the surface, and even help to block the vessels against the further extension or travelling of the iron into the circulation. And presently when the uterus regains contractility the obturating clots are squeezed out into the uterine cavity, as are the simple clots formed where no iron is used.

But if the iron may undoubtedly travel further along the vessels, and coagula may form in the sinuses in the walls of the uterus, the thrombi thus artificially formed may break up, or small emboli may result, and these carried to the heart may be pumped into the pulmonary or cerebral arteries, and cause rapid death. I am not, however, aware that a death from this cause has been demonstrated. The only one in which it has been suggested, is one described by Dr. Atthill, and this eminent clinical observer says the woman was at the point of death when the injection was resorted to. The "general thrombosis throughout the entire venous system observed could hardly have been produced in the moment or two which appear to have elapsed between the injection and death." Moreover, although we might understand that iron in its course along the iliac veins and vena cava to the heart would coagulate the blood in its track, it is not easy to understand how it could account for thrombosis in the "entire venous system," including necessarily many veins which the iron could not have entered. The conjecture that the thrombosis in this case was post-mortem is probably correct.

The coagula dwelling in part in the uterine sinuses may undergo decomposition where they are exposed to the contact of air and decomposing fluids in the uterine cavity—hence septicæmia may ensue.

That the first and immediate effect of iron upon blood is to cause coagulation is certain. A secondary property of iron is antiseptic. But this effect does not endure. All who have injected iron into the uterus are aware that the discharges become offensive. But Dr. Connel (*Edin. Med. Journ.*, vol. xx.) has shown by experiments that coagula produced by the action of iron-salt are peculiarly aseptic, and resist putrefaction change for an extraordinarily long time. Assuming, then, that the immediate object sought—namely, the arrest of the hæmorrhage—is attained, two secondary dangers are still to be apprehended. The clots softening and breaking down may give rise—1st, to the carriage of small emboli into the circulation; 2nd, to impoisonment of the blood by the absorption of the septic fluid or ichor proceeding from decomposition. Both these dangers I believe are real. But

I am also persuaded that the latter danger, that of septicæmia, has been exaggerated, that deaths have been attributed to it which may be explained wholly or partly by other hypotheses, and, as will be shown further on, that this danger may be generally, if not always, excluded by proper precautions.

In the first place, it must be borne in mind that septicæmia is especially prone to happen after post-partum hæmorrhage. The conditions which predispose to hæmorrhage, as debility, albuminuria, defective contraction of the uterus, all predispose to septicæmia. And when to these predisposing causes is added the hæmorrhage itself, the liability to septicæmia is greatly intensified. The formation of septic matter in the uterus is promoted, and the absorption is rendered far more active. Hence it is that septicæmia is so often observed after severe hæmorrhage, treated by what are called the ordinary methods, and of which iron-injections form no part.

It cannot reasonably be expected that the use of iron-injections will altogether obviate this danger; and yet cases of death from septicæmia following this treatment have been made the basis of reproach against it, as if this accident were inherent and special. The true question is, rather, whether septicæmia follows iron-injections more frequently than it does severe hæmorrhage treated by other means? Nor would answers in the affirmative be conclusive against iron. For the iron has mostly been used in cases so severe that other means had previously failed, and where the presumption was great that death would have ensued at once from the hæmorrhage. In some of these cases, then, it is fair to assume that the first effect of the iron was to rescue the patients from death. It is surely right, then, in cases otherwise desperate to use a remedy which will have this effect, and to hope that by subsequent care we may avert the consecutive dangers that may threaten the patient. Is it not absurd—worse than absurd—to withhold a remedy that may rescue from instant death from one cause—hæmorrhage—lest by saving her she may possibly die from another cause, septicæmia?

The cases in which death has followed the intra-uterine injection of iron have been subjected to rigorous analysis by Dr. Frederick Pollard (*Brit. Med. Journ.*, 1880). His conclusions support those above set forth. They may be summed up in this one proposition: The dangers of hæmorrhage are quick death or remote disease, whilst the immediate and remote dangers of iron-injection are rare.

I omit discussion of transfusion, which has given brilliant results. This measure belongs strictly to the restorative treatment. I will only state that arrest of the hæmorrhage must precede transfusion.

The fourth question. The means of obviating the dangers incident to the use of iron to the interior of the uterus?

We may conveniently take the dangers enumerated seriatim, and consider how each may be most effectively avoided or mitigated to minimize shock. The necessary manipulation should be as gentle as possible. A stream of hot water at 100° F. should first be slowly propelled through the uterus to wash out clots and fluid blood. Thus the uterus being grasped by the hand of an assistant externally so as to reduce its bulk and the internal superficies, three to four ounces of the iron-solution, at a temperature of 80° F. should be slowly propelled, care being taken that the cervix be freely patulous, so as to allow free escape of the fluid injected.

To minimize the danger of air being propelled into the veins, 1st, the uterus should be compressed as described; 2nd, care should be taken to expel all air from the syringe before introducing the uterine tube; 3rd, the uterine tube should be

gently slipped into the uterus, simply guided by a finger, avoiding opening the cervix by the hand.

To minimize the danger of some of the fluid being propelled along the tubes, the precautions already advised will go far to attain this object.

To meet the more serious danger from iron being carried into the circulation and causing venous and cardiac thrombosis, the preceding precautions still offer the best protection. If the uterus is held firmly grasped during the injection, the influence of suction-force is diminished, and the compressed vessels are rendered less permeable.

By these precautions the danger is really reduced to a vanishing point.

To meet the dangers that may arise, 1st from the clots formed in the sinuses breaking up and giving off emboli, the preceding precautions will be of service. Further means are: The maintenance of steady pressure upon the fundus of the uterus by compresses and bandage; the subcutaneous injection of ergotine soon after labour; the regular administration by mouth or injection into the cellular tissue of quinine and ergot continued for several days (this practice I have for many years past followed in every case of labour, whether iron has been used or not). Lastly, care in keeping the patient well nourished.

To meet the danger of septicæmia, whether from the decomposition of the thrombi in the uterine vessels or in the uterine cavity, we must, in addition to the foregoing precautions, carefully syringe out the uterine cavity once or twice a day from the first day, with about eight ounces of carbolized water in the proportion of 1 in 50.

By the scrupulous adoption of these measures we may hope to so far minimize danger as to leave the patient little else to contend against than the inevitable consequences of the hæmorrhage.

Various means have been adopted, however, to lessen the risks enumerated by avoiding the use of injecting apparatuses to carry the iron. Early in my experience I contrived large vulcanite tubes, perforated at the uterine end, which was loosely packed with sponge, soaked with the solution. The tube introduced into the uterus, the fluid was then squeezed out by a piston-rod.

Other contrivances have been used. Thus Dr. Roper uses a rod clothed at one end with sponge, like the swab of a cannon, which being saturated with the solution, is passed into the uterus, and the entire inner surface is wiped over with the iron.

Or the rod may be hollow, and perforated with large holes at the end covered with sponge, so that the fluid can be driven into it by a piston-rod after the swab-end is in the uterus. I think these swabs, which must necessarily be bulky, are on that account objectionable. They are not so convenient in use as the syringe; since the preliminary use of the syringe is all but indispensable to make a last appeal to the diastaltic force, and to ensure that the uterus shall be clear of blood. I believe that with due observance of the rules I have enjoined, the syringe will, after all, be found the most effective and the safest instrument. The application of masses of sponge soaked in styptic solution, and carried by the hand into the uterus, must, I think, be condemned as a rough and inefficient expedient, calculated to increase shock, and favour the admission of air into the uterus.

Summary in Propositions.—The physiological prevention and arrest of post-partum hæmorrhage depend upon—

Muscular contraction of the uterus.

Diversion of the blood-current from the pelvic region.

Lowering of arterial tension.

Retraction of the uterine blood-vessels into the uterine substance.

Coagulation of blood in the uterine vessels.

The fulfilment of these processes depend upon—

The ordinary course of the labour.

The integrity of the nervous centres: ganglionic, spinal, cerebral.

Upon sound quality and just volume of the blood.

Sound structure and right action of the heart and blood-vessels, liver and kidneys, skin.

The rational treatment of post-partum hæmorrhage is based upon the restoration of the equilibrium of the functions, and the appeal to the forces upon which those functions depend.

There is a physiological hæmorrhage consisting in the discharge of the excess of blood provided for the nutrition of the fœtus and uterus.

In the progress of hæmorrhage, three stages of decadence of the nervous forces upon which the maintenance of the just equilibrium depends are observed:—

1.—Disordered or perverted diastaltic force, marked by spasmodic or irregular uterine contractions.

2.—Partial failure of the diastaltic force.

3.—Suspension of the diastaltic force.

The selection of means to control hæmorrhage must be governed by the particular stage of nervous decadence, or power present. In the first stage, cold, heat, local compression, ecbolics, and the ordinary means which evoke diastaltic response are available. In the second stage, the same means may still be available; but it will sometimes be necessary to resort to the agents which are more especially indicated in the third stage. In this last stage is the crisis when styptics are imperatively called for.

The increasing gravity of the case calls for new remedies. Some dangers are common to all modes of treatment, and in all the stages of hæmorrhage.

It is not proved that the common dangers—as shock, entry of air into the vessels, fluid into the peritoneum, thrombosis, embolism, septicæmia, are especially likely to follow styptic injection. It is certain that these accidents are rare, and also, that by definite precautions, they may be still further reduced.

It is abundantly proved that many lives have been rescued by styptic injections where other means had failed.

On the Prevention and Treatment of Post-Partum Hæmorrhage.

Dr. Thomas More Madden, Dublin.

Death from flooding after delivery should be considered as a generally preventible accident, and in the more perfect midwifery practice of the future will doubtless be entirely unknown. In their zeal to contribute to their complete deliverance from what is still, with too much reason, an ever-present terror to obstetric practitioners, some eminent modern authorities, who advocate the employment of perchloride of iron and other remedies on the least appearance of post-partum hæmorrhage,

appear to have an exaggerated idea of the frequency of severe flooding, and to forget that some slight loss of blood is a natural termination of labour. When due care is taken in the way of preventive treatment fatal hæmorrhage is a comparatively rare accident. Thus, in a practice of upwards of twenty years in various countries, tropical as well as European, and during his connection with the largest lying-in hospital in Great Britain, Dr. More Madden has only seen one case of death from hæmorrhage after child-birth.

We may almost always obviate, although we cannot yet always arrest, flooding. The majority of cases of post-partum hæmorrhage are met with in women who have previously borne children, and the probability of its occurrence is in proportion to the number of the patient's previous confinements. In the Dublin Lying-in Hospital, of 89 cases of this kind, in 65 the patients were multiparæ. In such cases when we have any reason to anticipate hæmorrhage, the membranes should be ruptured as early as possible during labour, so as to allow the uterus to contract gradually and firmly; and a dose of ergotine, or a drachm of the fluid extract of ergot, should be injected hypodermically before the head comes to press on the perinæum. As a prophylactic of hæmorrhage, the efficacy of a course of any astringent preparation of iron given during the last months of pregnancy is unquestionable.

Hæmorrhage after delivery, from laceration of the cervix uteri, is more common than was formerly the case. This arises from the abuse of the long curved forceps which is now too often needlessly resorted to before the full dilatation of the os. Hence, Dr. More Madden thinks that in this way the abuse of the long forceps does as much harm as the use of the short, straight forceps does good in judicious hands.

Reviewing his experience of the various modern methods of arresting flooding, the writer says that the injection of hot water into the uterus in such cases is most uncertain in its hæmostatic action, and is only useful in cases of extreme depression of the vital powers from excessive hæmorrhage, and after the failure of other remedies. Neither is the injection of cold or iced water to be depended on. The injection of a strong solution of perchloride of iron, although generally effectual as a styptic, is so hazardous, from the risk of its causing metro-peritonitis, that the writer now seldom resorts to it. But he strongly recommends what he regards as a most effective and comparatively safe method of arresting post-partum hæmorrhage—namely, the introduction of a sponge soaked in a solution of the perchloride of iron which is to be passed into the uterus (grasped in the accoucheur's hand), and retained there until a firm contraction is produced, by which the sponge and the hand in which it is held are expelled together from the uterine cavity, and the flooding is stopped. At the same time he lays great stress on external manual pressure over the uterus in all cases until contraction is produced; and he points out the possible danger of thus introducing the hand into the uterus, unless when rendered necessary by severe hæmorrhage. With regard to the treatment of collapse from hæmorrhage after delivery, he regards transfusion, as now practised, as almost useless in the great majority of cases of the emergency for which it is recommended. Instead of transfusion, he advocates the hypodermic injection of large doses of sulphuric ether, as suggested by von Hecker; and he refers to the history of some remarkable instances of the good effects of this remedy in apparently hopeless cases of collapse from post-partum hæmorrhage.

DISCUSSION.

Dr. De Montbrun, Trinidad: Dr. Barnes' views respecting physiological puerperal hæmorrhage are very correct; and although I think they are entertained by all medical practitioners, I would like them widely spread and taught in order to reassure young doctors and attendants. There are other causes of hæmorrhage after parturition—namely, laceration of the cervix uteri, and of the wall of the vagina, and rupture of perinæum; but in all these accidents we can, by putting one hand on the hypogastric region, ascertain that no danger from inertia of the womb is impending. It is to be hoped, with Dr. More Madden, that in the more perfect midwifery of the future, death from post-partum hæmorrhage will be entirely unknown. Unfortunately a long time will elapse before we may arrive at such happy results. We ought to bear in mind that women, especially those of the poorer classes, being generally very careless, consult an accoucheur only when in bed or in danger of life, and that in many instances the fearful accident arrives, in cases in which we cannot possibly foresee its occurrence. In countries situated within the tropics, the symptoms are most formidable; and the danger to life is much more imminent, from the constant heat, profuse perspiration, and malarious effluvia, which, disintegrating the blood, makes it poor in red corpuscles, and less plastic. Insufficient nourishment, by abuse of strong fermented drinks, contributes to weaken women, who generally marry at an early age, and are predisposed to congestions of their womb. From these and other causes it is of common occurrence to meet after a tedious or rapid labour, or after protracted operations, with abundant, unavoidable hæmorrhage, which, of a sudden, carries the patient to the brink of death. Jupissim or liquid extract of ergot is of very little value, principally because it is not absorbed, from weakness of the stomach, which frequently throws it up. Introduction of a hand into the uterus to remove clots, and try to excite its contraction, increases the pain and anxiety, and sometimes makes more profuse the loss of blood, which, from the presence of the clots, was before that flowing less rapidly. Ice proves doubtful, if not hurtful, in the gravest cases. Astringents, lemon-juice, and injections of perchloride of iron have failed to produce contraction of the vessels, and even the compression of the aorta, with fat belly and the enormous volume of the uterus, distended by clots, does not answer to our expectation. So in presence of one of those cases of extreme gravity, I quickly carry the lady to the edge of her bed as for version, with her head as low as possible, and putting myself between her legs, I embrace with both my hands the whole fundus of the womb; and very gently but steadily compressing it, I squeeze it, thoroughly clearing it from its clots; exerting, at the same time, due and constant pressure on the aorta with the ulnar side of my right hand; then taking with my left a kerchief soaked in pure vinegar, if I can procure it, otherwise in plain water, I thoroughly plug the vagina, so that the womb, being compressed by the external manipulation and the tampon, no further dilation or loss of more blood is allowed. Immediately after I inject into the hypogastric region ten grains of ergotine every ten minutes, and order brandy and hot beef-tea at intervals.

Professor Winckel, Dresden: I would call attention to two measures which we in Germany have often employed to arrest post-partum hæmorrhage, and which are not referred to in the paper by Dr. Barnes. It is well known that in violent hæmorrhage slight nausea and vomiting occur, and, in consequence, absorption of medicines and nourishment given is made difficult or impossible. We have, however, in the injection of sulphuric ether under the skin of the abdomen an excellent

means for increasing the action of the heart and the tone of the vessels, and that in the shortest time. In two minutes the odour of ether can be recognized in the breath, and this injection may be repeated frequently (every five minutes, a full Pravas' syringeful=0·7). This is more especially suited for persons who suffer before labour from hydræmia, with or without nephritis; and in such I employ the injections with advantage in slight losses of blood. The other means which I can strongly recommend in severe post-partum hæmorrhage, consists in bandaging the lower extremities as far as the pelvis with an elastic bandage. The good effect of this means in diminishing the work of the heart, can be seen even a few minutes after in the great vessels, the heart, and therewith the brain. The bandage may be left on for half an hour to an hour, or longer. These means will not stop the bleeding at once, but they preserve the strength of the patient, diminish and remove the danger of bleeding, and help in the local treatment of the causes of the bleeding, on which I have no reason to enter further here.

Professor R. Beverly Cole, San Francisco: The subject under discussion is one of the most important that has thus far been considered by the Section. First, because of the gravity of condition of the patient; second, the great necessity of being ready to meet the exigencies of the case in hand. Often there is no time in which to experiment with the various agents suggested by different authorities, such as lemon-juice, alum, ice, hot water (as suggested by Dr. Albert Smith, of Philadelphia), or even the styptic chloride of iron (as suggested by Dr. Barnes). Often the patient has lost so much blood as to possibly have not more than an ounce or two left, and this must not be allowed to escape. Shall we wait, under these circumstances, for the action of these remedies? I maintain, no! We are not authorized to waste the precious few moments left to us and to her, but are called upon to use that which Nature has supplied every man with—namely, the hand. Empty the uterus of any and all coagula; seize the uterus through the abdominal walls, and so hold it as to cause its opposing sides to come together, and thus compress the bleeding vessels: compress, also, the aorta upon the lumbar vertebræ. Cut off, as this will, the supply, and no further hæmorrhage can possibly occur—certainly so long as the pressure is maintained. I wish distinctly to be understood, that in this manipulation I do not attach importance to kneading, so commonly practised by European practitioners; but a firm, steady pressure of one hand over the aorta, whilst the other holds the lateral halves of the uterus in positive and close contact with each other. This pressure should be kept up for at least an hour, in some cases for a much longer time; giving stimulants—brandy and water and ergotin by the mouth, hypodermically, or by the rectum. In conclusion, the aim should be to empty the uterus of coagula, which act as a tent, preventing the organ from contracting, which it is so desirable to bring about; then inducing contraction, which the hand, applied as suggested, will always accomplish. Embolism and thrombosis are liable to result from applications by injection of any preparation of iron; and for these reasons I am positively and uncompromisingly opposed to them.

Dr. John A. Byrne, Dublin: To any one who has seen a good deal of post-partum hæmorrhage, and who, like others in this room, has come to regard it as the dread enemy whom we must be always prepared to meet, I would say that the mode of averting it, recommended by Dr. Beverly Cole, will not, on every occasion, be successful—viz., manual pressure. Dr. Cole has ridiculed,

more suo, the carrying into the sick chamber the *armamenta* necessary to meet emergencies of all kinds. But surely it is not necessary to remind Dr. Cole that this is not done, nor should it be done; for all remedies, appliances, &c., should be left in another room. But I must confess that I never go to any distance from my house without being accompanied by everything necessary to meet difficulties that may arise; and one of the remedies, without which I never go, is the solution of perchloride of iron. I must say that I have heard in this discussion the old ground of treatment in post-partum hæmorrhage gone over again and again. One gentleman relied on pressure, others on ergot, others on galvanism, others on cold—ice, the introduction of the hand, &c. Now, whilst all are useful, and to be employed according to the case (and always manual pressure during the last two stages of labour), yet we may always expect to meet with a case in which all these remedies will fail, and our patient's life will be jeopardized by hesitation or delay. Now, in these cases, we have an unfailing aid (in most cases) in the plan recommended by Dr. Barnes. The dangers of this treatment have been described here to-day—the sudden death, the introduction of air, the thrombosis, the peritonitis, &c. Well, we know that there is some danger. But are we to let a woman die, before our eyes, because there may be a possibility of one of these dangers occurring? Are we to see her life-stream gush away, and not try to arrest it? Remember, we are steering between Scylla and Charybdis, and that one minute may decide the event. We must do it carefully and with every precaution, and in the manner laid down by Dr. Barnes; and the result will be, as you will find, that in the majority of cases you will save life, and the consequences will not be formidable nor fatal.

Dr. Matthews Duncan, London: In hæmostasis I trust exclusively to uterine retraction. Without that the hæmorrhage may be or may not be stopped. With it when complete the hæmorrhage is certainly stopped. Styptics are, therefore, in my opinion, of no use, unless they act as irritants producing uterine contraction and retraction. Kneading and compression of the uterus are, I think, ninety-nine per cent. of our available good means for producing hæmostasis. I say nothing of the other pressing indication—namely, to keep the woman alive. Many other methods contribute a little to favour this uterine retraction, but they are insignificant when compared with kneading and compression. The intra-uterine injection of perchloride, &c., of iron I regard as more dangerous than the condition it was intended to obviate, my own experience and reading seeming to me to demonstrate this.

Dr. Playfair, London: I have particularly to draw attention to a point in connection with this subject, which I believe has not sufficiently been dwelt on, and that is, the extreme importance of prevention. I believe that if this lesson were driven into the mind of the practitioner, that every case is to be treated as if hæmorrhage was about to occur, then it would be of the rarest possible occurrence. If I hear of a man who is always having hæmorrhage amongst his patients, I feel myself justified in concluding that that man either does not know, or does not practise, the proper management of the final stage of labour. I believe that no more important rule has ever been laid down than that formulated long ago by the President, and that is that no attempt should ever be made to remove the placenta until twenty minutes have elapsed since delivery, which allows time for coagula to form in the mouths of the vessels, and if this is properly attended to hæmorrhage will very rarely occur. Finally, as

to the use of iron injections, I feel that in the very severe cases in which they have been used they are of great value. Like all powerful remedies, they are not devoid of risk. I agree that in ninety-nine cases out of a hundred they are not wanted, but in the hundredth nothing else will save life. And then they act, not by producing contraction, which can no longer be evoked, but by congealing the mucous lining of the uterus, and producing thrombi in the open vessels. One of the chief risks I believe to be in forming hard clots in the uterus, which are apt to become septic, and thus produce mischief.

Dr. Budin, Paris: The treatment of this important question may be divided into (1) the preventive treatment, (2) the treatment of the hæmorrhage itself, (3) the subsequent treatment. The preventive treatment it is evident requires the exercise of knowledge and skill in the delivery. As regards the treatment of the hæmorrhage itself, one of the speakers has justly said that it is necessary that the accoucheur in attendance should be ready to employ every means. We could speak of the dangers associated with these means: every proceeding may be dangerous, but the greatest danger arises from the hæmorrhage itself. It is above all necessary to ward off death. We must arrest the hæmorrhage. The subsequent treatment is also important. When women have lost a considerable amount of blood during delivery, when they are pale and sickly, then a state of almost syncope continues. The subcutaneous injections of ether are of great value; they cause sharp pain at the seat of injection, and excite the woman and revive her. The other means referred to by Prof. Winckel—the employment of an Esmarch bandage—may perhaps prove useful, but to the different means the use of cold should be added. For the arrest of hæmorrhage cold is certainly useful, but when the hæmorrhage has been stayed, when the uterus is and remains contracted, when it is no longer necessary to revive the patient, we should have recourse to heat to avoid further loss of power. Dr. Laborde has made the following experiments:—He bled guinea-pigs to whiteness. It was in winter. Some were left near the fire-place before the fire, and these survived; the rest were exposed to the cold, and died. When I attempt to resuscitate a still-born child, I have for a long time wrapped it in warm linen when I insufflate it. In the same manner we should place hot bottles to the feet and sides of the woman. We should wrap her with hot coverings, at the same time give her stimulants, &c. In those cases of severe hæmorrhage, to which I have been called in, I have employed heat, together with other means, with the best results.

Dr. Edis, London: I think the keynote to the treatment of post-partum hæmorrhage is the preventive treatment. During pregnancy the patient's general health should be attended to. She should be sustained, during the process of labour, by suitable nourishment. The membranes should be ruptured as soon as the os uteri is sufficiently dilated. Forceps should be timely applied before the uterine powers are exhausted by fruitless labour. Chloroform should be withheld or given with great care, if a tendency to hæmorrhage exists. The uterus should be firmly gripped, following up the expulsion of the child, and held steadily. A dose of ergot should be given just as the delivery is imminent. The expulsion of the placenta should not be hurried for at least a quarter of an hour. If these measures are properly attended to, post-partum hæmorrhage seldom occurs; but where, from the labour being very precipitated, the practitioner is not at hand, and hæmorrhage

ctually occurs, then compression of the uterus, the application of cold, the jection of cold water into the uterus, or hot water if the vital powers are very epressed, the hypodermic injection of ether or ergotin, and other available remedies, hould be resorted to. If, however, the hæmorrhage persists, and the condition o graphically described by Dr. Barnes supervenes, then the injection of iron he ecommends is the only means at our disposal to snatch the patient from mminent death.

Dr. JONES MORRIS, Portmadoc: As a country practitioner, who has attended some undreds of labour cases, I have seen but one case of post-partum hæmorrhage. This appears to be due to the fact that when the head is on the perineum I give a full dose of ergot; after delivery, place the hand on the uterus externally and eep it there till the placenta is expelled, and do not remove it till I am satisfied hat the uterus remains in a good state of contraction. In the one case referred o, when thinking what best treatment to use, the patient fainted, and the æmorrhage stopped. I think we should try and follow Nature as much as ossible, and remember that there is a tendency, by the process of fainting, to top the hæmorrhage. I should be inclined, in case of necessity, to trust cold ater, or hot water, rather than iron.

Dr. J. M. QUIMBY, Jersey City, N.J.: I agree with Professor Playfair that ost of the fatality that occurs in post-partum hæmorrhage is due to the ck of proper attention in the third and last stages of labour. The method of ractice that I have followed for the last twenty years has been, that when I ave a patient in whom I think any serious hæmorrhage is likely to occur, I ommence constitutional treatment about two or three months previously to par- urition. If the patient is pale and chlorotic, with swelling of the limbs, &c., I give er five drops each of tinct. ferri mur., and the tinct. nuc. vom., ter die, after eals; which has usually a very happy effect. Then, one or two hours previous o the termination of labour, I generally give, according to circumstances, from two rachms to half an ounce of Squibb's fluid, ext. of ergot, together with ten drops f tinct. nux vom. By grasping the uterus firmly, and manipulating, or kneading, t pretty vigorously, also introducing one hand in the cervix, or the whole hand nto the body of the uterus, producing some rotation and pressure with the hand, n ninety-nine cases out of one hundred, sufficient contraction will be induced to control the hæmorrhage. Where the labour has been tedious, and the patient exhausted, I think there should not be too much haste to remove the afterbirth. An hour should elapse, or at least a half hour, after the expulsion of the child, so as to give the exhausted uterus a little time to regain contractile power. Out of 2,000 cases, I have had thirteen cases of post-partum hæmorrhage, all of which recovered under this method of treatment, except one, which was complicated with uterine tumour. In reference to the method which Dr. Barnes so strongly recommends of injecting perchloride of iron within the uterine cavity, it is surrounded by a great many difficulties, if not actual dangers, to the ordinary practitioner. Some of the difficulties are that all clots or portions of the placenta must be removed, or, in other words, the uterus must be entirely emptied, otherwise the injection will not be efficacious. Therefore I agree with Dr. Beverly Cole and Professor Pallen, of New York, that external pressure, electricity, &c., may be quite as effectual as the method suggested by Dr. Barnes, and devoid of many of its dangers and difficulties.

Dr. C. G. Rothe, Altenburg: Among the methods of exciting contraction of the uterus, I should like to draw your attention to the external use of cold water, in form of the rain-douche to the abdominal walls, and to the lower part of the spine. In a desperate case, where hot and cold injections were of no avail to arrest the most severe hæmorrhage in an almost moribund woman, I resorted to this method, I had the woman held over a bathing tub, and poured on her quantities of water through the sieve of a water-pot over the abdomen and spine, and within half a minute the hæmorrhage ceased and never returned, the woman making a good recovery. Another method acting on the same principle of exciting uterine contraction by reflex-action is a spray of ether applied to the spine and abdomen, but you have not it always at hand. As regards the danger of the injections of the perchloride of iron, I fear that those who assert that they never have seen any danger arising from it, will yet come to see it. I lost a patient, in whom I had arrested the hæmorrhage twice by the injection of iron without any accident, in making it a third time on the fourth day when some oozing seemed to indicate a new hæmorrhage. I injected only a few drops of a ten per cent. solution, when the woman at once complained of a severe pain in the left inguinal region. Five days afterwards she died of peritonitis. A post-mortem was not allowed. Had I forced the uterus open and looked for remnants of the placenta or membranes I should probably have saved the patient.

Dr. Paul Mundé, New York: I have used iron (dilute solution) once in post-partum hæmorrhage, and iodine once, and once compression of placental site between the fist in utero and the other hand outside for more than two hours, before I dared withdraw my hands; all these cases recovered. But my experience both in a large hospital and fairly large private (chiefly operative) practice, has led me to the conclusion that by proper preventive means, as a rule, hæmorrhage can be prevented even in cases predisposed to it. There are several main factors in this prevention which should always be observed: 1. Never to remove the compressing hand from the fundus uteri from the moment the head has passed the vulva, until the placenta is expelled and the uterus has become firmly and apparently permanently contracted. While the child is being cared for and the cord ligated, the nurse may be entrusted with this compression. 2. Always give ergot by the mouth as soon as possible after the birth of the head; and if the condition of the mother (anesthesia prostration) is such as to render its absorption in this manner too slow, or a more rapid effect is desired, the ergot should be injected hypodermically, but deep near the umbilicus. 3. Never hasten the expulsion of the placenta, but continue gentle friction of the fundus until its contraction and smooth rounded outline shows that the placenta has been expelled, at least into the vagina; a delay of half an hour can do no harm, but if the placenta should, nevertheless, be retained still longer, it is probably adherent, and requires manual detachment. 4. Continue friction of fundus for fifteen to thirty minutes after expulsion of placenta, and watch the patient for at least an hour afterwards. 5. Do not hesitate to give stimulants after the birth of the child if the woman be prostrated, and the same after expulsion of the placenta.

Dr. McClintock, Dublin: In my experience, promptitude and energy are of the utmost value and importance in adopting any plan of treatment. My estimate of iron is very high; I have used it in a limited number of cases, and I think its employment is attended with considerable risk; but desperate cases require desperate

remedies, and I always carry some in my obstetric bag. No speaker has alluded to the influence of chloroform in predisposing to post-partum hæmorrhage; but my deliberate conviction is that it does produce this effect; and, therefore, in all cases where there is reason to apprehend hæmorrhage, I scrupulously withhold the administration of chloroform. In using ergot hypodermically, I plunge the needle into the buttock, and have never seen any unpleasant local consequence to ensue. Before using iron injection, I think it is very important for the success of this operation that the uterus should be free of clots. In my practice, I invariably give the patient some stimulant—a glass of port wine, or of brandy-and-water, immediately on the expulsion of the fœtus; and this, I think, helps to restore nervous power and bring about good uterine action. I never omit applying cold to the sacrum and lower part of spine, and think this mode is equally efficacious to the application of cold to the hypogastric region. In the way of preventive treatment, prior to the accession of labour, I have found good results from the administration of gallic acid, or of muriated tincture of iron, for some days or weeks, and continued up to the setting in of labour.

Dr. Barnes, London: The discussion has not been very strictly limited to the question of treatment. It has diverged into questions of causes and prevention, so that the main object I had in view—namely, that of pointing out that there are distinct physiological indications for the employment of the several remedies in common use, has not been discussed. It seems to have been assumed by some speakers that the special object of my memoir was to advocate the use of perchloride of iron; whereas, I only sought to show the circumstances under which it is to be preferred to other remedies, all useful in their proper places. Iron comes in when other means fail. It comes in when we can no longer rely upon the means which induce contraction of the uterus. Dr. Duncan said contraction only can be relied upon to control hæmorrhage. Every one agrees that it is the best factor, every effort is made to secure it. But I affirm, most emphatically, that iron injections will arrest hæmorrhage and save life when contraction cannot be produced. Therefore, there must be a last factor remaining, after the failure of contraction, which can be relied upon to arrest hæmorrhage. Hence we may see in styptics an agency that may be called in after the exhaustion of the nervous centres. Professor Winckel dwelt upon two points as having been neglected—namely, the subcutaneous injection of ether and Esmarch's bandage. Ether in this way is in familiar use in this country; but it is not a means of arresting hæmorrhage, it is restorative from its effects. Use it, but you must use other means to arrest hæmorrhage. I have not used Esmarch's bandage, but I will certainly do so in future. Nothing has pleased me more than the decisive clinical testimony borne in favour of my practice by the Dublin School, which I regard as the greatest in Great Britain.

Dr. More Madden, Dublin: I will not attempt, after the exhaustive reply of Dr. Barnes, to follow the different speakers, and to enter into the consideration of all the subjects which have been imported into the discussion. In reply, however, to Prof. Cole and Dr. Matthews Duncan, the latter of whom regarded the use of perchloride of iron as being more dangerous than the flooding for which it is employed, I would say that Dr. Matthews Duncan's experience is very exceptional, and probably is peculiar to himself. For my part I, without asserting the general safety of the perchloride of iron in common use, consider that the midwifery practitioner

who, when attending a case of severe post-partum hæmorrhage, requiring the use of the powerful styptic and stimulus to uterus contraction referred to should, from any prejudice or undue timidity, refrain from employing it, and thus allow his patient to die from unarrested flooding, would be as morally responsible for her death as a man that would stand quietly by on the bank of a stream whilst a fellow-being was drowning there, when by stretching out his hand he might have rescued him.

On Trismus and Tetanus of the Uterus in Labour.

Dr. George Roper, London.

I have selected the above subject for discussion in the Obstetric Section of the Congress for the following reasons:—Firstly, because it has scarcely been recognized by British obstetric writers. Secondly, on account of its rarity, and our consequent inexperience of its nature, and want of knowledge as to its correct and successful mode of treatment. It is, moreover, one of the most terrible of all forms of difficult labour, often resulting in death to both mother and child.

I am indebted to Dr. Herman for having brought to my notice that Spiegelberg* has given a good description of it, and he has kindly translated for me that author's remarks on it.

Spiegelberg's observations and reflections are so exactly in accordance with my own that I cannot do better than transcribe them. He says:—"Tonic partial cramp, or spastic stricture affects only those muscular fibres which are arranged in the manner of a sphincter. It is observed only at the external and internal os uteri. Stricture of the external os occurs only in the period of dilatation, indeed in its early stage, and especially in primiparæ; the few cases I have seen concerned only such. The border of the os uteri is persistently tense and hard, sometimes perfectly rigid, and the tension remits at shorter or longer intervals, but never entirely passes off. The tissue of the rest of the uterus is yielding. There is no trace of dilatation of the os during a pain, indeed there is more apparent narrowing, and the presenting part recedes in an upward direction. At the same time there is severe pain on touching the os uteri, and spontaneously in the sacrum and thighs. In this the neighbouring organs participate, especially the bladder and bowels, with colic, tenesmus and nausea. If the uterine muscular fibre is at the same time acting well, then the cervical-segment, especially its anterior wall, may be deeply pressed down by the fœtus, and consequently the os uteri displaced upwards and backwards, and deep laceration of the border of the os may ensue. This stricture develops itself when the membranes remain entire, as well as after premature rupture of them. Its causes are either rough manipulation, whether for diagnostic purposes or to widen the too slowly dilating orifice, or from metritic conditions; and on this account the prognosis for the lying-in period is not without concern. The treatment consists in attention to the general conditions, application of warmth (fomentations, baths, irrigation of the vagina), derivatives, as mustard and turpentine, to the hypogastric region, and the free use of narcotics. If the stricture remains hard, and threatens danger to the mother or child, then the border of the os should be sufficiently incised. It is better to make one or

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* "Lehrbuch der Geburtshülfe," 1878, p. 421.

two deep incisions than many superficial ones. . . . . Strictures of the inner os are more frequent than of the outer os. In the dilatation period I have seen it only in premature labour after too early rupture of the membranes; in labour at full term with breech presentation, in which, owing to the presenting part being so swollen, it was mistaken by the practitioner for a new growth; twice in cross presentations, with premature rupture of the membranes. Mostly the stricture is observed in the expulsive stage. . . . . This condition, like cramp of the external os, is very painful and accompanied with the same general irritation, the pain extending to the neighbouring organs. . . . . Its causes are, early escape of the liquor amnii in pelvic and cross presentations, untimely pulling on the presenting part, over-stimulation of the muscle generally, the improper use of ergot, chilling of the lower parts of the body, and metritis."

This is what Spiegelberg says about trismus of the outer and inner os; here follows his description of tetanus of the uterus itself (p. 419) :—

"From this we must exclude every sensitive, painful, and slightly effective contraction, which is often in practice taken for cramp pains, but which does not belong to this class. The latter (cramp pains) are met with under two forms; as permanent general contraction, or as spastic stricture. The continuous contraction, the tonic general cramp of the uterus (in its highest degree known as tetanus), is an increase of the already mentioned too vigorous pains, and is characterized by a want of complete relaxation of the uterine muscular fibre, if indeed, now more now less regularly, a remission of the highest degree of contraction now and again shows itself."

The following cases, which are taken from Smellie's reports, may be regarded as examples of trismus of the os uteri :—

Case 387.—Breast, neck, funis, and chin presentation; hæmorrhage. Os uteri rigid and not capable of being stretched by manipulation. Several attempts of this kind were made, with the patient placed in various positions. The crotchet was then used unsuccessfully. Expectancy then relied on for a time without any progress. It had been determined to dilate with scissors, but in the meantime the patient died undelivered.

Case 389.—Premature labour in a primipara—the face presented. Flooding commenced at early period of labour—os uteri could not be dilated by manual stretching. The os uteri snipped with scissors—child delivered by podalic version during which the incision was extended by laceration—death of mother on the 4th day after delivery.

Case 396.—In this there was flooding with arm presentation—the os uteri very rigid—not to be dilated by manual force—os uteri snipped and torn while delivering the child after podalic version—death of mother on the 7th day after delivery.

The four following cases occurred under my own observations :—

Case 1.—*Placenta prævia in a primipara at the end of the eighth month of pregnancy—transverse presentation—trismus of the os and cervix uteri—difficulty in effecting version and extracting the child—death of mother two hours after delivery.*—On the 25th of April, 1874, I was asked by Mr. Gordon Brown of Finsbury Circus, to see with him a patient, a Jewess, æt. 22, in labour at the end of the eighth month of pregnancy, with her first child. Flooding had commenced five days before I saw her. At the end of the fourth day Mr. Brown had ascertained

that the case was one of central placenta prævia. At this period, he ruptured the membranes and separated the placenta; and afterwards there was no more flooding. The os uteri was the size of a florin and so rigid and tense that dilators had no effect in causing expansion of it. The entire placenta was subsequently removed; when the child's hand came into the os uteri. Traction on the arm, with a view of dilating the os, caused the limb to separate; the tissues were soft, as the child had been dead several days. She was left for the night, and on Mr. Brown's visit next morning he found the os uteri in the same rigid and unyielding condition. There had been no rhythmic pain, the patient had lain still all night without complaining, yet she was in a restless anxious state, and had a quick pulse. I saw her with Mr. Brown, at noon of the same day, and although there were no labour pains, I thought from her anxious and exhausted expression of countenance and quick pulse that delivery was urgently needed. Mr. Brown put her well under chloroform, and I proceeded with a view to deliver the child by version. The bipolar method was impracticable. The cervix uteri was undeveloped, for I could detect the inner and outer os, with a small intervening portion of cervical canal. Two fingers could be passed through, and by forcible upward pressure the ribs of the fœtus could be felt; but so great was the tension of the os, especially the inner one, that I could gain no further entrance into the uterus. The os rather contracted than expanded when attempts were made to dilate it. Chloroform in no degree diminished the spasmodic rigidity; and while the patient was fully under its influence, five or six of Barnes' dilators were burst in attempts at expansion without producing the desired effect. We now agreed to leave her for six or eight hours, hoping she might obtain some sleep after the chloroform. Nourishment was ordered to be given frequently. We met again at the appointed time, when we found her in a very serious condition. Pulse 130, pinched features and an aspect of great exhaustion. The os uteri of the same size and in the same rigid state. The uterus had become largely distended and was tympanitic from decomposition of the child. The introduction of a perforator into the child's abdomen let off some fœtid gas, which for a time relieved the uterus from distension. We were anxious that she should not die undelivered, and therefore requested Dr. Braxton Hicks to meet us in consultation. By the time he arrived there were symptoms of impending death. Dr. Hicks was unable to introduce his hand through the resisting os, but, with a long crotchet, a hold was obtained on the ribs, and by forcible traction the child was somewhat moved, and a large quantity of stinking gas let out of its chest: afterwards, with a small, long, blunt hook passed up through the os, Dr. Hicks succeeded in catching a knee, which was brought down. Much traction was required to deliver the child. The flaccid, decomposed state of the child facilitated its passage, and the moribund condition of the mother somewhat favoured this. She died two hours after.

Case 2.—*Premature labour—placenta felt within the cervical zone of the uterine cavity—hæmorrhage—transverse presentation—trismus and tetanus of uterus—delivery by decapitation—recovery of mother after long and severe illness.*—On the 18th of January, 1876, a midwife of the Royal Maternity Charity, requested me to visit E. S., æt. 29, No. 4, White's Row, Spitalfields, in labour with her eleventh child. She was within a week of the seventh month of pregnancy. The midwife had been called to her twelve hours before the time of my visit, and had found her suffering with severe and frequent labour pains. The membranes were nearly protruding from the vagina; they were ruptured, and a large flow of liquor amnii took place, and the pains at once ceased. When I first saw her she was free from rhythmic pains. The uterus, as felt through the abdominal

walls, was small and hard. Spasmodic pains were excited on passing the fingers within the os. She complained of a constant, unremitting pain over the sacrum. The right hand of the child and a loop of funis projected through the os uteri; the funis was very tense from blood forcibly squeezed into it by constant uterine contraction. The os uteri was of the size of a crown-piece; I could only introduce two fingers through it, when the chin and mouth of the child could be felt. The head was extremely extended, the occiput being doubled back between the shoulders; the edge of the placenta was within reach. There was a slight amount of flooding. Manipulation excited a greater degree of contraction of the os; attempts to expand it caused it to contract more. There was no possibility of turning the child by the ordinary method. The bipolar mode failed. We, therefore, left her for a time, trusting that the uterus might act naturally and force the presenting part down on the os, and thus expand it. In three hours no alteration in any respect had taken place. A Barnes' dilator of large size was now introduced, and left in the os, but the full effect of this was lost, as the patient sat up, and broke the metallic tube of the bag. Eight hours had now elapsed since I first saw her, and as she had a pulse of 120, and an anxious and pinched expression of face, we considered it expedient that she should be delivered. No further descent of the presenting part, nor expansion nor relaxation of the spasmodic os had occurred. As the child was very small and flaccid, traction on the arm was made as a means of dilating the os, but to no purpose; the arm gave way. A small blunt hook was then passed round the neck, and traction made on it, with a view to facilitate the application of a small sharp hook. (This small sharp hook here shown I had made, as, in a former case, I had found it impossible to introduce the sharp hook of ordinary size.) After very much difficulty (it may here be observed that in all these cases the fingers of the operator get painfully cramped, and after a time become almost useless) decapitation was effected. By steady traction, the headless body was delivered, and the head was perforated and brought away by the cephalotribe. There was difficulty in getting the placenta, as the funis had been divided by the sharp hook close to its insertion into the placenta. No laceration could be discovered, but the patient was much collapsed after delivery. Tr. opii ʒss was given every four hours till she was well under its influence. The next morning the pulse was 120, temp. 102°, abdomen painful on pressure and tympanitic. She continued severely ill for three weeks from metritis and general pelvic inflammation. There were extensive masses of inflammatory induration in the right iliac fossa, and the uterus was large, hard, painful, and exceedingly fixed. At the end of six weeks she was convalescent.

Case 3.—*Placenta prævia—premature labour—oblique presentation—trismus of the uterus—difficulty in extracting the child—mother died undelivered.*—On the 20th of February, 1878, at 10 A.M., a midwife of the Royal Maternity Charity requested me to visit E. S., æt. twenty, residing at 8, Richard's Place, Haggerston, in labour with her second child, at the end of the seventh month of pregnancy. She had had more or less of a draining kind of discharge of blood since the fourth month of pregnancy. Three hours before I saw her she had been seized with a severe flooding; and although she lost a large quantity of blood, she was not blanched. The lips were of good colour, and she had a good pulse. On examining the abdomen, the oblique position of the child could be made out; its head was in the right iliac fossa, and the breech above the left crest of the ilium. The os uteri was so high in the pelvis that it could only just be reached by the finger; it was open only to the size of a shilling, and was exceedingly rigid. As there was now no flooding, I did not introduce my hand into the vagina to make a

more minute examination. The cervix and lower segment of the uterus were in a measure undeveloped. The outer and inner os, with an intervening portion of cervical canal, could be distinguished. I ordered her some sulphuric acid and opium, with plenty of beef-tea, milk, and eggs; and directed that she should be kept perfectly at rest, and in the event of further hæmorrhage occurring, I was to be summoned. She passed the day and night comfortably, but at seven o'clock next morning another attack of hæmorrhage happened, I was with her soon afterwards, and found her very pale; her extremities were cold, and the pulse very quick and feeble. She had had no labour pains. The os uteri was about the size of a florin, thus showing that dilatation had been slowly proceeding; but the os was so rigid and spasmodically tense, that no reasonable amount of force could stretch it. The placenta was centrally placed; but by passing one finger round its edge, and making strong pressure on the fundus uteri, I succeeded in hooking a knee and bringing it down through the os. The body of the uterus was soft. By making traction on the knee, the child was placed in a straight position. I then waited, in the hope that labour pains would be set up. During the process of getting down the leg, and after this period, no further flooding occurred. Half a grain of ergotine was injected subcutaneously, and brandy, milk, and eggs were given freely. At 11 A.M. I left her in charge of the midwife, with instructions that she was to use gentle traction, in co-operation with labour pains, should these come on. I returned to her in two hours, when I found her condition as perilous as when I left her. She had had no labour pains, and there was no further descent of the fœtus, but the os seemed to be not quite so tense. By steady traction I gradually drew the breech through the os, and then the trunk; but great difficulty was experienced in getting down the arms. During this proceeding the whole circumference of the os uteri came into view at the orifice of the vagina, and any further force than I had already used would have pulled the uterus out of the pelvis or split up the cervix. Finding it was impracticable with safety to deliver the head without perforating it, I sent for my obstetric bag, but before it arrived the patient had expired. Pressure on the fundus had been used throughout in co-operation with traction. In ten minutes after death I proceeded to extract the head from the uterus; and although the relaxation of death had done much to reduce the tonicity of the cervical structures, yet extreme force was required to do this.

CASE 4.—*Placenta prævia (full term?)—transverse presentation—trismus and tetanus of the uterus—difficulty of version (podalic)—evisceration of fœtus—death of mother two hours after delivery.*—This case occurred in the practice of Dr. Eve, of Lewisham, and he has kindly furnished me with the following report. This extends from the time he first saw the patient to the time of my visit—a period of about sixty hours:—

"I first visited Mrs. P. D. W. (aged twenty-seven years, primipara), on Sunday evening, August 29, 1880.

"She stated that there had been a slight discharge of blood from the vagina all day, accompanied with occasionally a uterine pain. She suffered from frequent nausea and vomiting, but had no feeling of faintness, and was in good spirits.

"Upon making a vaginal examination I found the os uteri situated just behind the superior ramus of the left pubis, dilated to nearly the size of a florin, hard and rigid. The fingers could not be passed through it, nor the presentation detected. Several ribs could be felt through the wall of the uterus, which lay across the top of the vagina. Upon an external examination the child was found to be placed transversely, with the head above the right acetabulum.

"She was ordered to bed, and directed to take cold beef-tea, made thin, soda-water, and occasionally milk in soda-water; napkins wrung out in vinegar and water were applied to the vulva, and the following mixture prescribed:—

Acid. nitric. dil.	℥20
Acid. mur. dil.	℥20
Tr. belladon.	ʒj
Nepenthe	ʒss.
Tr. limonis	ʒss.
Aquæ	ad ʒviij

ʒss secunda quaque hora.

"*Monday morning, Aug.* 30.—She had a good night; rather more discharge of blood from the vagina. No uterine pains. The vagina was plugged, and she was ordered to suck ice and to take all her nourishment cold. The os uteri remained in the same condition, and the presentation could not be felt. The same mixture was continued.

"*Tuesday morning, Aug.* 31.—She did not have quite so good a night, the discharge much less, and the sickness had quite subsided; plug removed in the afternoon, and the mixture discontinued, ordered to take occasionally champagne, and the nourishment cold as before. The os uteri was not examined, as there were no labour pains, and the examination might have caused a return of the discharge.

"*Wednesday morning, Sept.* 1.—She passed a tolerably good night. I was sent for at seven o'clock in the morning, the patient having felt occasionally slight uterine pains, and had a return of the discharge. Upon making a vaginal examination the os uteri was found dilated to about the size of a five-shilling piece, the edge of the placenta was found to be overlapping the posterior margin of the os uteri, and toes of a foot could just be felt. The os uteri remained rigid and undilatable, and upon an external examination the uterus was felt in a tonic state of contraction. All that was done was to pass a finger through the os uteri and separate the placenta as far as it could be reached. At 9 A.M. the discharge decreased, but the os uteri remained in the same rigid condition, without the least prospect of being able to introduce the hand. The toes of the foot could not now be felt, but the fingers of a hand were just within reach.

"10 A.M.—No improvement in the patient's condition; and, as there appeared no prospect of the uterus relaxing from its tonic state, Dr. Roper was called in to see the patient."

When I first saw her, she was in a state of great excitability; the pulse was quick, and there was a deep coloured patch of redness on each cheek. Ordinary observation did not detect any of the usual signs of active labour; indeed it was difficult to discern that she was in labour at all. There was a constant unremitting pain about the sacrum and lower part of the abdomen. On external examination the uterus felt hard and firmly contracted; the os uteri was dilated to the size of a crown-piece. The internal and external os could be distinguished from each other, and there was a short portion of cervical canal between the two. (This undeveloped condition of the cervix led me to believe that the patient had not gone to full term. Or might not this undeveloped state of the cervix depend on the implantation of the placenta in the cervical zone?) The inner os was thick and rounded and in a state of great tension, and under examination it seemed to contract, and was tender to the touch. Two fingers only could be introduced into the uterus. I could just touch the toes of the child's left foot. These pointed

backwards, and I could feel the heel of the same foot externally above the symphysis pubis. The patient complained of severe pain when external pressure was made on the heel with a view of pushing it downwards through the os. The edge of the placenta could be felt posteriorly, close to the margin of the os. At no period of the labour was there any flooding of importance. Before proceeding further it was proposed to give her chloroform, but to this she strongly objected, and her scruples could not be overcome. I tried various means to bring down the foot, by grasping the toes with polypus forceps without avail. My fingers were so cramped by the spasmodic os, that I was quite tired out. At last I succeeded in snaring the big toe with an écraseur, and although the foot was so immediately over the os, I had the greatest difficulty in getting it down, so firmly was it grasped by the uterus. The foot having been drawn down into the vagina, no reasonable amount of traction could get more than the thigh through the os. The uterus, as felt through the abdominal wall, was persistently tense. After waiting two hours in expectation of rhythmic pain being set up, in which we were disappointed, we agreed to leave her for a longer period to see what Nature might do. Nourishment and a moderate quantity of stimulant were ordered. After an interval of six hours I visited her again. Dr. Eve had been with her about an hour before I arrived, and had succeeded in extracting the breech through the os, but the resistance to this proceeding was very great. After removal of the abdominal and the thoracic viscera the chest was gradually drawn through; the arms could only be brought down by means of the crotchet hooked on the clavicles. The spasmodic os now grasped the child's neck. I tried to perforate the skull to its base, to use the cephalotribe without perforating, and then attempted to use the forceps, but all were impracticable. Throughout all this time the uterus had remained in a state of unremitting tension, without any rhythmic or expulsive action, and there was considerable obliquity of it from the fundus being deflected towards the splenic region. As the aspect of the patient was most serious, and all means of delivery had been tried except incision of the os, it seemed expedient to extract the head by force, and this was done. The placenta was brought away without trouble. It was now evident that the patient was in a state of fatal collapse. The uterus was firmly contracted, but there was a considerable flow of bright coloured blood, which doubtless came from a transverse laceration of the posterior segment of the cervix.

She died two hours after delivery. The very small amount of hæmorrhage throughout the case could not be regarded as a cause of the fatal result.

The remarks of Spiegelberg, quoted at the beginning of this paper, respecting the nature, causes, and treatment of trismus of the os uteri, together with the examples given in Smellie's and my own cases, are such a sufficient commentary on the subject, that any further observations are scarcely necessary to elucidate this very important kind of difficult labour.

I think, however, that some additional remarks are desirable. Firstly, as to its nature: we must be mindful, as Spiegelberg has remarked, to distinguish trismus of the os uteri from all other forms of obstructed labour. The affection must be recognized as an exceedingly rigid and tense state of the os and cervix uteri from spasmodic contraction alone, all cases of rigidity of the os resulting from structural changes of the uterine tissue being excluded.

The minor causes, as rough manipulation for diagnostic purposes, stretching the os uteri mechanically, premature rupture of the membranes, and the indiscreet use of ergot, may be passed over without remark.

In speaking of the causes of trismus, it may be premised that, in treatment, the removal of the cause (real or supposed), ordinarily, does not entirely, or in a great degree, relieve the trismus. As an example, take a transverse presentation to be the cause. By rectifying the malposition the trismus is not overcome, but persists in as full force as ever.

To this I shall refer hereafter.

The frequent association of a mild form of trismus with premature labour is familiar to every accoucheur as a difficulty in the first stage of labour; and however we may endeavour to explain this, the fact exists.

Dr. Barnes ("Lectures on Obstetric Operations," 3rd edition, p. 493) says, in speaking of the conditions commonly present in placenta prævia:—"The first is an immature uterus. Flooding frequently occurs before the full term of gestation is complete. The uterus is therefore taken by surprise, before its tissue is developed, before it has acquired its normal contractile power; besides this, the tissue is ill adapted to expand."

Although in discussing placenta prævia Dr. Barnes frequently refers to the difficulty as connected with the undilated and undilatable os of the immaturely developed uterus, yet he does not thoroughly recognize its spasmodic or trismic condition.

Uterine polarity seems to afford a rational explanation of trismus in labour before full term. At a premature period, fundal polarity has not become established, whilst cervical polarity remains in force; hence there is, in premature labour, as connected with an immaturely developed cervix, a degree of what may be called normal trismus, to which is superadded the morbid rigidity of a spasmodic kind.*

We are greatly indebted to Dr. Champneys for having translated a paper written by Reil on this most interesting subject, with some valuable remarks of his own. This article was published by Reil in 1807, just seventy-four years ago, and seems to have been forgotten, or passed over, as of no importance, until it was rescued from oblivion by Dr. Champneys. Reil, in this paper, clearly recognizes both trismus and tetanus of the uterus. He says: "At times contraction fixes its focus at the time of labour in the neck of the womb, instead of in the fundus, and the former is by this means spasmodically contracted." "In other cases, every point in the whole surface of the uterine walls becomes at once the focus of contraction; every part draws itself together with equal strength and without intermission, as in rigor mortis. Hence the pressing of all points of the circumference towards the central point of the uterine cavity, and the incarceration of its contents, embryo, placenta, &c."

Transverse presentations stand in close relation to trismus as a cause: but, as already observed, the trismus does not cease when the malposition is rectified.

The behaviour of the uterus in ordinary transverse presentation differs much from that observed in transverse presentation combined with trismus. In the former, the uterus often remains passive, although conscious of the difficulty with which it has to deal, even although the os uteri is soft and largely dilated; but as soon as the malposition is rectified, rhythmic, vigorous action ensues, and expulsion of the child is readily effected. Occasionally, however, the uterus in like circum-

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* *Obstetrical Journal of Great Britain and Ireland*, January, 1880.

stance, does not remain passive, but acts with great violence, even to the extent of expelling the child by way of spontaneous evolution.

The conduct of the uterus in transverse presentations with trismus affords great contrast to this. After version, rhythmic, expulsive action rarely occurs, and there is no tendency of the uterus to act and expel its contents.

The body of the uterus, whether it be inactive or in tetanic action, shows no disposition to force the child through the spasmodic os. Traction on the leg to assist delivery seems to render the os even more spasmodic, and the progress of the labour is rather retarded than expedited by such attempts. The os tightly grasps the thigh of the child, and obstinately resists the passage of the half breech through it. We may leave the fœtus for many hours, with one of its lower limbs firmly held in the os, and yet no expulsive effort is set up by the uterus. During a long period there is no progress towards delivery, and with the advent of exhaustion, the child has to be extracted by force, or by some mechanical means, or the mother left to die undelivered.

Trismus, as the foregoing reports show, frequently occurs with placenta prævia, and as placenta prævia rarely goes on to full term, we have, co-existing with it, all those difficulties which belong to premature labour, as well as those which especially and solely concern placenta prævia itself.

In the cases which are reported, hæmorrhage does not seem to have been an important factor as a cause of death. Perhaps in one of Smellie's cases as well as in one of my own, it had a serious influence. It may be objected that Smellie's cases were not those of placenta prævia, but I think their histories indicate that they were such. Smellie does not speak of feeling the edge of the placenta, but yet this might have been implanted within the cervical zone of the uterine cavity so as to come within the category of placenta prævia. Although I have given three of Smellie's cases as being of a trismic kind, he does not seem to have differentiated them as such; but rather considered them to be cases of transverse presentation associated with an ordinary rigid state of the os uteri accompanied by flooding.

I have entitled this paper "Trismus and Tetanus of the Uterus," but it is chiefly with trismus that I am concerned, as it is more common than tetanus. A slight degree of tetanus, however, is not infrequently to be observed with trismus; indeed, tetanus may be considered an extension of trismus, sometimes totally, sometimes partially involving the uterus. In two of my own cases (2 and 4) there was well-marked tetanus.

The tetanus which occurs in connection with trismus at an early period of the first state of labour must be distinguished from that which enters at the end of the second stage of severe and protracted labour. The latter condition is the outcome of exhausted labour. The uterus, in obstructed labour, having for a long time used violent efforts to expel the child, becomes exhausted, ceases to act rhythmically, and subsides into a rigid, tense condition, its action being one of unremitting contraction.

Dr. Braxton Hicks, in an able paper,* has fully explained this kind of tetany, and the serious exhaustion of the organic powers of life generally, which follows it.

The exhausted uterus, remaining in a state of persistent contraction, has no

* On the Condition of the Uterus in Obstructed Labour; and an Inquiry as to what is intended by the terms, "Cessation of Labour Pains," "Powerless Labour," and "Exhaustion;" *Obs. Trans.* vol. ix.

interval of ease to recruit its nerve force. There is one constant drain on the ganglionic system, from which the uterus chiefly derives its nerve force, and indirectly a consequent exhaustion of all the vital organs which are dependent on the same system of nerves for their action.

The mere exhaustion of animal power, of which a patient loudly complains in protracted and severe labour, is of but little importance in result, but when the powers of organic life become depressed, the case then assumes a very serious aspect.

Between trismus and tetanus in an early stage of labour, and the tetanus of the exhausted uterus after severe labour, there is this resemblance: they both lead to death in the same direction. Firstly, by exhausting the nerve force of the uterus, then of the ganglionic system generally. In each case there is no interval of ease, during which a fresh store of nerve force can be generated. There are, however, strong points of difference between the two kinds of cases. The tetanus of exhausted labour occurs at the end of the second stage of labour, after severe and long-continued action of the uterus in insuperable obstruction; whereas trismus occurs at the beginning of the first stage of labour, without any previously severe action of the uterus. The condition of the uterus in the one case is initial, in the other it is terminal.

In one respect there is a sameness in these two kinds of cases—viz., the absence of rhythmic pains; indeed, in each case there are present none of the ordinary signs of labour, and unless a particular examination be made, or their histories known, one could not feel certain that such patients were in labour at all. The metritic condition mentioned by Spiegelberg as a cause of trismus is important. I am not certain that this was not an element in one of my own cases (No. 4).

Treatment.—(1.) The first and chief object is to subdue the spasm of the os and cervix, for this is the source of the difficulty. Without effecting this we shall not be able to do with safety what is required, whether we have to deliver by force a child normally placed, or by podalic version in case of malpresentation.

(2.) The common soothing methods, as warm baths, vaginal irrigation and the ordinary anti-spasmodics are useless.

(3.) Opium does not materially help us.

(4.) Chloroform, from which we might hope for perfect aid, is valueless so far as regards subduing the uterine spasm; but this is what we might expect from its well-known physiological action. Chloroform acts with completeness in temporarily arresting the functions of the cerebro-spinal system of nerves; but unless its influence be pushed to a dangerous extent, it has but little control over the ganglionic system of nerves, upon which the uterine action mainly depends. There can, however, be no difference of opinion in the conclusion that it should be administered during any operative procedure which may be determined on.

(5.) Mechanical stretching of the os, in Smellie's experience, failed. In Case 389, he says, "But although the os uteri was so dilated as to receive my thumb and four fingers, and felt as thin as the edge of a piece of parchment doubled, I could not stretch it wider, even though I proceeded in a slow manner and at intervals." He then desisted for a time, and further remarked:—"I went to work again, and used greater force than before, but to little purpose. I could do nothing but cramp and weary the fingers of both hands." In Case 396 he says:—"I by degrees introduced my fingers into the vagina, and afterwards my fingers into the os internum. This I endeavoured gently to stretch by pushing up my fingers in the form of a cone; but, to my surprise, found it so rigid that it would not dilate in the

least. I then used greater force, and repeated it several times by using one hand till it was fatigued and cramped, and then the other, but all to no purpose." This mode of treatment failed in my own cases. The dilating force we now possess in Barnes' bags has not been found successful in my own practice. In one case (No. 1) six of Barnes' dilators were burst by Mr. Brown and myself without producing the least expansion of the os.

(6.) Forceps used as a dilator would seem to afford us a hopeful prospect; but as this instrument can be used in cephalic presentations only, we cannot often have the opportunity of applying it, seeing that trismus so frequently occurs when the head does not present. Given a head presentation, I think it beyond question that the forceps should be used whenever the instrument can be introduced through the os.

Dr. McClintock, in his annotation to Smellie's case (No. 389), makes some observations on the dilating powers of the forceps. He remarks, "Beyond all doubt the os uteri yields more easily and more safely to a dilating force *ab intra* than to one *ab extra*."

Whilst agreeing with Dr. McClintock as to the superiority of a force *ab intra*, over a force *ab extra*, it may be said that traction on the fœtal leg after podalic version affords as good an example of an *ab intra* force as that exerted by forceps; yet in one of my cases no safe amount of traction thus used could expand the rigid os sufficiently large for the passage of the child.

(7.) Incision of the os, as recommended by Spiegelberg, and practised by Smellie in two cases, whilst it affords evidence of a recognition of the great obstacles with which we are confronted, has yet to have its value proved by further experience. Death was the result in each of Smellie's cases.

(8.) What benefit are we to derive from expectancy?

In Smellie's case (No. 387) and in one of my own (No. 3) the patients died undelivered, by being left, as some may allege, too long:—

It may be said that in each of these two cases there had been serious flooding; yet, from the mode of death, I think we must conclude that trismus was the greater factor. I will merely suggest how far it might be probable that in some of these cases, if left to Nature, the uterus would continue to retain the dead fœtus in its cavity for an indefinite time after the manner of so-called missed labour?

(9.) As transverse presentations so frequently occur in conjunction with trismus, version of the child will often be a necessary part of treatment. This operation will be found difficult in proportion to the degree of tetanus. With trismus simply, the bipolar method may succeed; but not when tetanus exists. But by whichever method we succeed in effecting version, then comes the great difficulty in extracting the child through the spasmodic os. No greater difficulties can be imagined than those which presented themselves in my own case (No. 4). After a foot had been brought down into the vagina, the thigh, half-breech, abdomen, and chest (after evisceration) were extracted only with the greatest difficulty, and the arms could only be liberated from the grip of the os by the use of the crotchet.

(10.) Decapitation was practised by me in one case (No. 2) by means of a small sharp hook introduced through the narrow os. Extraction in this case was much facilitated by the small size of the fœtus, combined with the soft state of its immature structures.

I hope my remarks will sufficiently elucidate the nature of this formidable complication of labour. The cases I have given from Smellie, with my own, afford striking examples of the difficulties they present in practice.

In conclusion, I venture to submit that trismus of the uterus, as a complication of placenta prævia, has not, hitherto, been fully recognized. Its importance is great in the treatment of placenta prævia, inasmuch as it is so frequently an obstacle to that speedy delivery which is so desirable on account of hæmorrhage.

The fear of danger from flooding has often led practitioners to effect forced delivery, thus causing an injury to the cervical structures, which becomes more disastrous than the hæmorrhage itself would have been if treated by less violent means.

A further consideration of trismus uteri in this point of view would extend far beyond the object contemplated in this paper.

I am willing to confess that I feel a great perplexity as to the adoption of any method of treatment from which a frequent or easy success may be expected.

I view the treatment of trismus as a problem not easy of solution, and I hope that any discussion which may ensue upon the reading of this paper will lead to the establishment of some definite and scientific line of treatment.

P.S.—The cases given in this paper to illustrate trismus and tetanus of the uterus are of the most severe kind, and mostly fatal.

It is however, not my intention to ignore the varying degrees of severity of this complication of labour.

Trismus and tetanus of the uterus present themselves in every degree: from the greatest intensity down to one that is only slightly evident.

DISCUSSION.

Professor Simpson, Edinburgh : I regret that in the cases which Dr. Roper has adduced from his own experience there is room to doubt whether they were really illustrative of a true trismus or spasmodic contraction of the cervix. They were all cases of placenta prævia ; and besides that the cervix, in such instances, has the ordinary organic rigidity of a prematurely-acting uterus, it has the additional rigidity attendant on the morbid condition of the uterus, which favoured the occurrence of the placenta prævia. The cervix I believe to be perfectly passive during labour, and the only cases where I have ever met with any trismus in it were those in which the sphincter of the os internum was in a state of spasmodic contraction. One of the cases in which I have seen it was a patient in whom the child presented by the head, but with the arm folded back on the nucha. The spasm of the upper part of the cervix was overcome by steady, long-continued traction of the head with a pair of axis-traction forceps.

Dr. Paul Mundé, New York : I saw a case of tetanus of the uterus while assistant to Professor Scanzoni, some thirteen years ago. The case was that of a primipara in tedious labour, for which no cause could be ascertained ; the os was dilated to nearly the size of the palm of the hand, perfectly flabby, the head in the pelvic brim. After waiting some forty-eight hours without an increase of the pains, and employing the few allowable measures for exciting pains (hot baths, injections, active motion), I ruptured the membranes, thinking that no harm could be done thereby. But soon the patient began to complain of tearing abdominal pains, the uterus became harder and harder, finally quite board-hard, with no intermediate relaxation and contraction, the os again became smaller, and as rigid as a cord ; and this tetanic contraction prevailed for fully thirty-six hours, in spite of hot baths, chloroform, morphine. The child succumbed to the constant pressure of the placenta. Finally it was decided, the os having become slightly relaxed, to extract, and the forceps were

applied, and ineffectual steady traction made for over an hour by several gentlemen; then the head was trephined and extracted with the cephalotribe. The mucous linings of the vagina sloughed off in consequence of the pressure, but the woman finally made a good recovery. Since this case I am, with rare exceptions, very careful to avoid rupturing the membranes before complete dilatation of the os.

Dr. Braxton Hicks, London: I wish to point out the great importance of recognizing this condition, to which I have paid particular attention; as it is on the want of early recognition of it that the great danger arises. As soon as it has commenced, and long before it is severe, it is imperative to act, either by withdrawal or diminution of the passenger. With respect to the cases narrated, it is difficult to understand the exact condition, whether it was a general trismus extending from the os to the whole body, or whether it was only of the os and lower uterus. And this point was scarcely explained by the author. My own opinion leads me to think that it is general, springing from impediment at the os. In two cases I found, during the removal of fœtus, that the uterus remained not lax, but still rigid and distended, though empty.

Dr. Van den Bosch, Liège, showed a crotchet and lamineur, by Professor Wasseige, of Liège.

Professor Hennig, Leipzig, showed Billroth's clamp, for holding temporarily the pedicle of tumours.

Professor Küstner, Jena, showed his pelvimeter.

Another Uterine Dilator.

Dr. Henry O. Marcy, Boston, Mass.

The instrument which I shew, as now modified, was originally intended for quite a different purpose—that of reducing an inverted uterus.

Here I obtained the idea of an elastic pressure and counter-pressure being produced by one and the same force used for dilating the cervix uteri.

At first I used metallic rings sewed firmly into the points of fixation in the cervix—generally four—and through them drew elastic cords which traversed the hollowed handle of the instrument and had their point of fixation behind it. I derived perhaps just as good results as thus used, but the instrument was cumbrous and inconvenient, Having thus ascertained that the stitches subjected to strain did not cut out, which I at first very much feared, I substituted a spiral spring concealed in the body of the instrument (and this is why the shaft is so large, but it should be smaller).

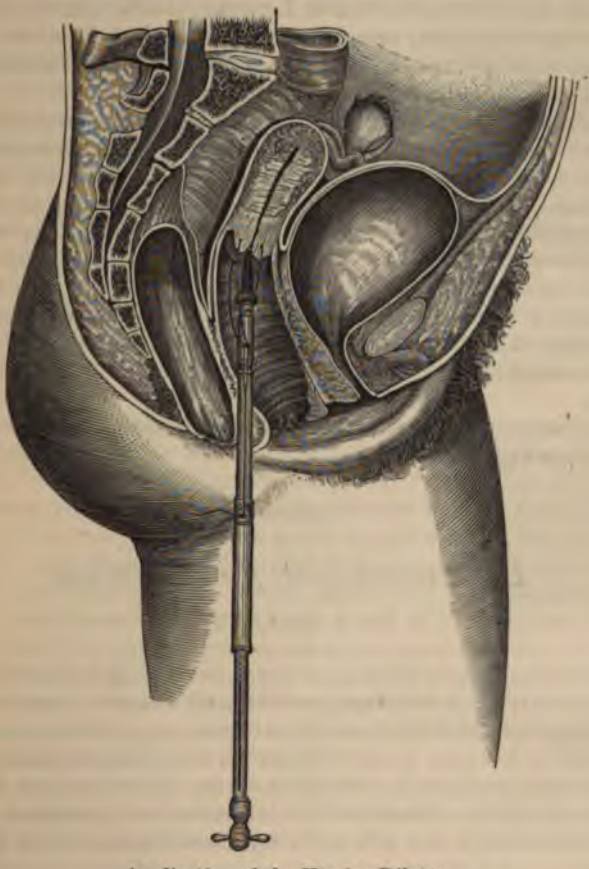
By this modification I was enabled to do away altogether with the rings and rubber, and it much simplifies the instrument. There is so little pain attending the operation that it is usually preferable to make the application without ether, and then we are advised of the suffering produced. Placing your patient in the lateral semi-prone position, by the use of the Sim's speculum, you readily introduce somewhat deeply a needle without cutting edges, armed with a quite long strong ligature into each side of the cervix (I think it quite important to use a round needle).

These—and usually two are sufficient—are drawn through a loop upon each side of the instrument and secured to the projections upon the cylinder opposed to the spring.

The force is graduated at the distal extremity by a screw not unlike that of the écraseur.

The amount of this force is read upon a scale in the side, and is graduated to 15 lbs., however, I have rarely used more than the half or two-thirds of this power in dilating.

Fig. 44.



Application of the Uterine Dilator.

We do not presume to enter into discussion of the dilatation of the uterus, or advise the instrument here shown as the best in all cases. With the marked improvements in instruments for this purpose during the last few years, why inflict upon the profession, over-burdened in this direction already, a new instrument at all? Certainly not unless it can be shown to have some advantages.

All the elastic intra-uterine dilators, such as sponge tents, sea-tangle, rubber, &c., have this evident disadvantage, they dilate much where the pressure is least—that is, they first swell and fill the uterine cavity, and when removed are always, as we know, constricted at the only point where we desired to dilate the canal, presenting usually more or less of an hour-glass shape.

Now I think two propositions will be readily granted me; first, that, as a rule, we do not desire to irritate or inflame with the dilating substance, of whatever material, the endometrium of the uterine cavity; and secondly, that when we do have accidents or complications happening because of the use of dilators, it almost always occurs from the injury or inflammation of the body of the organ. It is generally admitted that the cervical portion of the uterus can be submitted to a great variety of manipulations almost without danger, and it is equally true that, as a rule, this is the part requiring rectification. Dysmenorrhœa due to stenosis or flexion I thought to have been in this way more radically cured, as well as safely treated, because the combined forces, brought to bear upon the part, act in most thoroughly modifying the nutrition and subsequent condition of the tissues, since the cervix is elongated in ratio to the dilatation.

The process can be continued as may be required by the substitution of one size after another. Conical probe extremities may be used, and in some cases very probably preferred, but I have hitherto used only a blunt-pointed cone. In one instance of extreme weakness and emaciation, produced by the vomiting of pregnancy, I continued the dilating pressure at a low tension for eight hours, very decidedly modifying the condition of the cervical tissues without either relieving the suffering or causing abortion, which finally, some days afterwards, I was obliged to produce. In cases where only a moderate change is required I still frequently use other instruments, Ellinger's preferred. Caution is required in watching the process, of dilatation, otherwise there might be produced, that which I have laboured to avoid, the introduction of the dilator further than desired.

With this reservation I shall believe dilatation will be not only safe but the result derived therefrom highly satisfactory.

Les Accouchements par l'Electricité.

Dr. Apostoli, Paris.

Je viens vous entretenir d'une façon sommaire de l'application de l'électricité aux accouchements; je dois d'abord justifier le titre de ma communication: *Les accouchements par l'électricité;* il pourra peut-être vous paraître prétentieux et hors de propos avec ce que peut donner l'électricité, mais j'espère vous montrer que l'application thérapeutique de cette force trouve des indications fréquentes et multiples en obstétrique, d'abord pendant la grossesse et le travail, et qu'elle devra ensuite se généraliser, à mon avis, après les accouchements. Mon but, en effet, est double:—

1.—Restaurer une application ancienne, mais délaissée à tort, et trop souvent critiquée; légitimer en un mot l'application de l'électricité pendant le travail; donner ses indications, ses moyens et ses résultats.

2.—Vous entretenir d'applications nouvelles, sur lesquelles j'appelle toute votre attention soit, quelquefois, pendant la grossesse pour combattre des troubles locaux ou réflexes contre lesquels nous sommes le plus souvent impuissants; soit toujours après la délivrance, que ce soit couche ou fausse-couche, pour remplir une indication prophylactique du plus haut intérêt.

C'est de l'Angleterre que nous sont venues les premières applications obstétricales de l'électricité dans la personne de Radford, Barnes, Mackenzie, &c., mais c'est au docteur A. Tripier, de Paris, mon maître en électrothérapie, que l'on doit

les vues les plus rationnelles et les plus complètes qui ont tracé la voie que je vais vous décrire.

Et d'abord, Messieurs, quel est le mode électrique qu'il faut employer; quel sont ses avantages, et comment faut-il l'appliquer? Nous devons toujours nous servir du courant induit ou faradique, engendré par une bobine à fil gros et court; c'est le courant dit de quantité, en opposition avec celui qui est dit de tension engendré par une bobine à fil long et fin. Seul, ce courant de quantité a une action puissante et immédiate sur la contractilité musculaire, et peut remplir suivant le besoin, une indication double:

1.—Action d'abord mécanique ou physique; par la voie de contraction musculaire il fait raccourcir temporairement tout muscle, lisse ou strié, compris entre ses deux points d'application; après avoir produit de toutes pièces la contraction musculaire, il peut devenir au besoin un agent orthopédique ou de redressement toutes les fois qu'il s'adresse à une déviation organique tenant à un relâchement musculaire.

2.—Action dynamique ou vasculaire exerçant, par la contraction des fibres lisses des vaisseaux sanguins, une action directe sur la circulation, l'activant partout où elle est languissante ou paresseuse, combattant directement la congestion et la stase, faisant en un mot, par le moyen du massage musculaire interstitiel du vrai drainage circulatoire; elle rétablit l'équilibre partout où il est perverti, et a par suite un retentissement direct sur la nutrition des organes dont elle a influencé le rythme circulatoire.

Voulez-vous un exemple tiré de l'ordre chirurgical? Prenez une entorse simple, celle qui est curable rapidement par le massage manuel; faradisez cette entorse comme vous le ferez pour l'utérus, en traversant l'articulation de part en part par un courant de quantité; une ou deux séances de trois à cinq minutes vous donneront un résultat immédiat, plus rapide et plus complet s'il est possible que par le massage; la douleur aura disparu et le mouvement sera plus rapide. Que s'est-il donc passé sous la seule influence instantanée de la faradisation? Une partie de l'épanchement s'est résorbé, ou du moins la liberté a été rendue à une circulation qui se trouvait entravée ou abolie par la compression d'un épanchement interstitiel; les fibres musculaires *intus* et *extra* des parois vasculaires se sont contractées, et une suractivité dans la propulsion sanguine s'en est suivie; il y a donc eu action dynamique manifeste dont le résultat ne saurait être nié. Pareille chose va se passer avec l'utérus.

Nous avons donc en main une force puissante. Nous pouvons la doser, c'est-à-dire en graduer l'emploi à volonté suivant l'effet à obtenir, et suivant la résistance à la contraction ou l'inertie musculaire.

Nous pouvons la localiser, limiter le champ de son action, la concentrer dans un organe périphérique ou central, lui assigner en un mot un siège qu'elle ne dépassera pas.

C'est donc une force soumise que nous possédons complètement, débutant et finissant à volonté, et dont nous pouvons, chose très-précieuse, répéter l'action aussi souvent que nous le voulons, sans aucun dommage ni aucune altération des organes sur lesquels elle est appliquée.

Tout appareil faradique peut, au besoin servir, mais je ne saurais trop vous recommander celui de A. Gaiffe, de Paris, qui réalise les conditions suivantes: avoir sous un petit volume, un appareil porteur de sa pile avec une bobine mobile, qui peut ainsi, à volonté, passer graduellement d'un minimum zéro, au maximum.

Car cela est très-important dans la pratique, surtout pour l'utérus qui supporte

mal les surprises et ne saurait tolérer d'emblée en général un courant qui débuterait par une certaine intensité.

Il faut préparer sa contraction par une progression sans secousses et sans chocs de la bobine, de zéro à une quantité donnée, pour avoir le maximum d'effet et surtout le minimum de douleur.

Pour électriser l'utérus voici en quelques mots les conditions indispensables et fondamentales du manuel opératoire* dont l'inobservance a conduit à des fautes, et à l'abandon ou à la critique d'une force, dont on n'avait pas su tirer tout le parti thérapeutique qu'elle est destinée à rendre.

1.—Il faut, pour obtenir le maximum d'effet, que le courant soit appliqué le plus près possible de l'utérus, et qu'un des pôles, le négatif, soit toujours, si c'est possible, introduit dans sa cavité; le circuit est fermé par deux larges tampons, très-humectés d'eau, et appliqués au-dessus du pubis, de chaque côté de la ligne blanche, en évitant de se rapprocher du pli de l'aine.

S'il est impossible d'introduire dans l'utérus la sonde qui forme le pôle négatif, comme cela a lieu pendant la grossesse ou le travail, il faut alors l'appliquer dans un des culs-de-sac, et de préférence dans le postérieur; ou bien il faut fermer le circuit sous le sacrum par une large plaque métallique recouverte de peau de chamois, largement imbibée d'eau.

2.—Si la sonde est placée dans l'utérus, elle doit être tenue avec une légèreté extrême, le bec en avant, appuyé sans aucune pression sur la face interne de la paroi antérieure de l'utérus.

3.—Il faut que pendant l'opération toute la douleur résultant du passage du courant et de la contraction utérine soit concentrée dans le ventre au-dessus du pubis, au niveau de la ligne blanche, et que le point d'application de la sonde, surtout si elle est dans l'utérus soit toujours et absolument indolore.

On doit éviter en un mot, toute action mécanique de la sonde, capable de produire une lésion et des complications qu'on pourrait imputer à tort à l'électricité, ainsi qu'une position vicieuse, latérale ou postérieure, capable de produire une diffusion du courant dans le plexus sacré, et par suite de provoquer dans les jambes une douleur très-vive absolument stérile.

Il faut donc prévenir la femme de ce qui va se passer, d'abord pour éviter toute surprise chez elle, et puis pour qu'elle avertisse immédiatement l'opérateur dans le cas où elle éprouverait une douleur autre que celle obligatoire. Toute sonde douloureuse ou même sensible, toute douleur irradiée dans les cuisses ou les jambes, prouve que l'opération est mal faite, et que la sonde est mal placée, soit qu'elle butte trop fort contre une paroi utérine, soit que la situation trop latérale ou postérieure du bec favorise la diffusion du courant dans les nerfs sciatiques; une rectification est alors immédiatement nécessaire.

4.—La dose et la durée de la faradisation seront proportionnées avec l'effet à obtenir.

Veut-on combattre une inertie? S'arrêter comme intensité, et comme durée, dès que la contraction apparaît, pour lui permettre d'évoluer toute seule.

Est-ce une hémorrhagie qu'on veut arrêter? Persevérer jusqu'à ce qu'elle soit au moins diminuée et recommencer aussi longtemps et aussi souvent qu'il y a à craindre une récidive.

Fait-on une application prophylatique après l'accouchement pour hâter l'invo-

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* Voir pour plus amples détails mon traité sous presse, "Les Accouchements par l'Electricité."

lution utérine, ou une application thérapeutique pendant la grossesse pour combattre un trouble local ou réflexe, trois minutes de durée, et une intensité qui peut aller des deux tiers au maximum de cette bobine sont, en général, suffisantes pour obtenir le résultat cherché.

5.—Ne jamais interrompre brusquement une séance ; ramener d'abord la bobine en arrière pour diminuer l'intensité, et lever ensuite le trembleur.

J'arrive, pressé par le temps, aux applications thérapeutiques.

Que peut-on, que doit-on faire pour l'utérus en obstétrique ? Voici comme, à vol d'oiseau, et rangées par ordre chronologique, les indications qu'il m'a été donné de remplir avec un succès constant.

1.—Une femme enceinte se présente à vous avec des désordres multiples si fréquents du système nerveux ou digestif : inappétence, gastralgie, vomissements incoercibles, névralgie à mille formes différentes, véritable protée à aspect multiple et à gravité variable qui constitue tout autant de troubles d'ordre réflexe, tenant à des lésions circulatoires de l'utérus, dont le modus nous échappe, mais dont le siège est manifeste. Vous connaissez toute l'impuissance de la thérapeutique en pareille circonstance ; eh bien ! faradisez alors l'utérus, comme je l'ai fait un grand nombre de fois, en prenant des précautions pour la sonde qui est toujours ici placée dans un cul-de-sac, ou à l'entrée de l'orifice, et vous aurez alors une sédation très-rapide des phénomènes morbides, ce qui est d'un double enseignement. Cela prouve d'abord que la cause pathogénique est bien dans l'utérus, intéressant surtout l'appareil circulatoire, puisque c'est sur lui que l'on agit en cette circonstance, en modifiant la modalité circulatoire.

Cela démontre ensuite, ainsi que l'a très-bien dit le Dr. A. Tripier, que la faradisation n'est pas, quoique on ait écrit tout le contraire, un abortif ; car, je déclare que j'ai pu électriser en toute sécurité des femme senceintes dans un but thérapeutique. Il faut donc autre chose pour faire avorter une femme que la contraction utérine ; s'il n'en était pas ainsi en effet, elles avorteraient toutes, puisqu'elles ont toutes des contractions conscientes ou non de l'utérus pendant la grossesse ; pour provoquer l'accouchement ainsi que l'avortement il faut le décollement préalable de l'œuf ; sans décollement initial, pas de travail possible. Aussi ceux qui ont voulu demander à la faradisation ce qu'elle ne peut donner, la mise en œuvre du travail non commencé, ont été induits en erreur ; elle est impuissante par la contraction utérine seule à provoquer le décollement de l'œuf, et par suite le début du travail.

2.—Pendant les premiers mois de la grossesse une femme a un enclavement de son utérus en rétroflexion considérable ; que fait-on pour la préserver d'un danger imminent ? Tous les moyens conseillés sont le plus souvent infidèles ou dangereux. Faradisez alors l'utérus en concentrant l'action électrique sur sa face antérieure par un pôle placé dans le cul-de-sac antérieur, et l'autre dans la vessie ou au-dessus du pubis, et vous opérerez le redressement de cet organe par le raccourcissement progressif de la paroi antérieure relâchée.

3.—J'arrive aux indications pendant le travail, et d'abord l'inertie utérine. Devons-nous rester désarmés, comme on l'est presque toujours, en présence d'une inertie utérine qui rend l'accouchement long et laborieux, sous le fallacieux prétexte que la vie de la mère ou de l'enfant ne sont pas directement compromises ?

Non, Messieurs, parce qu'il faut toujours songer à la sécurité de l'avenir ; j'entends surtout parler ici de l'inertie utérine qui se manifeste avant la dilatation du col, suffisante pour permettre une intervention chirurgicale, telle que le forceps ou la version. La fatigue utérine, en effet, est, toutes proportions gardées, en raison

de la longueur du travail ; or, l'utérus fatigué se contracte, mal ou peu après la délivrance, l'involution utérine est plus lente et plus difficile ; c'est en un mot une maladie qui pourra éclater demain, et dont l'inaction du médecin a préparé l'évolution. Le seigle ergoté devant être légitimement proscrit pendant le travail, et tous les autres moyens étant presque illusoires, vous avez sous la main un agent précieux de contraction constante de l'utérus, et d'accélération fréquente du travail. Je dois, en effet, placer ici un correctif très-important : la faradisation, quoique produisant une contraction utérine constante, n'accélère pas toujours le travail de dilatation. Pourquoi cela ? Quoique le chapitre des inerties utérines soit encore à faire, on peut toutefois constater le fait suivant : il y a des inerties où le centre cérébral a une prépondérance telle que le centre spinal ou réflexe est arrêté ou diminué dans son fonctionnement. Donnez alors du chloroforme à la femme et le centre cérébral étant annihilé, le centre spinal reprend toute son autonomie, et tel utérus dont le travail ne marchait pas malgré la faradisation, subira à l'instant une stimulation nouvelle pour aboutir rapidement à la délivrance.

Donc, en face d'une inertie, la faradisation vous aidera toujours, et vous devrez, comme je l'ai fait toujours, y recourir ; mais à elle seule, elle ne sera pas constamment suffisante.

4.—Après l'inertie utérine vient l'hémorrhagie que je veux considérer dans son ensemble, qu'elle se produise pendant le travail ou bien après la délivrance. Quel que soit le moment de son apparition il faut l'arrêter, ou du moins essayer de l'atténuer le plus possible ; or, je ne connais pas de moyen plus rapide et plus puissant que la faradisation. Le seigle ergoté est long à agir, son action est souvent infidèle, et sa dose ne peut être impunément accumulée, ni trop souvent renouvelée. La faradisation, au contraire, docile dans notre main, fait immédiatement contracter l'utérus, et je l'ai toujours vu agir dans l'espace de trente secondes à une minute pour arrêter, ou tout au moins diminuer les hémorrhagies que j'ai observées après la délivrance.

Que son action soit mécanique, en s'opposant directement par la contraction des parois utérines à l'écoulement sanguin, ou que son action soit dynamique, et qu'elle s'oppose à certaines hémorrhagies passives, telle que celle après la délivrance, en accélérant la circulation, et en combattant directement la stase ou la congestion vasculaire qui sont les premiers facteurs de ces hémorrhagies, peu importe, le résultat est indéniable, et je vous engage à y recourir, toujours sans préjudice des autres interventions que pourraient réclamer, par exemple, l'hémorrhagie dûe à une insertion vicieuse du placenta.

5.—L'expulsion du fœtus vient d'avoir lieu, la délivrance naturelle ne se fait pas ou se fait longtemps attendre ; que fait-on d'une façon classique ? après une couche on conseille d'aller détacher et cueillir le placenta, de faire en un mot la délivrance artificielle ; après une fausse-couche on conseille, faute de mieux, d'attendre et de parer aux accidents par des irrigations intra-utérines antiseptiques. Dans l'un et l'autre cas il peut y avoir, et il y a souvent, péril et danger, résultant, d'un côté, d'une intervention trop active, et de l'autre, d'une intervention trop passive. Quant à moi, dans ces deux cas je faradise l'utérus et rien de plus—j'attends. Je faradise tous les jours, une, deux fois, jusqu'à ce que la délivrance se fasse spontanément.

Mais de même que la faradisation est impuissante à provoquer, *ipso facto*, le début du travail, et que pour qu'elle puisse porter tous ses fruits, elle réclame le décollement préalable de l'œuf, de même pour la délivrance, il faut un décollement préalable du placenta, la contraction utérine spontanée ou artificielle est impuissante à obtenir ce résultat.

J'ai eu des exemples multiples de l'innocuité et de l'efficacité de cette intervention; j'ai vu des rétentions du placenta durant de un à quinze jours, où grâce à la faradisation patiemment continuée j'ai conjuré tout accident résultant de la rétention du délivre, et où je me suis applaudi de l'uniformité et de la simplicité de mon intervention.

A la faveur de cette suractivité circulatoire, de cette hypérémie vasculaire imprimée à l'utérus porteur d'un placenta, on prévient la stase de la congestion vasculaire, et je crois que c'est par ce *modus faciendi* qu'on évite toute résorption infectieuse, et qu'on assiste à ce spectacle curieux à tous égards, d'un utérus qui peut conserver sans aucun péril un placenta un et plusieurs jours avec une sécurité complète.

6.—J'arrive enfin, Messieurs, à une indication nouvelle, capitale et maîtresse, celle à laquelle j'attache le plus de prix, parce qu'elle ne souffre pas comme les autres des cas particuliers, mais parce qu'elle peut et doit, à mon avis, s'appliquer après toute couche ou fausse-couche.

Une femme vient d'être délivrée, de grands devoirs lui restent encore à accomplir pour récouvrer la santé totale, et prévenir la maladie. L'involution utérine doit se faire complétement, sinon il y a péril pour elle. Or, que fait-on pour l'y aider, que fait-on pour assurer le retour à l'état normal de cette fonction physiologique? On donne du tilleul, et on reste les bras croisés devant elle, comptant sur la bonne nature pour la réparation à laquelle nous nous déclarons impuissants de co-opérer. Mais le séjour au lit? me direz-vous. Oui, le séjour au lit est une excellente chose, et en présence de l'inaction classique, il est même indispensable; mais combien de temps les femmes y restent-elles? A peine neuf jours, pour la grande majorité, et pour beaucoup, moins de neuf jours.

Le préjugé public existe, il est général, et je crois que nos efforts réunis seraient impuissants à l'ébranler, il veut que les femmes soient totalement remises au bout de neuf jours, et que beaucoup d'entre elles puissent reprendre leurs occupations à cette date; or je vous le demande, l'involution utérine est-elle terminée à ce moment? Votre réponse est uniforme je pense: elle est à peine arrivée au deux tiers de son évolution totale, et le reste va s'accomplir dans la station debout, et au milieu d'une déambulation plus ou moins laborieuse.

Quant aux femmes qui, par exception, consentent à rester au lit quinze et vingt jours, elles sont menacées de ce fait, d'une rétroflexion ou rétroversion de l'utérus qui résulte du retrait trop lent d'un organe gros, congestionné, molasse et qui s'effectue dans un décubitus dorsal le plus souvent trop prolongé.

Nous nous trouvons donc en présence d'une évolution pathogénique multiple, dont nous connaissons le premier anneau; elle afflige les femmes des grandes villes dans une proportion de plus en plus effrayante, et l'on peut la résumer en trois mots: hémorrhagie secondaire—engorgement ou métrite—rétroflexion ou rétroversion.

Voilà le mal qui par sa fréquence et sa gravité, est un vrai mal social. Voici le rémède que je formule ainsi: étant donné une femme qui vient d'être délivrée d'un enfant à terme ou non, je faradise immédiatement son utérus pendant trois minutes environ; je renouvelle cette opération de huit à dix fois pendant six jours en moyenne après un accouchement à terme et normal; de quinze à vingt fois, après une fausse-couche ou un accouchement laborieux, pendant dix à quinze jours en moyenne. Que fais-je ainsi, Messieurs, et comment justifier physiologiquement cette méthode? Rien n'est plus simple. Je sollicite l'involution utérine; je l'accélère et je la termine sûrement; je ne permets pas à l'utérus de s'arrêter ou de faire une halte dans la marche de son retrait. A l'inertie utérine, à la stase ou à la

congestion sanguine, qui se manifestent si fréquemment dans l'état puerpéral, je réponds par la stimulation faradique. Je combats ainsi l'inertie si elle existe, et la préviens dans les autres cas; j'augmente par le jeu des parois vasculaires le débit artériel et veineux, luttant ainsi contre la congestion ou mieux la stase veineuse, premier facteur de toute hémorrhagie secondaire et de tout engorgement ultérieur.

Par une contraction immédiate et puissante, imprimée à l'utérus, je lui restitue d'emblée sa place et sa direction normale, sans lui laisser le temps de s'infléchir ou de se dévier.

La faradisation est donc pour moi l'adjuvant, ou le complément de la nature, qui en a si souvent besoin.

Voilà ce que la théorie avait inspiré, voici ce que la clinique a répondu: Les femmes se portent immédiatement mieux. Leur convalescence est plus rapide. L'involution utérine est considérablement accélérée.

Depuis deux ans et demie, j'ai suivi pas à pas l'évolution de la santé de la plus part de mes accouchées, et j'ai pu ainsi constater, chez toutes celles que j'ai soignées complètement, qu'aucune n'a eu d'hémorrhagie post-puerpuérale; qu'aucune n'est actuellement atteinte d'engorgement ou de métrite; et qu'elles n'ont pas de rétroflexion ou de rétroversion.

Mon observation actuelle porte sur quarante cas, qui comprennent couches et fausse-couches, et une grande partie d'accouchements laborieux; j'ai fait près de six cent faradisations de l'utérus à l'état puerpuéral, aussi sans témérité je crois pouvoir conclure:—

1.—Que la faradisation de l'utérus est toujours absolument inoffensive.

2.—Qu'elle restaure la femme le plus promptement possible, et assure son avenir utérin, exerçant ainsi une action prophylactique des plus justifiées qui doit avoir un retentissement salutaire sur les causes si fréquentes de la stérilité.

En finissant, Messieurs, si je devais, d'un mot, résumer ma communication tout entière, je lui donnerais cette formule:

L'électricité passe aux yeux de la plupart des médecins pour un médicament excitant, convulsivant, phlogistique dont il faut se méfier et craindre la réaction inflammatoire. Eh bien! c'est absolument tout le contraire qu'il faut dire: la faradisation bien appliquée est un calmant, un sédatif, et un antiphlogistique dont je ne saurais trop vous conseiller l'emploi.

Voilà, Messieurs, ce que j'ai vu, et j'ose espérer que ces observations ébauchées, confiées à vos lumières et à vos recherches nouvelles, ne tarderont pas à porter leurs fruits.

Mittheilungen über die Erfahrungen mit der, seit dem ersten April 1880 im Königreich Sachsen, eingeführten Instruction für die Hebammen zur Verhütung des Kindbettfiebers.

Professor WINCKEL, Dresden.

Nicht mit Thesen will ich Sie heute behelligen, nur mit Thatsachen vor Sie treten, und Ihnen Erfahrungen mittheilen, die hoffentlich für Manchen von Ihnen Anlass zu weiterer Prüfung dieser so wichtigen Frage geben werden. Das Königreich Sachsen, welches beiläufig eine Bevölkerung von etwa zwei und eine halbe Million Einwohner hat und jährlich zwischen 125,000 und 132,000 Geburten, hatte in den

letzten 5 Jahren vor 1878 eine puerperale Mortalität von 610 pro Jahr. Die Zahl seiner Hebammen beträgt über 1,700 und sind dieselben auf 33 Bezirke vertheilt. Seit dem 1. April vorigen Jahres ist nun für diese Personen, auf Grund der von mir in dem dritten Bande meiner "Berichte und Studien," S. 441–442 gemachten Vorschläge, eine von dem königl. Landes-Medicin.-Colleg. entworfene und von der Regierung approbirte Instruction erlassen worden, über die ich jetzt berichten werde, so weit von den betreffenden Bezirksärzten dieselben gemacht worden sind.

Instruction für die Hebammen zur Verhütung des Kindbettfiebers.

§ 1.—Die Hebamme soll dafür sorgen, dass womöglich jede von ihr berathene Schwangere schon vor ihrer Entbindung sich ein eigenes Mutterrohr, einen Katheter und eine Flasche der unten verschriebenen flüssigen Carbolsäure anschaffe.

§ 2.—Die Hebamme soll, da sie öfter erst zu Kreissenden geholt wird, eine genügende Menge solcher Carbolsäurelösung stets mit sich führen, und immer mehrere ganz neue Katheter und Mutterrohre vorräthig halten, um den betreffenden Gebärenden dieselben käuflich überlassen zu können.

§ 3.—Vor und nach jeder inneren Untersuchung einer Schwangeren, Kreissenden oder Wöchnerin hat die Hebamme sich jedesmal mit Seife und unter dem Gebrauch einer guten Nagelbürste sehr gründlich beide Hände, besonders aber die Gegend über und unter den Nägeln in einer 1—2procentigen Carbolsäurelösung zu reinigen.

§ 4.—Katheter, Mutterrohre, leinene Tücher zum Reinigen der Kreissenden, welche bei der Entbindung gebraucht werden, sollen während derselben stets in 2procentiger Carbolsäurelösung liegen.

§ 5.—Zur Einölung der Instrumente und der Finger nach dem Waschen der Hände darf nur 2procentiges Carbolöl oder 2procentige Carbolvaseline benutzt werden. Beide kann sich die Hebamme zu Hause bereiten, indem sie 15 Tropfen ihrer flüssigen Carbolsäure mit 3—4 Esslöffeln voll gutem Tafelöl oder mit derselben Menge Vaseline tüchtig umrührt; oder sie kann dieselben aus der Apotheke beziehen, soll jedoch eins von Beiden stets mit sich führen.

§ 6.—Nach einer jeden Geburt sind erstlich in der Seitenlage der Entbundenen bei Besichtigung des Dammes die äusseren Geschlechtstheile der Wöchnerin mit 2procentiger Carbollösung, ohne Schwamm, nur mit feiner weicher Leinewand abzuwaschen, und zweitens ist, dafern nicht der Arzt etwas Anderes bestimmt, eine Einspülung von ½ Liter einer 2procentigen Carbolsäurelösung in die Scheide der Entbundenen zu machen.

Es empfiehlt sich, jede Wöchnerin in den ersten 5 Tagen des Wochenbetts womöglich täglich zwei Mal, bei übelriechendem Wochenfluss öfter, jedesmal mit ½ Liter 2procentiger Carbolsäurelösung auszuspülen, vorsichtig mit einem reinen Tuche abzutrocknen und dann die Geschlechtstheile derselben mit einem in 2procentiger Carbolsäurelösung ausgerungenen Stopftuche oder mit Salicylwatte oder Carboljute, ähnlich wie mit einem Stopftuche, zu bedecken.

§ 7.—Die Hebamme hat dem Bezirksarzte von jedem Falle schwerer fieberhafter Erkrankung einer ihrer Wöchnerinnen oder von einem Falle, den der hinzugezogene Arzt für Kindbettfieber erklärt hat, sofort Anzeige zu erstatten.

§ 8.—Einen jeden Todesfall einer Wöchnerin hat die Hebamme ungesäumt, wenn möglich persönlich, oder sonst schriftlich mit Angabe der Todesursache ihrem Bezirksarzte anzuzeigen.

§ 9.—Ist eine der, von der Hebamme entbundenen Wöchnerinnen am Kindbettfieber erkrankt, so soll sie dieselbe nicht mehr selbst besuchen, sondern diese

kranke Wöchnerin durch die Angehörigen oder durch Kranken- oder Wochenwärterinnen allein verpflegen lassen.

§ 10.—Ist eine der Wöchnerinnen einer Hebamme am Kindbettfieber gestorben oder sind in ihrer Praxis zwei oder mehrere Wöchnerinnen kurz hintereinander am Kindbettfieber erkrankt, so darf die Hebamme 14 Tage hindurch keine Entbindung übernehmen, sondern muss in dieser Zeit durch gründliche Reinigung ihrer Kleider und durch täglich 1—2malige Waschung ihrer Hände und Vorderarme in 5procentiger Carbolsäurelösung und unter jedesmaliger Benutzung der Nagelbürste sich auf das Sorgfältigste säubern.

§ 11.—Von ihrem Bezirksarzte erhält die Hebamme eine flüssige Carbolsäure verschrieben, welche aus 150 Gramm krystallisirter Carbolsäure und 7½ Gramm destillirtem Wasser besteht. Mittelst derselben bereitet sie sich eine 1 oder 2procentige Carbolsäurelösung, indem sie ein oder zwei Theelöffel voll jener flüssigen Carbolsäure in ½ Liter reines Wasser giesst, eine 5procentige, indem sie 5 Theelöffel voll zu derselben Menge Wasser nimmt.

Die Flasche mit der flüssigen Carbolsäure soll die Aufschrift haben:

Stark ätzende Carbolsäure.
Zu Waschungen nach Vorschrift
1, 2 oder 5 Theelöffel voll auf
½ Liter Wasser zu nehmen.
Für die Hebamme N.
in N.

In dreifacher Beziehung gedenke ich die Berichte dieser Aerzte aus dem ganzen Lande Ihnen vorzuführen, nämlich erstens in Betreff jedes einzelnen Paragraphen der Instruction selbst, die ich Ihnen hier in mehreren Exemplaren vorlege, zugleich mit einer Karte der Bezirke Sachsens, zweitens in Bezug auf die von jenen zu der Instruction gemachten Verbesserungs-Vorschläge, und endlich bezüglich einiger Urtheile und Ansichten jener Aerzte über die Bedeutung der Instruction. Hierbei sei bemerkt, dass nur von einem Bezirksarzt kein Wort über die Instruction vorgebracht worden ist.

Was nun zunächst die einzelnen Paragraphen betrifft, so wird von 5 Bezirken gemeldet, dass § 1 nur unvollständig oder selten befolgt sei, weil er beim Publikum und selbst bei Aerzten auf Schwierigkeiten stosse, des Kostenpunktes wegen; es handelt sich nämlich um die Vorheranschaffung von Carbolsäure, Katheter und Mutterrohr, auf Kosten der Schwangeren; jene Bezirke sind: Leipzig Stadt, Dippoldiswalde, Annaberg, Chemnitz und Borna.

Zu § 2 wird bemerkt, dass über die Kosten dieser Vorschrift die Hebammen in 2 Bezirken (Oschatz Stadt und Annaberg) geklagt hätten, dass die Carbolsäure zwar von den Hebammen vorräthig gehalten wurde (Löbau, Flöka) aber nicht Instrumente (Löbau). Der Bezirksarzt von Leissnig berichtet, dass 3 Gemeinden einer unbemittelten Hebamme, die zur Ausführung der Instruction nöthigen Instrumente auf ihre Kosten angeschafft hätten; viele Aerzte ferner in Leipzig Stadt und Land, Flöha, Grossenhain, Oschatz und andere, dass sie dafür Sorge getragen, den Hebammen die Carbolsäure möglichst billig zuzustellen, und Dresden Stadt endlich liefert seinen Hebammen dieses Präparat ganz umsonst.

§ 3 wurde nach Angabe des Bezirksarztes in Wurzen mannigfach nicht befolgt, ebenso in Plauen, Dresden Stadt, und ferner klagten die Hebammen, dass die Hände nach den Waschungen mit Carbolsäure aufsprängen (Glauchau).

Zu § 4 wurde Nichts bemerkt.

§ 5.—Ein Bezirksarzt verbrannte sich die Hände mit dem von der Hebamme

bereiteten Carbolöl.—Dieses kommt aber auch bei dem von den Aerzten selbst bereiteten vor.

§ 6 ist bezüglich seiner drei Vorschriften am verschiedensten beurtheilt, namentlich die zweite, die Injection mit 2procentiger Lösung gleich nach der Geburt. Sie wurde gut aufgenommen in Pirna, fand seltenen Widerstand in Meissen und keinen Widerstand in Chemnitz. Im Allgemeinen im Bezirk bei Aermeren (Schwarzenberg, Oschatz Land, Annaberg, Glauchau) anfangs Widerstand, später nicht mehr (Freiberg). Aeltere Hebammen wur en als Hinderniss bezeichnet von Annaberg, Borna, Chemnitz, Grossenhain, Leis nig.

Aerzte als Hinderniss der Injection traten auf in Borna, Schwarzenberg, Dippoldiswalde.

Schmerzen bei der 2procentigen Injection wurden berichtet von Plauen, Dresden Stadt, Chemnitz, Oschatz Stadt.

Ein Intoxicationsfall unmittelbar post partum (Annaberg) günstig verlaufen.

Ein Todesfall durch Injection in die Scheide am achten Tage wurde nach Angabe des Bezirksarztes nicht durch die Carbolsäure bewirkt (Glauchau).

§ 7 wurde gut befolgt (Leipzig Land, Grossenhain, Schwarzenberg, Zittau, Chemnitz, Freiberg) fast wider Erwarten gut (Flöha).

Der Bezirksarzt überzeugte sich persönlich mehrmals von der Art der Erkrankung der als inficirt gemeldeten Wöchnerinnen (Wurzen, Löbau).

§ 8 ebenso wie § 7 nur 1 Mal falsche Anzeige der Todesursache: Schwäche statt Puerperalfieber (Wurzen).

§ 9 soll in Glauchau auf Schwierigkeiten stossen, weil nur eine Hebamme auf 4—5 Quadrat Kilometer kommt, es wurde in einem Falle durchgeführt in Leipzig Stadt und Oschatz Stadt, in einigen in Hainichen und überall ohne Schwierigkeit in Leipzig Land. Nur im Bezirk Plauen machte sich der Mangel der Pflegerinnen in Folge des § 9 in einzelnen Fällen fühlbar. Auch bemerkt der Arzt dieses Bezirkes, dass das Fortbleiben der Hebamme die Wöchnerinnen ängstige und sie auf den Charakter ihrer Erkrankung aufmerksam mache.

Höchst interessant und auch von einem Bezirksarzt bereits als sehr segensreich in drei Fällen erprobt (Flöha) ist nun, das nach § 10 ermöglichte Suspendiren derjenigen Hebammen, welche einen Todesfall an Puerperalfieber gehabt haben, und zwar auf 14 Tage. Dasselbe ist nicht weniger als 37mal schon in dem ersten halben Jahr des Bestehens der Instruction zur Ausführung gekommen, und zwar:

in Bautzen . . .	2mal	in Leipzig Stadt . .	2mal
in Borna . . .	1 „	in Löbau . . .	1 „
in Dresden Land . .	2 „	in Marienberg . .	1 „
in Dresden Stadt . .	4 „	in Zittau . . .	1 „
in Flöha . . .	3 „	in Zwickau . . .	6 „
in Grossenhain . .	3 „	in Annaberg einigemal.	
in Leipzig Land . .	9 „		

Ausserdem wurden bestraft wegen Nichtbeachtung der Instruction:

in Leipzig Stadt . .	1	in Plauen . . .	3
in Leipzig Land . .	3	in Oelsnitz und Zittau je .	1
in Zwikau . . .	3	in Grossenhain mehrere.	
in Schwarzenberg . .	7		

Man ist also ernstlich bemüht gewesen, den Instructions-Vorschriften mit dem nöthigen Nachdruck Geltung zu verschaffen.

Zu § 11 wird von Flöha bemerkt, dass nur einmal versehentlich die flüssige Carbolsäure zu Einreibung auf den Leib statt des Aether's von einer Hebamme bei

einer Blutung benutzt worden sei. Nur von Oschatz Stadt wird mitgetheilt, dass die Carbolsäure bisweilen herauskrystallisire, während die Bezirksärzte von Wurzen, Flöha, und Hainichen entweder bemerken, dass das Präparat keine Gesundheitsschädigung bewirkt, oder sogar, dass es sich in allen Fällen bewährt habe.

Gehen wir nun zweitens zu den von den Bezirksärzten gemachten Verbesserungs-Vorschlägen über, so sind deren nur sehr wenige.

Gewünscht wurde eine Belehrung und Erläuterung zu der Instruction und ein kleineres Druckformat derselben von Oschatz Stadt, eine 1procentige Lösung zu den Injectionen post partum von Dresden Stadt, Oschatz und Plauen, eine Suspension der Hebammen nach § 7 und 8, event. in das Ermessen des Bezirksarztes zu stellen (Wurzen).

Ferner vorgeschlagen als Anfang der Carenzzeit der letzte Besuch bei der erkrankten Wöchnerinn; die Anwendung eines Hohlmasses statt der Theelöffel zur Bereitung der Lösungen, weil letztere zu ungleich seien (Oschatz Stadt), ausserdem die Exclusion der Hebamme schon bei Erkrankung einer ihrer Wöchnerinnen, aber nur auf 8 Tage, dann Wiederversuch der Praxis, und bei neuen Erkrankungen erst längere Exclusion (Plauen). Endlich wurde gewünscht, dass der Besuch gesunder Wöchnerinnen den excludirten Hebammen erlaubt sei.

Drittens Urtheile und Ansichten.

Nur ein Bezirksarzt (Löbau) findet die Instruction nicht nöthig, nicht wirksam, zu theuer, nicht ausführbar und in Bezug auf den Beginn der Exclusion zu unbestimmt. Ein anderer hat Bedenken wegen Gefahr dieses Mittels in der Hand der Hebammen (Oschatz Land), ein dritter findet die Durchführung der §§ 9 und 10 auf dem Lande schwierig, weil die Angehörigen nicht brauchbar zur Pflege seien. Von Plauen und Auerbach wird die Durchführung und Kenntniss der Instruction als noch nicht allseitig erwähnt, dagegen bemerkt der Bezirksarzt von Pirna, dass sie sich beim Publikum mehr und mehr einbürgere; der von Chemnitz, dass der Erfolg ihrer Einführung recht befriedigend sei, der von Oschatz Stadt und Kamenz, dass sie segensreiche Folgen haben werde, und endlich der von Flöha, dass sie bereits solche gehabt habe. Und dass sie solche in Zukunft haben möge, hofft und erwartet auch Ihr Referent, mit dem Wunsche, dass ähnliche Einrichtungen bald auch in anderen Ländern in so ausgedehnter Weise getroffen werden möchten wie in Sachsen, damit es immer mehr gelinge, jenen furchtbaren Feind der Neuentbundenen zu bekämpfen.

Endlich will ich in Betreff der puerperalen Mortilität des vergangenen Jahres noch erwähnen, dass bei 128,350 Entbindungen zum ersten Male die Mortalität nur 465 betragen hat, eine Ziffer welche gegen das Jahr vorher bei fast gleicher Geburtszahl fast 130 Todesfälle weniger aufweist und erheblich niedriger als die puerperale Sterblichkeit des letzten Jahrzehntes ist, ein möglicher Weise zufälliges, immerhin aber sehr erfreuliches Zusammentreffen.

Antisepsis in der Geburtshülfe.

Professor SPIEGELBERG, Breslau.

Nachdem erkannt war, dass die eigentlichen Wochenerkrankungen nur accidentelle Wundkrankheiten sind, musste die grosse Reform, welche die Antiseptik in der Chirurgie hervorrief, sich nun ganz besonders auch bei der Behandlung,

respective der Verhütung des Puerperalfiebers geltend machen. Hätte man auch schon lange erkannt, dass grosse Reinlichkeit viel zu einem gesunden Verlaufe des Puerperiums beitrug, so würde doch praktisch sehr viel damit nicht erreicht.

Die Anschauung, dass die die puerperalen Wunden inficirenden und die verschiedenen Entzündungen des Genitalapparates anregenden Stoffe, wesentlich von Aussen auf die Wunden gebracht werden, beherrschte dann die Geister; der Gedanke, dass jene sich auch, und zwar recht häufig, im Genitalcanale selbst entwickeln können, trat zurück. Darauf hin wurden nun alle zur Verhütung der Erkrankungen empfohlenen Massregeln ergriffen: Reinlichkeit der Hände und Instrumente, Fernbleiben der Geburtshelfer von Kranken, der Studirenden von anatomischen Arbeiten, Verhängung des Interdictes über Wärterinnen und Hebammen, welche kranke Wöchnerinnen zu pflegen haben. Da jedoch die Erfahrung zeigte, dass die Erkrankungs-Häufigkeit der Wöchnerinnen damit doch nur um weniges geringer ward, so ergab sich als natürliche Folge, dass die secundäre Antiseptik, die nach der Geburt zur Beseitigung der Erkrankungen eingeleitete, die intrauterine Spülung und Drainage, wesentlich cultivirt wurde; ja es blieb noch die Meinung weit verbreitet, dass doch nicht alle Puerperalfieber accidentelle Wundkrankheiten seien, sondern dass sie auch noch anders entstehen können, und dass es auch essentielle Puerperalfieber gäbe.

Die hier befindliche Lücke wird nur ausgefüllt, wenn man sich ein volles Verständniss über die Entwickelung der Infection zu verschaffen sucht, und dazu muss man die Geschichte der chirurgischen Antiseptik herbeiziehen. Die ganze Listersche Lehre und deren Praxis beruht auf der durch die Erfahrung jetzt hinreichlich bestätigten Anschauung, dass die Infection durch die Einwirkung der, in der Umgebung der Kranken vorhandenen, Giftkeime auf die frische Wunde zu Stande kommt, und dass es deshalb absolut nothwendig ist, diese Umgebung von jenen Keimen zu säubern, und wo das nicht möglich ist, die Wirksamkeit dieser Keime zu ertödten, so lange die Wunde offen ist; danach aber die Wunde geschlossen zu erhalten. Wendet man diese Regeln auf das Puerperium an, so heisst dass: die strengste Reinlichkeit und Antiseptik in der Zeit, in welcher die puerperalen Wunden entstehen, also während der Geburt, sowohl auf Seiten der Hülfeleistenden, wie der Gebärenden, und dabei Fernhalten der Luft vom Genitaltractus; und da das nicht ganz möglich ist, Desinfection der in jenen eingedrungenen Luft durch fleissigste Carbol-Spülung desselben in der Geburt.

Nach diesen letzteren, Sorge für Schluss des Genitaltractes, Förderung der Involution und absolute Ruhe für denselben, also Vermeidung jeder intravaginalen und intrauterinen Manipulation, welche nicht dringend geboten ist; ist sie das aber, dann muss sie unter streng antiseptischen Cautelen wieder ausgeführt werden.

Die secundäre Antiseptik, das heisst die antiseptische Behandlung ausgebrochener Erkrankung, leistet nicht viel.

Direct helfend ist sie nur bei den Erkrankungs-respective Fäulnissprocessen im Genitaltractus, welche die Innenfläche desselben noch nicht überschritten haben, also so lange die Parenchyme der Organe noch frei sind; sobald aber das nicht der Fall ist, und es ist so in der Regel, ist die Antiseptik nur ein werthvolles Hülfsmittel zur Besserung, aber kein zuverlässliches, da durch Drainage und Spülung, &c., die erkrankten tiefen Heerde ja nicht getroffen, die in den Organismus eingedrungenen Giftkeime weder entfernt noch unschädlich gemacht werden —abgesehen von den Inconvenienzen der praktischen Ausführung der secundären Antiseptik.

DISCUSSION.

Dr. REID, Glasgow: I have for some years taken a special interest in this subject, and desire to describe the practice which I have been led to adopt. First I ought to say that I believe normal labour to be a perfectly aseptic process; that the fœtus and membranes are expelled without the admission of air into the vagina, and that, in the course of the puerperium, any decomposition of the discharges, which takes place at the vulva, does not pass into the vagina or uterine cavity. But the case is quite different in abnormal, and, especially, operative midwifery. Air is then taken not only into the vagina but into the uterine cavity itself. There is then a great probability that decomposition of the fluids will ensue and possibly septicæmia. But antiseptics, to ensure perfect safety, must be used both in normal and abnormal labour. The practitioner or nurse cannot otherwise make sure of not introducing septic matter into the vagina during ordinary examination. I have elsewhere called attention to the fact that the slight feverish attacks from which many women suffer after labour, are really due to septicæmia, and are fertile causes of sterility. My practice is to use a 1 in 20 solution of carbolic acid in oil at every vaginal examination, it matters not when, where, or how often it may be necessary. After operations the uterus and vagina are washed out with a watery solution three times in the twenty-four hours. This method of treatment does not free the practitioner from the duty of using all the ordinary means for ensuring the safety of his patient.

Dr. FANCOURT BARNES, London: In the British Lying-in Hospital all my patients are delivered under the carbolic spray. This not only disinfects the hands of the nurses and midwives round the patient, but prevents the entry of any poisonous germs into the vagina at the moment when it is distended and opened by the exit of the child. Twelve hours after labour every patient has the vagina syringed with a 1 in 40 solution of carbolic acid. A carbolic spray is always playing in the middle of each ward. During the first three or four days after labour each patient has a mixture containing liquid extract of ergot, quinine, dilute phosphoric acid, and tincture of opium. This ensures permanent and complete contraction of the uterus, and the consequent expulsion of any decomposing clots or pieces of membrane which may be present in the uterus. All washings of the genitals are made with a 1 in 80 carbolic solution. Before making examinations the midwives and nurses anoint their fingers with carbolic oil. The British Lying-in Hospital has always been favourably known in regard to its low mortality, but since the introduction of a stricter antiseptic system, there have been no deaths at all from puerperal fever so-called. Before I used the antiseptic precautions it was quite common to find the patient's temperature as high as 103° or 104° F. on the third or fourth day after delivery. Now, however, it is the rule for a patient to go through both her labour and the lying-in without any rise of temperature whatever. With regard to the wards themselves, they are washed with 1 in 80, carbolic solution, disinfected by burning sulphur, and then left empty, with all the windows open, for four or five days previous to the reception of fresh patients.

Prof. MAGGIOLI, Rome: I agree with Prof. Spiegelberg, that in order to prevent puerperal infection it is necessary to free the air from germs, or the open wounds from their contact and influence, by antiseptic measures; but I differ from him with regard to secondary antiseptic treatment. Quinine is an antidote to the malarial poison in the system, and I think this fact opposed to Spiegelberg's view; I see no reason why a remedy for septic, as quinine is for malarial, poison may not be used. Whatever precautions be taken during and after delivery, a certain number of women will suffer

from puerperal infection, and I cannot agree to do nothing for these. I would draw attention to the use of iodoform in such cases, when given internally. Numerous observations would be required in order to establish its value. Iodoform is absorbed readily, and tolerated by the mucous membrane of the stomach and intestines. It is extremely diffusible in the fluids and tissues of the body, for it has been found in most of the secretions and excretions. It, moreover, remains for a considerable time in the blood and tissues. It has been found in the urine four days even after the last dose was taken. When absorbed into the blood and tissues, it breaks up and sets iodine free, which in the nascent state is probably most powerful in its action. The iodine may circulate in the blood dissolved in the fat, or as iodo-albumen, or in both forms. Iodoform, of all the preparations of iodine, contains the largest proportion of iodine, and when given in small doses it causes no trouble. It, is moreover, the best local application to putrid and septic wounds and ulcerations; and if it be a local antiseptic, why should it not act internally in a similar manner on the septic virus? I have given it in four cases, and Professor Moleschott has orally communicated to me two cases in which he had employed it. All six were cases of puerperal septicæmia. In my four cases the iodoform was administered internally, and I think the cure which followed was in great part due to the drug. In Moleschott's case, it was applied to the skin as an ointment. Six cases of course do not suffice to establish the value claimed for iodoform, but these cases are brought forward at this time in the hope of eliciting facts bearing upon the question.

Dr. Graily Hewitt, London: As regards the prevention of introduction of septic matter from without, the rule should be to use the nailbrush assiduously in all midwifery cases. Other precautions, which have already been mentioned, I will not repeat. Very many cases, according to my experience, are cases of septic action arising from within. One class of cases is that arising from too complete rest on the back during the first few days after labour. Septic fluid then collects in the vagina, and readily finds its way into the system if a slight laceration exists. The remedy for this is to insist on a certain degree of motion. Dr. Goodell of Philadelphia has, I think, done good by insisting on the necessity for drainage of the vagina by alteration of position. The next point I will allude to is the drainage of the uterus. In order to secure this the uterus must be kept in a state of good contraction. To produce good contraction it is necessary to pay great attention to maintaining the forces of the patient. Food of good quality should be given after labour, and the general strength thus maintained. It is a maxim with horticulturists, that to preserve a plant from disease and the attacks of insects, it is necessary to maintain it in a rapidly growing state. The nutrition of the body must be maintained at a high standard, and made to move rapidly in order to prevent morbid action. Thus contraction would be insured. In practice I would strongly recommend application of pressure outside as an adjuvant. Carefully applied pressure aids the uterus to contract, and has the effect of aiding the draining of the uterus. I have seen many cases of puerperal fever, so-called, where this pressure has produced manifestly good effects.

Dr. Edis, London: I regret Prof. Spiegelberg is not present, as I controvert some of the statements made in the abstract read. Cleanliness of the hands and instruments, forbidding students engaged in dissecting to attend midwifery, &c., had exercised a very considerable influence in reducing the mortality of the lying-in-wards in Vienna. Assuming that infection resulted from the action of germs floating about, we cannot be too careful in keeping the atmosphere of the lying-in-room as fresh and pure as possible —employing carbolized oil for examination, washing the hands and cleaning the nails

most scrupulously, removing all soiled linen at once, not leaving the patient soaking in her secretions for two or three hours after labour, and insisting generally upon the utmost cleanliness. The habit of keeping the patient lying rigorously on her back for a week or two following parturition is a very injurious one—the lochia accumulate in the posterior cul-de-sac of the vagina, and often prove a most potent source of evil. The patient should be allowed to sit up to pass water morning and evening from the first, so as to expel all coagula or accumulations from the vagina. The syringe with weak carbolic acid (1 in 40) or other disinfectant should be employed to wash out the vagina at least twice daily. All soiled linen should be immediately removed from the room and placed in water with carbolic acid. The windows should be opened daily, and efficient ventilation of the room at night secured. The employment of the carbolic spray is impracticable, as a rule, in private, and in the present unsettled state of the question it is doubtful whether its employment is advisable.

Prof. LAZARÉWITSCH, Kharkhoff: Infection after delivery may be caused by the contact of decomposing matter with the cellular tissue, the blood-vessels or lymphatics when they have been injured. In order to prevent infection, attention must be paid to the uterus and vagina, which may contain blood-clots. Most frequently the uterus is in a state of flexion; then it should be replaced and compressed, so as to discharge its contents. Midwives should be careful to avoid the wounds of the genital mucous membrane. Sponge should not be used, but in its place wadding wet with carbolized water mixed with glycerine.

Prof. TARNIER, Paris: In 1856, when I was interne of the Maternity, there was a mortality of 5 per cent.; this has been reduced to 2 per cent. in the Old Hospital, and to 0·75 per cent. in a pavilion constructed some years ago on my plans. In this pavilion all the rooms are separate from one another, and do not communicate by inside passages; each one has a door and a window which open directly out in such a manner as to make it impossible for a person to go from one to the other without taking what may be called a bath of external air. The corners of these rooms are round, in order to prevent the accumulation of dust; the floor, the walls and the ceiling are imperishable, and can be washed with a stream of water. All the furniture is of iron; the mattress, pillow and bolster, both for mother and child, are of chaff. A fanlight placed in the wall, and looking on the external corridor, allows the nurse to see everything that happens in each ward. When a recently delivered woman is convalescent and leaves the ward, everything is washed—floor, walls, ceiling and furniture. The chaff in the mattress and pillows is burnt; the linen and coverings are sent to the laundress; all is changed, and the following day a new patient coming into the ward to be confined would only observe objects thoroughly cleaned. The women rest until they are quite well in the bed in which they were delivered. In order to protect the mattress from being soiled, I formerly used a sheet of caoutchouc, which was washed with carbolized water, but these washings were not always as perfect as I wished, and for several years I used simply tarred paper, which is very cheap and is burnt with the chaff. As soon as possible after her arrival, the patient is placed in a bath or washed; the physician, house-physician, midwife and nurse, wash their hands in an antiseptic fluid. But what fluid should be preferred? To satisfy myself on this subject I made the following experiments. In several glasses I placed water; in others a solution of boric acid; in others a solution of corrosive sublimate $\frac{1}{1000}$ (Van Swieten's solution): in all these glasses I placed a fragment of placenta and allowed them to stand at the mean temperature. In a few days all the glasses except two contained innumerable living microbs; the two glasses which did not contain living microbs were those

which contained the solution of corrosive sublimate, and saturated solution of boric acid (40 grammes to the litre) respectively. I have myself the greatest confidence in bichloride of mercury as a parasiticide. Whenever I suspect my hands—be it in hospital or in private—I wash them in Van Swieten's solution; my internes do the same, and we have never suffered in the least from the mercury. I believe this solution to be good for washing the hands and instruments, but I have never used it for the patient. The sounds which I use are of red caoutchouc, and before using they are plunged into carbolized water. This is an indispensable precaution, for I have seen two deaths from nephritis in consequence of the use of an improper catheter. The tampons and charpie used for arresting hæmorrhage in cases of placenta prævia soak continually in water until withdrawn for use. For vaginal examination we use carbolized vaseline. To prevent germs floating in the air from falling on the wounds made by the passage of the head, the carbolized spray has been advised. I object to this procedure. When it is employed for several hours the women become wet and cold. I have adopted a different procedure in my wards. When the head emerges, a midwife holds in her hands a brush plunged into carbolized oil (1 in 10), and each time that the vulva gapes during the contractions of the uterus she mops the fœtal head. When the head retires after a pain, it carries with it the carbolized oil into the genital canal, and this protects the soft parts of the urethra. In washing the patient, carbolized water and cotton are used. I have for a long time discarded sponges. The cotton should be placed in carbolized water some time before use, and left to soak without manipulation; the cotton then preserves an elasticity which makes it retain the water, while if it be squeezed it imbibes the water rapidly; but the result, as Prof. Guyon has shown, is an inelastic mass. For the toilette, on one of the floors I employ carbolic acid solution (1 in 40), on the other a saturated solution of boric acid. The results have been almost the same. I have never made vaginal or uterine injections when the patient has been doing well. I have had recourse to them in pathological conditions only. Even when the lochia were most offensive, I have sometimes obtained extremely satisfactory results from them. I have tried the antiseptic method in dressing the umbilical cord; the cord dies then rapidly, and gives off no bad odour, but it falls off very slowly in 10 or 15 days. This disturbs the women and retards their departure from the hospital, and I have consequently given up the treatment.

Dr. H. O. Marcy, Boston: The three great factors which have in a large degree revolutionized modern surgery are, asepsis, antisepsis, and drainage. I rise only to give my experience in the latter, although I have for years recognized the importance of the two first, and in instrumental cases of delivery have used all antiseptic precautions. I have devised the double rubber tubes which I here exhibit, and in such septic cases these are left within the uterine cavity, and serve for drainage, while the nurse can easily irrigate the endometrium as may be directed.

Dr. J. N. Quimby, Jersey City, N. J.: I agree with Dr. Edis that some members of the profession show a tendency to carry this antiseptic method of treatment to an extreme, and that the idea of using it in ordinary cases of confinement seems somewhat absurd. As far as cleansing the patient thoroughly, and having the clothing and the person washed with soap and water, or perhaps a solution of carbolic acid in water, is well enough, and in hospital cases absolutely necessary. If a medical attendant has been engaged with any case of contagious disease, or making any dissections or post-mortem examinations, he should see that he is thoroughly disinfected himself with solution of carbolic acid, so that he should carry no infectious matter on his hands, nails, or clothing. I wish to offer a few remarks in criticism in reference to a remark

which Dr. Edis has just made—viz., that "he directed his patient, from the first day of confinement, to sit up to urinate, that the vagina might be emptied of any clot or secretion that might be lodged therein." I was sorry to hear Dr. Edis make this remark, as I consider it a great mistake, as, in my experience, some of the worst cases of metro-peritonitis or puerperal fever that I have ever seen have arisen from the very fact that these patients, contrary to order, had sat up in bed to urinate, on the first or second day, which, I think, was the cause of bringing on the attack which came very near proving fatal. The assertion that Dr. Edis made, that an erect position is necessary to get rid of an accumulation which might perchance be a source of danger or a nidus of septic infection, is groundless, as I cannot accept the idea, that healthy secretions from a normal uterus in an ordinary case of confinement could be the means of infection, if they remained a short period in the vagina.

Note on the Statistics of Childbirth in the Lying-in Wards of the Workhouse Infirmaries of England and Wales for ten years, 1871–80.

Dr. F. J. Mouat, London.

There are few questions of greater interest and importance in relation to the poor of all countries, than the provision of proper professional attendance during the parturition of the women of that great class. The risk of carrying them safely through their confinements, in public institutions, has been so great, as to have led eminent authorities of nearly every nationality to doubt the prudence and propriety of treating them in maternities, or in the lying-in wards of general hospitals, where such exist.

The International Medical Congress held in Brussels in 1875, recommended the complete abandonment of great maternities, or lying-in hospitals, and the substitution of small *maisons d'accouchement*, with separate rooms.

I have no intention of raising a discussion on so vital a question, but I wish to submit to this great international gathering some authentic figures bearing on the subject, collected by authority, from the records of the department of the Local Government Board to which I am attached.

These figures cover a sufficient length of time—a decade—and are themselves of adequate magnitude to throw considerable light upon the question, in any future discussion that may be entered upon. My recent inquiries into the hospital question generally, have satisfied me that this discussion should not be long deferred. It cannot be in better hands than in those of the distinguished obstetricians of all nations assembled at this Congress, and to their careful consideration I submit a brief analysis of the figures referred to.

In a parliamentary paper published in 1867,* it was stated that one very remarkable fact was disclosed in the returns to an order of the House of Lords, as to whether recovery from illness was retarded by the existing accommodation and arrangements of workhouses. This was the infrequency of child-bed fever (metria) and the small rate of mortality produced by it in those houses. Figures, regarding 12,000 deliveries in the metropolitan workhouses, for five years, were produced in support of this contention.

With a view to ascertain whether this immunity continued, and was shared by all

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* Report of the Committee appointed to consider the cubic spaces of Metropolitan Workhouses. Presented to both Houses of Parliament, 1867.

England and Wales outside the metropolis, a return of the births, and of deaths in child-birth in the whole of the workhouses under the authority of the Local Government Board during the 10 years 1871-80, was called for in May last. The return has just been completed, and I am permitted to submit an abstract of the results of the inquiry to the Congress.

Thirty workhouses in the metropolis, and six hundred and fourteen in the rest of England and Wales, have sent in statements carefully compiled from their records. They convey information respecting 87,726 deliveries: 23,117 in the metropolis, and 64,609 in the provinces. In the institutions within the registration area of London there were 204 deaths, 0·88 per cent., or 1 in 113. In the provinces the deaths numbered 561, 0·86 per cent., or 1 in 115.

Of the deliveries enumerated above, in the provinces 17,830 were *primiparæ*, and in the metropolis 5,990, in all 23,820 first confinements.

The unmarried mothers were 49,807 out of London, and 16,288 within the metropolitan area, making a total of 66,095, or nearly three-fourths of the entire number delivered. The absolute accuracy of this part of the returns is doubtful, as there has not been time to verify it.

There were few entries of still-born infants, 162 in all. The records are probably not quite exact in this particular also. The twin births were 949, the triplets 3 in number.

In 373 of the provincial workhouses, and 5 of those in the metropolis, representing 25,198 accouchments, there was not a single death in the decade. In 136 others, 2 of which were in the metropolis, there was 1 death in each, to 13,310 deliveries in all. The remainder of the deaths ranged from 2 to 39 in the whole of the rest of the workhouses.

The following workhouse infirmaries had the largest death-rate:—

Workhouses.	Deliveries.	Deaths.	Workhouses.	Deliveries.	Deaths.
Croydon	634	11	Islington	1,008	13
St. Olave's, Southwark	582	11	Lambeth	1,807	14
Bradford	592	12	Whitechapel	857	16
Bristol	790	12	St. Marylebone	2,267	18
Sheffield	612	12	St. Pancras	2,286	26
St. Giles', Bloomsbury	1,075	12	Liverpool	3,148	31
Kensington	1,162	12	Manchester	2,265	39

The causes of death were:—

	Provinces.		Metropolis.	
	Deaths.	Ratio per Cent.	Deaths.	Ratio per Cent.
Metria	116	0·180	30	0·1
Post-partum hæmorrhage	49	0·076	23	0·1
Puerperal convulsions	81	0·125	13	0·1
,, peritonitis	102	0·158	43	0·2
All other causes	213	0·330	95	0·4

Note.—The death-rate is calculated on the whole number of deliveries.

The great majority of deaths from *all other causes* were in reality incidents or accidents of the confinements, and in most cases not necessarily connected with parturition, such as syphilis, pneumonia, phthisis, small-pox, scarlet fever, heart disease, &c. The returns under this head are not, however, complete, and do not cover the whole ground.

The deaths from metria, and the other purely puerperal affections, were for the most

part in poor women, admitted in a state of exhaustion from starvation and misery, the victims in many cases of drink and destitution, syphilis, and the anæmic condition found in the lowest stratum of the paupers of the lanes and courts of our great centres of industry. Not a few of the deliveries among the unmarried were doubtless the outcome of seduction, and the depressing conditions associated with shame, and its serious consequences.

The mortality from metria in all but a very few of the workhouses was in solitary cases, and spread in the wards to which they found admission in only one or two instances. Whenever it occurred the case was, as a rule, isolated as soon as it was diagnosed, and the ward was disinfected, and disused for a sufficient length of time to prevent a renewal of the disease in new admissions, when reopened.

It is a matter of some interest to know how far the above figures compare with the returns of the Registrar-General for the whole population, rich and poor, the vast majority of whom are delivered in their own homes. The Poor Law returns show the proportion of paupers to the whole population—viz., 34·6 per 1,000 in the decade. The former alone are admitted to the poor-houses.

In the forty-second report of the Registrar-General of England, presented to Parliament in April last, in Table 44, p. xxiii. is an abstract of the deaths of women in England referred to child-birth, in each of the 33 years from 1847–79.

From this it appears that there were in all 23,953,400 births of children born alive, and 116,648 deaths from metria and child-birth, or in the proportion of 0·5 per cent. From metria alone the recorded deaths were 42,647, or in the ratio of 0·2 per cent. From what in the return referred to are termed accidents of child-birth, the deaths were 74,001, or at the rate of 0·3 per cent. The whole mortality combined during the lengthened period in question, was 4·9 per 1,000.

Multiple births and the still-born are not specially accounted for in this return.

It will thus be seen that the death-rate from metria alone was less in the workhouse infirmaries than in the entire population, and the whole record appears to me to establish the fact that a pauper woman delivered in a workhouse lying-in ward, has as fair a chance of life as a poor woman confined in her home. Indeed, if the Registrar-General's returns could be classified with sufficient minuteness to show exactly in what classes the deaths in parturition really occurred, and at what ages, the proportion of *primiparæ*, and other collateral conditions necessary to a rigorous mathematical deduction, I have every reason to believe, that the result would not be unfavourable to the Poor Law Department.

Eine Hypothese über puerperale Eclampsie.

Professor HALBERTSMA, Utrecht.

Im Jahre 1871* habe ich die Hypothese aufgestellt, dass die meisten Fälle von puerperaler Eclampsie die Folge sind von Retention der Harnbestandtheile in Folge von Druck auf die Ureteren durch die stark ausgedehnte Gebärmutter.

Meine Hypothese ist u. a. bestritten von Schröder und Kleinwächter. Ihre Autorität scheint mir grösser, als die Kraft Ihrer Argumente. Was von Ihnen gegen meine Hypothese angeführt wird, spricht gerade dafür. Schröder hat meine Hypothese nicht angenommen, weil nach ihm die entzündlichen Nierenaffectionen hierdurch nicht erklärt werden. Doch ist Harnretention durch Druck

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* *Centralb. für die medicin. Wissensch.* No. 27.

auf die Ureteren das einzige bekannte Moment, welches die Entstehung von entzündlichen Nierenaffectionen in der Schwangerschaft erklären kann. Auch das Hauptargument von Kleinwächter spricht nicht gegen, sondern für meine Hypothese. Wäre meine Hypothese haltbar, dann müsste nach Kleinwächter Eclampsie bei Ovarialtumoren viel öfter beobachtet werden. Allein ist nicht eher das Nichtvorkommen von Eclampsie bei den grössten acut entstehenden Ovarialtumoren das beste Zeugniss, dass die Eclampsie nicht von einem allgemein vermehrten Druck in der Bauchhöhle herrühren kann, sondern anders erklärt werden muss. Bei meiner Hypothese wird das Nichtvorkommen von Eclampsie bei Ovarialtumoren hierdurch erklärt, dass diese nicht zwischen den Ureteren gelegen sind wie der Uterus gravidus. Je mehr dieser ausgedehnt ist, um so länger ist der Weg, den die Ureteren abzulegen haben von den Nieren zur Blase, um so grösser muss der Druck und die Ausdehnung der Ureteren sein. Sie kommen hierdurch in den Zustand eines gebogenen, gedehnten Rohr's. Bei einem stark ausgedehnten Uterus werden insbesondere die Ureteren gedrückt und gedehnt, bei Ovarialtumoren werden die Ureteren nicht mehr gedrückt, als die anderen Organe des Bauches.

Durch eine nähere Auseinandersetzung meiner Hypothese will ich versuchen meine Gegner zu meinen Anhängern zu machen. Meine Hypothese ist gegründet auf das anatomische Verhalten der Ureteren zum Uterus, auf Experimente und klinische Beobachtung.

Die Ureteren, die von hinten und oben nach vorn und unten verlaufen, umgeben den Uterus und den oberen Theil der Vagina. Man darf weiter mit Sicherheit annehmen, dass schon eine geringe, dauernde Compression des Uterus für eine vollständige Retention des Harns ausreicht, wie ich im Jahre 1871 im *Centralbl.* (a. a. O.) schon betont habe.

Was lehrt nun die klinische Beobachtung? Dass eine der meist vorkommenden Erscheinungen die spärliche Urinlösung ist.

Unter welchen Umständen wird Eclampsie am meisten beobachtet?

1.—Bei stark ausgedehntem Uterus, wie bei Zwillingschwangerschaft.

2.—Kommt Eclampsie viel öfter vor bei Primiparæ, wo alle Theile mehr gespannt sind, als bei Multiparæ und insbesondere, wie es scheint, bei alten Primiparæ, wo die Theile am wenigsten dehnbar sind.

Ausser durch starke Spannung und Dehnung der Ureteren kann Retentio Urinæ auch noch durch ein anderes Moment in der Schwangerschaft und insbesondere während der Geburt entstehen. Die Ureteren können zwischen den vorliegenden Theil und die Beckenwand gedrückt werden, ein Druck, der unter anderem bewiesen wird durch das Entstehen von Fistulæ uretero-uterinæ et vaginales. Wenn der Kopf lange im Beckeneingang stehen bleibt wird es nur von der Lage des Kopfes abhängig sein, ob dieser Druck stattfindet oder nicht.

Spricht das Vorkommen von Eclampsie im Wochenbette gegen meine Hypothese? Ich glaube nicht. Es ist deutlich, dass die Folgen Urinretention die sich während der Geburt entwickelt hat, erst in den ersten Stunden nach der Geburt zum Ausbruch kommen können. Ausserdem ist es ein Factum, dass die gestörte Urinlösung gewöhnlich direkt nach der Geburt noch nicht aufgehört hat. Ist es nun zu gewagt anzunehmen, dass die Muskeln der Ureteren, die so lange gedehnt und gedrückt wurden, in den ersten Stunden nach der Geburt noch nicht zur Norm zurückgekehrt, noch gelähmt sind, und dass hierin die Retention des Urins in den ersten Stunden nach der Geburt seine Erklärung findet? Wenn man sich dann weiter dieses merkwürdigen Factums erinnert, dass Eclampsie insbeson-

dere während der ersten Stunden nach den Geburt vorkommt, nach dem zweiten Tag post partum eine grosse Ausnahme ist, und dann auch anders erklärt werden kann (z. B. durch Druck eines parametritischen Exsudats), dann scheint auch hierdurch meine Hypothese bestätigt zu werden.

Ausser der Urinretention in Folge der Ureterendehnung und des Drucks, verdient noch eine andere Sache, nämlich die Contraction der Gebärmutter, unsere Beachtung. Dieses muss von Einfluss sein auf die Erhöhung des Secretionsdruckes in den Nieren, insofern bei der Contraction die Uteruswände jedesmal anämisch werden und im übrigen Gefässsystem und zunächst im Bezirk der Niere collaterale Hyperämie entstehen muss. Dies muss zu erhöhter Secretion und bei behindertem Abfluss zu übermässiger Spannung im Nierenbecken und oberen Theil des Ureters führen, wovon secundär die Folge Rückwärtstranssudation von Urinbestandtheilen in's Blut, resp. in die Lymphgefässe sein würde.

Diese plötzliche Dehnung und Spannung des Nierenbeckens kann vielleicht erklären, warum insbesondere die Uræmia puerperalis von Convulsionen begleitet ist, und zugleich warum die Anfälle sich gewöhnlich einer Wehe anschliessen.

Jetzt sind auch verschiedene Beobachtungen da, wobei diese Urinretention in den Ureteren durch Autopsie constatirt wurde.* Er fand in 8 von 32 Sectionen nach Eclampsia puerperalis wichtige Anomalien in den Ureteren, gewöhnlich bestehend in sehr starker Ausdehnung oberhalb des kleinen Beckens statt.

Zieht man nun in Betracht, dass mit der Geburt die Ursache der Retentio Urinæ gewöhnlich aufhört und es desshalb öfters sehr schwer sein wird Aenderungen an den Ureteren zu sehen, wenn die Frauen einige Zeit nach der Geburt gestorben sind, dann kann die Anzahl von constatirten Fällen schon gross genannt werden.

Mir selber steht nur ein Fall zur Verfügung, wo bei der Section die Ureteren untersucht wurden.

Es betraf eine Primipara, die am Ende ihrer Schwangerschaft von Convulsionen befallen wurde. Das Becken hatte eine Conjugata vera von 9 Cm., die übrigen Dimensionen waren normal. Der vorliegende Kopf lag auf dem Beckeneingang. Die Urinlösung war sparsam, der Urin dunkel gefärbt, enthielt viel Eiweiss, feste, degenerirte Cylinder, einzelne rothe und weisse Blutkörperchen. 24 Stunden nach der Geburt wurde eine grosse Quantität Urin entleert. Die Frau starb 52 Stunden nach der Geburt. Bei der Section wurde Katarrh und belangreiche Verdickung der Ureteren gefunden und ein leichter Grad von parenchimatöser Nephritis.

Wir resumiren. Eine der Haupterscheinungen bei Eclampsia ist verminderte Urinlösung oder Retentio urinæ. Wie ist dieses zu erklären? Die Blase zeigt keine Abnormitäten, die Nieren sind oft gesund oder nicht genug geändert, um diese Erscheinung zu erklären. Schon, per exclusionem, muss man an eine Anomalie der Ureteren denken. Allein sie müssen auch nothwendig in einigen Fällen verengt oder geschlossen werden und in ihren peristaltischen Bewegungen gehemmt. Und gerade in diesen Fällen, wie bei Zwillingsschwangerschaft, bei Fällen von geringer Dehnbarkeit der Ureteren, wie bei Primiparæ und insbesondere bei älterer Primiparæ, kommt Eclampsie am meisten vor. Weiter ist jetzt schon in verschiedenen Fällen der gehinderte Abfluss des Urins durch die Ureteren durch die Autopsie direkt bewiesen. Endlich wird durch meine Hypothese am besten erklärt, warum die sogenannten urämischen Erscheinungen in beiweitem den meisten

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* Cf. Lohlein, *Zeitschrift f. Geb. u. Gynäkol.*, 1879.

Fällen am Ende der Schwangerschaft, während der Geburt und in den ersten Stunden danach auftreten, immer dann, wenn die Ausdehnung des Uterus den höchsten Grad erreicht hat oder ihre Folgen, wie Ureterenlähmung, noch wirken können.

Darum meine ich, dass meine Erklärung vom Entstehen von den meisten Fällen von Eclampsia puerperalis—verhinderter Abfluss des Urins in Folge von Druck auf, und Biegung und Dehnung der Ureteren—bis jetzt den meisten Grund hat.

Zur Frage, ob diese Urinretention auch durch chemische Umsetzungsprodukte oder durch Aenderung der Circulationsverhältnisse im Gehirn wirkt, kann ich jetzt nichts neues beibringen und muss darüber selbst mit mehr Reserve sprechen als in 1871. Der Ausbruch der Convulsionen findet in vielen Fällen jedenfalls eine Erklärung in dem Reiz in Folge der Dehnung des Nierenbeckens.

Wird meine Hypothese bestätigt, dann sind wir doch der Lösung des Räthsels der Eclampsie eben so nahe gerückt, als dem Räthsel der Wundkrankheiten durch Entdeckung der Bakterien.

Die Aetiologie der Eclampsie und ihr Auftreten in Folge von Schwangerschaft, Geburt und in den ersten Stunden danach, wäre aufgeklärt.

Parallèle de l'Embryotomie et de l'Opération Césarienne.

Dr. G. Eustache, Lille.

(*a.*)—Le parallèle entre l'embryotomie et l'opération césarienne a été fait depuis longtemps. Cette dernière opération paraissait devoir être entièrement condamnée, et de fait elle est rejetée par la plupart des professeurs d'accouchements ; elle est également rejetée par presque tous les livres classiques d'obstétrique, tant en Europe qu'en Amérique.

L'enseignement officiel des facultés et des colléges de médecine est à peu près unanime sur ce point à savoir que : lorsqu'un accouchement est impossible spontanément ou par le forceps, c'est aux divers procédés d'embryotomie (crâniotomie, céphalotripsie, embryotomie proprement dite, que l'on doit avoir recours de préférence à l'opération césarienne ; celle-ci n'est considérée que comme un moyen extrême qu'il ne faut mettre en usage que lorsque tous les autres ont échoué ou sont franchement impossibles.

Cette solution n'a jamais été sérieusement établie sur la statistique ; la question religieuse s'en est mêlée et n'a pas peu contribué à passionner et à égarer le débat, en y faisant intervenir des raisons extra-médicales qui, si respectables et si impérieuses qu'elles soient, n'en doivent pas moins être écartées par le vrai praticien.

(*b.*)—Il est démontré aujourd'hui que l'embryotomie, en sacrifiant toujours l'enfant expose la mère aux plus grands dangers.

Il est et il sera toujours absolument impossible de connaître le degré réel de mortalité de cette intervention chirurgicale. Les statistiques établies à ce sujet sont trop peu nombreuses et surtout trop incomplètes pour permettre de baser sur elles une opinion sérieusement motivée.

Aucun de ceux qui ont pratiqué cette opération, surtout avec un rétrécissement de 6 centimètres et au-dessous, n'hésitera à croire avec moi qu'elle doit être et qu'elle est la plus souvent mortelle, malgré les quelques succès obtenus, et autur desquels on a fait beaucoup de bruit.

Je crois ne pas dépasser les limites des probabilités, en portant la mortalité de l'embryotomie à 50 pour 100.

(c.)—L'opération césarienne, qui sauve un très grand nombre d'enfants, donne pour la mère un résultat égal, bien supérieur à cette moyenne de 50 pour 100, pourvu qu'elle soit faite en temps opportun. Au double titre, elle mérite incontestablement la préférence sur l'embryotomie.

Les statistiques récentes, celle du Dr. Harris, de Philadelphie en particulier, justifient cette conclusion qui s'écarte entièrement de ce qui est officiellement enseigné ou écrit. Voir également les tables du Dr. Th. Radford, de Manchester.

Et pourtant, en présence des résultats de la chirurgie abdominale moderne, cela n'a pas lieu de nous surprendre.

L'ovariotomie donne des résultats bien supérieurs, puisque la moyenne des succès de ces dernières années n'est pas moindre de 80 pour 100.

L'opération de Porro, autrement grave à mon avis que l'opération césarienne simple a fourni 2 pour 100 de succès.

Grâce à la méthode antiseptique, le pronostic de toutes les opérations chirurgicales a changé du tout au tout. Par exemple, les dangers de l'infection puerpérale peuvent être entièrement conjurés.

Il m'est permis de dire qu'il doit en être de même pour l'opération césarienne—les succès récemment publiés en font foi.

Mais si l'on veut faire bénéficier l'obstétrique de la remise en honneur de l'opération césarienne, il faudra bien se garder de l'entreprendre dans des conditions défavorables de lieu et surtout de temps, c'est-à-dire ne pas attendre que la femme soit épuisée par un travail long et laborieux ; il faudra l'entreprendre de bonne heure.

Ces considérations que je ne fais qu'indiquer m'ont engagé à soumettre cette question aux membres éminents du Congrès. La plupart des illustrations obstétricales du monde entier se trouvent ici. Jamais une meilleure circonstance ne se présentera pour discuter ce point capital de la pratique médicale, qui a été trop hâtivement jugé, je crois, et qui demande une révision sérieuse.

Je fais donc appel aux accoucheurs de tous les pays, qu'ils apportent dans cette discussion le fruit de leur expérience et de leurs réflexions ; la question en vaut la peine, car il ne s'agit de rien moins que d'épargner la vie de plusieurs centaines d'enfants qui sont sacrifiés tous les ans, et d'exposer les mères à des dangers de mort réellement moindres.

Je leur demande donc d'examiner et de discuter les conclusions suivantes, que je crois devoir formuler comme la règle future du praticien.

Conclusions :—

D'une part, considérant :—1. Les résultats récents de l'ovariotomie et de toutes les autres opérations de gastrotomie.

2.—L'amélioration du pronostic de tous les traumatismes chirurgicaux sous l'influence de l'application méthodique des pansements antiseptiques.

3.—Les succès de l'opération de Porro.

4.—Les succès nombreux publiés dans ces dernières années à la suite de l'opération césarienne simple, autant pour la mère que pour l'enfant.

Et d'autre part, considérant que l'embryotomie en sacrifiant toujours l'enfant, expose la mère à des dangers au moins aussi grands que l'opération césarienne ;

Qu'elle est inapplicable dans beaucoup de cas de vices de conformation du bassin, par, exemple avec un rétrécissement de 5 centimètres et au-dessous.

Je conclus que :—

1.—Quand l'enfant est vivant au commencement du travail, et que le rétrécissement du bassin est au-dessous de 78 millimètres, limite extrême de l'application du forceps l'opération césarienne sera pratiquée de bonne heure, c'est-à-dire dès que le travail sera franchement établi, et d'après toutes les règles de la méthode antiseptique.

2.—Si l'enfant est mort et que le rétrécissement soit supérieur à 5 centimètres, on recourra à l'embryotomie. Au-dessous de 5 centimètres, l'opération césarienne devient une opération de nécessité.

En résumé, l'opération césarienne sera la méthode de choix, et l'embryotomie la méthode d'exception.

DISCUSSION.

Dr. LEOPOLD MEYER, Copenhagen: I only intend to occupy your attention for a few moments, but my ideas differ so totally from those put forward by the author of this paper, that I feel obliged to say a few words about the question. First with regard to the dangers of embryotomy, I am sure it is a great exaggeration to put the death-rate of this operation at 50 per cent. The danger is much less, especially when we use that valuable instrument the cranioklast, modified as it has been to serve for the cephalotribe in most cases, when we strictly carry out all antiseptic precautions and when we do not regard the most difficult cases only; the statistics of later years prove this sufficiently. Secondly, with respect to the dangers of Cæsarean section, nobody can yet tell for certain which operation is most dangerous: Cæsarean section or Porro's operation. But this last, among which we have very exact statistics, has a death-rate of about 50 per cent. When Prof. Eustache has saved both women on whom he has performed Cæsarean section we can only congratulate him on such a rare success. The author says: "Je conclus que:—1. Quand l'enfant est vivant au commencement du travail, et que le rétrécissement du bassin est au-dessous de 78 millimètres, limite extrême de l'application du forceps, l'opération césarienne sera pratiquée de bonne heure . . . . &c." I contest this conclusion, and that on account of the possibility of premature labour, in a future pregnancy, when the antero-posterior diameter measures from 68 to 78 millimètres. It seems as if the author had quite forgotten that such an operation exists. As the higher degrees of distortion of the pelvis are much rarer than the slighter degrees, the greatest number of pelves with an antero-posterior diameter less than 78 millimètres will measure about 75 millimètres in the conjugate, very few less than 70 millimètres, and in these cases we have all reason to expect a favourable result from the induction of premature labour. My plan is in these cases to perform embryotomy—an operation very much less dangerous than Cæsarean section—especially in cases with an antero-posterior diameter of over 70 millimètres, and thus to give the women the chance of having one, or may be many children born alive by the induction of premature labour. It is impossible to treat the question before us when we disregard this valuable operation, and I believe that it is on account of this neglect that the author has come to this—in my opinion—dangerous conclusion.

STATE MEDICINE.

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INAUGURAL ADDRESS,

BY THE PRESIDENT,

JOHN SIMON, C.B., D.C.L., LL.D., F.R.S.

In considering what should be my first words in the position which I r to occupy, I fortunately recollect a word of ancient wisdom: "What y who cometh after a king?" Already to-day the Chrysostoms of our)f the Medical Profession have, with their usual felicity, expressed, on is, the feelings which all must equally entertain on occasion of so great a

meeting of our professional brotherhood. I therefore need not, on behalf of the officers of this Section, say more than one word of cordial friendship and congratulation and welcome to those whom we have the pleasure and honour of seeing here: whether they come from within the limits of this United Kingdom of Great Britain and Ireland, or be our fellow-labourers from other lands. If I may add a word specially for myself, I would beg to say how peculiarly gratified I am to know that there will sit on the benches of this Section many of those workers and masters of our craft from whom it has long been my own daily privilege to learn.

My colleagues in the provisional service of this Section, and persons of authority in the general management of the Congress, have pressed on me their wish that I should occupy part of this half-holiday afternoon by addressing to you some general talk in relation to the subject-matter of our Section. I may confess myself to have believed, till almost the last moment, that my best presidential contribution to our meetings would be to sit as a respectful listener; but, on the other hand, I should so much regret to appear merely indolent in regard of what has been desired of me, that, on that ground, though reluctantly, and with unaffected apologies for what I fear may prove a tedious occupation of your time, I defer to the wishes which have been expressed.

Assuming that the members of the Section would not expect anything new in what I might say, I have speculated whether, perhaps, in directions which are not new, I might say anything which would suggest useful thought to outsiders interested in our proceedings. I have remembered that, in our Section of the Congress, we may peculiarly take for granted that the general public overhears what we say. Many of our matters of debate are matters of immense public concern; and the arguments relating to them are in many parts such as the laity can follow equally with ourselves. It has struck me that, as your President on this occasion, I may perhaps usefully have regard to that consideration. Chiefly, then, in that point of view, and with intentions which I hope may explain themselves as I go on, I shall offer you some remarks on certain of the scientific relations of State Medicine. I trust that the thoughts and convictions which I shall express will be fully as much yours as my own, and will thus be far more deserving of public attention than if I pretended to call them original; and, indeed, as regards one practical purpose which I have in view, there is nothing I would so much desire as that I may be deemed merely your mouthpiece or interpreter.

First: a few words on what may be called the general theory of our subject-matter. The term "State Medicine" corresponds to the supposition that, in certain cases, the Body-Politic will concern itself with the health-interests of the people—will act, or command, or deliberate, or inquire, with a view to the cure or the prevention of disease. Before any such supposition can be effectively realized, the Science of Medicine—that is to say, the *exact knowledge* of means by which disease may be prevented or cured—must have reached a certain stage of development; and, unless the science be supposed common to all persons in the State, the existence of State Medicine supposes a special class of persons whom the unskilled general public can identify as presumably possessing the required knowledge. Thus, given the class of experts to supply the required exact knowledge, the Body-Politic undertakes that, within the limits of its own constitutional analogies, it will make the knowledge useful to the community.

I have intimated that in State Medicine (just as in private medicine) the medical function may be exercised either in *curing* or in *preventing* disease; but practically these two departments of State Medicine are not of equal magnitude, nor are dealt with in quite the same spirit.

As regards Curative Medicine, modern governments have in general found it needless to interfere in much detail in favour of persons who require medical treatment. It is true that for cases which may be deemed exceptional—the case of the destitute poor, and the case of districts under grave pestilential visitation, modern governments seem universally to acknowledge that it is their duty to provide, at the public charge, such medical skill and such appliances as may be needed; but, apart from those exceptions, the only action which the State takes in regard of curative medicine is that which consists in its authentication of a skilled medical profession, and in the distinction which it sees fit to draw, as regards rights and privileges, between those who have been duly declared competent to treat disease, and those who have not been so declared.

Larger and far more various than the action taken by the State with reference to the cure of disease, is that which it takes in regard of prevention; and it is particularly of Preventive State Medicine that I propose to speak. In its legal aspect, it is represented by a considerable mass of statutes (nearly all of them enacted within the last thirty-five years) and by an army of administrative authorities and officers appointed to give effect to those enactments. I need not describe in detail the laws and administrative machinery to which I refer, but I may remind you of the largeness and variety of their scope, even by quoting only the terms in which I was able, twelve years ago, to speak of the public health law of England: "It would, I think, be difficult to over-estimate, in one most important point of view, the progress which, during the last few years, has been made in sanitary legislation. The principles now affirmed in our statute-book are such as, if carried into full effect, would soon reduce to quite an insignificant amount our present very large proportions of preventable disease. It is the almost completely expressed intention of our law that all such states of property, and all such modes of personal action or inaction, as may be of danger to the public health, should be brought within scope of summary procedure and prevention. Large powers have been given to local authorities, and obligation expressly imposed on them, as regards their respective districts, to suppress all kinds of nuisance, and to provide all such works and establishments as the public health primarily requires; while auxiliary powers have been given, for more or less optional exercise, in matters deemed of less than primary importance to health; as for baths and wash-houses, common lodging-houses, labourers' lodging-houses, recreation-grounds, disinfection-places, hospitals, dead-houses, burial-grounds, &c. And in the interests of health, the State has not only, as above, limited the freedom of persons and property in certain common respects; it has also intervened in many special relations. It has interfered between parent and child, not only in imposing limitation on industrial uses of children, but also to the extent of requiring that children shall not be left unvaccinated. It has interfered between employer and employed, to the extent of insisting, in the interest of the latter, that certain sanitary claims shall be fulfilled in all places of industrial occupation. It has interfered between vendor and purchaser; has put restrictions on the sale and purchase of poisons; has prohibited in certain cases certain commercial supplies of water; and has made it a public offence to sell adulterated food or drink or medicine, or to offer for sale any meat unfit for human food. . . . . Its care for the treatment of disease has not been unconditionally limited to treating at the public expense such sickness as may accompany destitution; it has provided that, in any sort of epidemic emergency, organized medical assistance, not peculiarly for paupers, may be required of local authorities; and, in the same spirit, it requires that vaccination at the public cost shall be given gratuitously to every claimant. The above survey might easily be extended by referring to statutes which are only of partial or indirect or subordinate interest to human health; but, such as it is, it shows beyond question that the Legislature

regards the health of the people as an interest not less national than personal, and has intended to guard it with all practicable securities against trespasses, casualties, neglects, and frauds."* At the time when that description was written, I unfortunately had to confess that the intentions of the Legislature were not carried into effect; for that the then existing laws (especially in respect of the local authorities which should give effect to them) were in a state of almost chaotic confusion and unworkability; but, since that time, an entirely new constitution of local authorities has been made, some thousands of additional officers have been appointed, and the general fabric of the law has been consolidated, and its powers in some respects extended and made more stringent, with a view to the better prevention of disease so far as legal powers and facilities can attain that object.

Such being the very large contribution which the Body-Politic makes to the purposes of Preventive State Medicine in this country, let us next see how we of the Medical Profession stand in respect of the scientific contribution which we distinctively owe to the same great object. In preventive, just as in curative, medicine, it occasionally happens that consequences, more or less valuable, result from some mere chance-hit of discovery; but except so far as this may sometimes (and but very rarely) happen, disease can only be prevented by those who have knowledge of its CAUSES—knowledge, which does not deserve to be called knowledge, unless in proportion as it is *conclusive and exact*; and thoroughly to investigate the causes and their mode of operation is the quite indispensable first step towards any scientific study of prevention. Essentially, we know how to prevent by having first learnt exactly how to cause. Therefore it is that preventive medicine has had almost no development until within these later times. The germinal thought of it may be traced in even the first days of our profession. The spirit, in regard of which Hippocrates has been aptly called the Father of Medicine—the scientific spirit of observation and experiment, as distinguished from the spirit of priestcraft, was one which his medical writings equally showed in their preventive as in their curative relations; and when he, some twenty-three centuries ago, expounded to his contemporaries that pathology is a branch of the Science of Nature—that causes of disease are to be found in physical accidents of air and earth and water, and in quantities and qualities of food, and in personal habits of life, he (not without risk of being denounced for impiety) virtually proclaimed for all time the first principle of preventive medicine, and indicated to his followers a new line of departure for those who would most largely benefit mankind. His followers, however, have had their work to do. True knowledge of morbific causes could only come by very slow degrees, and as part of the development with which the physical and biological sciences have, little by little, with the labour of ages, been building themselves up; and so, no wonder that, despite the lapse of time, even the most advanced of nations are hitherto but beginning to take true measure of the help which preventive medicine can render them.

Now, what is the nature of that *study of causes* through which we may gradually arrive at counter-causing or preventing? Addressing a skilled audience, I shall utter what to them is the merest commonplace when I say that, in the physical and biological sciences, we acknowledge no other study of causes than that which consists in EXPERIMENT. The study of morbific causes is no exception to that rule. It is solely by means of EXPERIMENT that we can hope so to learn the causes of disease as to become possessed of resources for preventing disease.

The experiments which give us our teaching with regard to the causes of disease are

* Eleventh Report of the Medical Officer of the Privy Council, 1869, pp. 20, 21.

of two sorts: on the one hand we have the carefully pre-arranged and comparatively few experiments which are done by us in our *Pathological Laboratories*, and for the most part on *other animals than man*; on the other hand, we have the experiments which *Accident* does for us, and, above all, the incalculably large amount of crude experiment which is *popularly done by man on man* under our present ordinary conditions of social life, and which gives us its results for our interpretation.

When I say that experiments of those two sorts are the sources from which we learn to know the causes of disease, I, of course, do not mean that the mental process by which an experiment becomes instructive to us is the same in regard of the two sorts of experiment. On the contrary, the ætiological problem (so long as it is a problem) is approached in the two cases from two opposite points of view; and the dynamical continuity of relation, which we call cause and effect, is traced, in the one case from the one pole, and in the other case from the other pole of the relation. In the one case, starting with knowledge of our own deliberately prepared *cause*, our question is, what will be its effect? In the other case, starting from a certain *effect* presented to us, our question is, what has been its cause? But in the second case, just as in the first, when the question is answered, when the problem is solved, when the relation of cause and effect has been made clear, we recognize that the conjuring-power which has brought us our new knowledge is the power of a *performed experiment*.

Let me illustrate my argument by showing you the two processes at work in identical provinces of subject-matter.—What are the classical experiments to which we chiefly refer when we think of guarding against the dangers of Asiatic cholera? On the one side there are the well-known *scientific* infection-experiments of Professor Thiersch, performed on a certain number of mice. On the other hand, there are the equally well-known *popular* experiments which, during our two cholera epidemics of 1848–9 and 1853–4 were performed on half a million of human beings, dwelling in the southern districts of London, by certain commercial companies which supplied those districts with water. Both the professor and the water-companies gave us valuable experimental teaching as to the manner in which cholera is spread. [See, at end of Address, the appended Note A.] I need not now state at length the facts of those experiments, probably known to all here, but may rather justify my parallel by referring to a different ætiological question which will be presently discussed in our Section. It concerns the *causation of tubercle*—the most fatal by far of all the diseases to which the population of this country is subject. On that subject, for the last sixteen years, we have had a new era of knowledge. It was the great merit of a Frenchman, M. Villemin, that he, in 1865, first made us fully aware that tubercle is an infectious disease. He did this by certain *laboratory experiments* performed on other animals than man. He found that general and fatal tubercular infection of the animal was produced when he inoculated it subcutaneously with a little crude tubercular matter from the human subject. That first laboratory investigation of the subject has been followed most extensively by others; and the further experiments, while entirely confirming M. Villemin's discovery, have shown that subcutaneous inoculation is not the only mode by which the tubercular infection can be propagated. Dr. Tappeiner and others have shown that the same effect is produced on the animal if tubercular matter (such as the sputa of phthisical patients) be diffused in spray in the air which the animal breathes; and Professor Gerlach, of Hanover, showed, twelve years ago, with regard to the bovine variety of tubercular disease (the *perlsucht* of the Germans) that its infection can be freely introduced through the stomach if bits of tubercular organs be given in the food, or if the healthy animal be fed with milk from the animal which has tubercle. That the communicability of tubercle from animal to animal is also being tested to an immense

extent by *popular experiments* on the human subject, is what a moment's reflection will tell; and from that wide field of experiment I select one instance for illustration. I have every reason to believe that Professor Gerlach's experiments on the communicability of tubercle by means of milk are very extensively parodied by commercial experiments on the human subject. I learn, on what I believe to be the highest authority in this country, that tubercle (in different degrees) is a malady which abounds among our cows; and that so long as the cow continues to give milk, no particular scruple seems to prevent a distribution of that milk for popular use. To the persons who consume that milk an important question as to the causability of tubercle is put in an experimental form. Whether they will become infected with tubercle, is a question which the individual consumers do not stand forward to answer for themselves, like the animals of the laboratory experiments: but Dr. Creighton's lately published book, entitled, "Bovine Tuberculosis in Man," and a paper in which I am glad to say he brings under notice of our Section the very remarkable series of facts on which he grounds that startling title, seem to suggest a first instalment of answer in accordance with Professor Gerlach's experimental finding.

The two sorts of experiment—the scientific and the popular—differ, as I have noted, in this particular: that the popular experiment is almost always done on man; the scientific almost always on some other animal. It is true that many memorable cases are on record, where members of our profession have deliberately given up their own persons to be experimented on by themselves or others for the better settlement of some question as to a process of disease; have deliberately tried, for instance, whether, in this way or in that, they could infect themselves with the poison of plague or of cholera; and, as regards one such case which is in my mind, I think it not unlikely that the illustrious life of John Hunter was shortened by the experiments which he did on himself with the ignoble poison of syphilis. There have been cases, too, where criminals have been allowed to purchase exemption from capital or other punishment at the cost of allowing some painful or dangerous experiment to be performed on themselves. And cases are not absolutely unknown where unconsenting human beings have been subjected to that sort of experiment. But, waiving such exceptions, the rule is, as I have said, that scientific experiments relating to causes of disease are performed on some animal which common opinion estimates as of lower importance than man. Now, as between man and brute, I would not wish to draw any distinction which persons outside this room might find invidious; but, assuming for the moment that man and brute are of exactly equal value, I would submit that, when the life of either man or brute is to be made merely instrumental to the establishment of a scientific truth, the use of the life should be economical. Let me, in that point of view, invite you to compare, or rather to contrast with one another, those two sorts of experiment from which we have to get our knowledge of the causes of disease. The commercial experiments which illustrated the dangerousness of sewage-polluted water-supplies cost many thousands of human lives; the scientific experiments which, with infinitely more exactitude, justified a presumption of dangerousness, cost the lives of fourteen mice. [See Note A.] So, again, with experiments as to the causation of tubercle:—judging from the information which I quoted to you, I should suppose that the human beings whose milk-supply on any given day includes milk from tubercular cows might be counted, in this country, in tens of thousands; but the scientific experiments which justify us in declaring such milk-supply to be highly dangerous to those who receive it were conclusive when they amounted to half a dozen. So far, then, as regards the mere getting of experimental knowledge, we must not, with a view to economy of life, be referred to popular, rather than scientific, experiment. And in the

same point of view, it perhaps also deserves consideration that the popular experiments, though done on so large a scale, very often have in them sources of ambiguity which lessen their usefulness for teaching.

Let me now briefly refer to the fact that, during the last quarter of a century, all practical medicine (curative as well as preventive) has been undergoing a process of transfiguration under the influence of laboratory-experiments on living things. The progress which has been made from conditions of vagueness to conditions of exactitude has, in many respects, been greater in these twenty-five years than in the twenty-five centuries which preceded them; and with this increase of insight, due almost entirely to scientific experiment, the practical resources of our art, for present and future good to the world, have had, or will have, commensurate increase. Especially in those parts of pathology which make the foundation of preventive medicine, scientific experiment in these years has been opening larger and larger vistas of hope; and more and more clearly, as year succeeds year, we see that the time in which we are is fuller of practical promise than any of the ages which have preceded it. Of course, I cannot illustrate this at length, but some little attempt at illustration I would fain make.

First, let us glance at our map. When we generalize very broadly the various causes of death (so far as hitherto intelligible to us) we see them as under two great heads, respectively autopathic and exopathic. On the one hand, there is the *original and inherited* condition under which to every man born there is normally assigned eventual old age and death, so that, sooner or later, he "runs down" like the wound-up watch with its ended chain; and, as morbidities under this type, there are those various original peculiarities of constitution which make certain individual tenures of life shorter than the average, and kill by way of premature old age of the entire body, or (more generally) by quasi-senile failure of particular organs. On the other hand, as a second great mass of death-causing influence, we see the various interferences which come *from outside:* acts of mechanical violence, for instance, and all the many varieties of external morbific influence which can prevent the individual life from completing its normal course. As regards cases of the first class—cases where the original conditions of life and development are such as to involve premature death (which in any such case will commonly show itself as a fault in particular lines of hereditary succession)—the problem for preventive medicine to solve is, by what cross-breeding or other treatment we may convert a short-lived and morbid, into a long-lived and healthy, stock; and this, at least as regards the human race, has, I regret to say, hardly yet become a practical question. But, as regards cases of the second class, evidently the various extrinsic interferences which shorten life have to be avoided or resisted, each according to its kind; and here it is that the scientific experimenters of late years have been giving us almost daily increments of knowledge. Two early instances, vastly important in themselves, though of a comparatively crude kind, I have already mentioned; and I now wish to glance at some illustrations of the immense scope and the marvellous exactitude of the newer work.

The invaluable studies of M. Pasteur, beginning in the facts of fermentation and putrefaction, and thence extending to the facts of infectious disease in the animal body, where M. Chauveau's demonstration of the particulate nature of certain contagia came to assist them—they, I say, partly in themselves, and partly in respect of kindred labours which they have excited others to undertake—have introduced us to a new world of strange knowledge. We have learnt, as regards those diseases of the animal body which are due to various kinds of external cause, that probably all the most largely fatal of them (impossible yet to say how many) represent but one single kind of cause, and

respectively depend on invasions of the animal body by some rapidly self-multiplying form of alien life. This doctrine, which scientific experiment initiated, has, for the last dozen years, been extending and confirming itself by further experiment. As soon as the doctrine began to seem probable, science saw that, should it prove true, it must have the most important corollaries. If the cause of an infecting human disease is a self-multiplying germ from the outside world, the habits of that living enemy of ours can be studied in its outside relations. It becomes an object of common natural history, it has biological affinities and analogies. We can cultivate it in test-tubes in our laboratories, as the gardener would cultivate a rose or an apple, and we can see what agrees and what disagrees with its life. And then, as the next and immeasurably most important stage, where nothing but experiment on higher living bodies will help us, we can try whether perhaps any of our modifications of the enemy have made it of weaker power in relation to the bodies which it invades, or whether, through our more intimate knowledge of its vital affinities, we can artificially give to bodies which it would invade a partial or complete protection against it. Such, at first blush, were the obvious possibilities of research which the new doctrine of infectious disease suggested to the mind of the pathologist; and never since the profession of medicine has existed had a field of such promise been before it. The promise has not been belied. A host of diseases has been worked at in such lines as I just now indicated, and with many of them important progress has been made.

It would be impossible for me even to name a twentieth part of the investigations which have been more or less successful. As regards some which have most struck me, I pass with but a word Dr. Klein's investigation of the pneumo-enteritis of swine; Professor Cohn's and Dr. Koch's and Dr. Buchner's respective contributions to the natural history of the anthrax bacillus; Dr. Bollinger's recognition of the microphytic origin of an important cancroid disease of horned cattle, with Dr. Johne's illustration of the inoculability of this disease; the research by Professors Klebs and Tommasi-Crudeli into the intimate cause of marsh-malaria; and, not least, the demonstration (as it appears to be) which Dr. Grawitz has recently published, that some of the commonest and most innocent of our domestic microphytes can be changed by artificial culture into agents of deadly infectiveness. I pass these and others, in order that I may more particularly speak of some which have already shown themselves practically useful; for in respect of some of them the time has already come when abstract scientific knowledge is passing into preventive and curative act.

First, and not in a spirit of national partiality, I will mention the application which M. Pasteur's doctrine has received at the hands of Mr. Lister, with regard to the antiseptic treatment of wounds: an application which, enforced and illustrated at every turn by Mr. Lister's own eminent skill as an experimentalist, has been confirmation as well as application of the parent doctrine; and the beneficent uses of which, in giving comparative safety to the most formidable surgical operations, and in immensely facilitating recovery from the most dangerous forms of local injury, are recognized—I think I may say, by the grateful common consent of our profession in all countries—to be among the highest triumphs of preventive medicine.

Secondly, out of the experimental studies of anthrax—chiefly out of those of Prof. Sanderson and Mr. Duguid in this country, and those of Dr. Buchner in Germany, and M. Toussaint in France, has grown a knowledge of various ways in which the contagion of that dreadful disease can be so mitigated that an animal inoculated with it, instead of incurring almost certain death, shall have no serious illness; and the further knowledge has been gained, that the animal submitted to that artificial procedure is thereby more or less secured against subsequent liability to the disease. In other words, with regard

to that disease, an infliction which sometimes spreads to man from his domestic animals, and one which in some parts of Europe is of serious consequence to agricultural interests as well as to animal life, the later experimenters—of whom I may particularly name M. Toussaint, and our countryman, Professor Greenfield, seem to be giving to the animal kingdom, and to the farmers, the same sort of boon as that which Jenner gave to mankind when he taught men the use of vaccination. Quite recently, our great leader, M. Pasteur, seems to have made, by new experiments, still further progress in the mitigation of anthrax.

Thirdly, a similar discovery has been made by M. Pasteur, with reference to the contagium of a very fatal poultry-disease, known by the name of fowl's cholera; he has learnt to mitigate that contagium to a degree, in which, if fowls be inoculated with it, they will suffer no serious ailment; and he has found that fowls so inoculated (or, as he, in honour of Jenner, would say, "fowls so vaccinated") are proof against future attacks of the disease.

Fourthly, Professor Semmer, of Dorpat, through experiments done under his direction by Dr. Krajewski, has made a similar discovery in regard of the infection of septicæmia—namely, he has found that by treatment like that with which M. Toussaint mitigates the contagium of splenic fever, he can bring the most virulent septic contagium into a state in which it shall be mild enough to serve for harmless inoculations; which inoculations, when performed, shall be protective against future infections.

Finally, in a different direction of experimental work, let me name the recent most admirable research which Dr. Schüller of Greifswald has made, nominally in respect of certain surgical affections of joints, but in reality extending to the inmost pathology and therapeutics of all tubercular and scrofulous affections. A knowledge of the fatal infectiveness of crude tubercular matter had been given (as I before said) by Villemin and those who followed him; and Professor Klebs, four years ago, declared the infective quality to be due to the presence of a microphyte (micrococcus) which he had succeeded in separating from the rest of the matter by successive acts of cultivation in fluids of inorganic origin. Dr. Schüller solidly settles, and widely extends, that teaching. According to his apparently quite unquestionable observations and experiments, the micrococcus which characterizes tubercle characterizes also certain affections popularly called "scrofulous"—namely, "scrofulous" synovial membrane, "scrofulous" lymph glands, and lupus: so that these diseases may be defined as essentially tubercular, and that inoculation with matter from any of them, or with a cultivation-fluid in which the micrococcus from any of them has been cultivated, will infect with general tuberculosis. The rapid multiplication of the tubercle-micrococcus in the blood and tissues of any inoculated animal can be verified both by microscopical observation and by inoculative experiment; and an extremely interesting part of the research, in explanation of certain of our human joint-diseases, is the demonstration that if in the inoculated animal a joint be experimentally injured, that joint at once becomes a place of preferential resort to the micrococcus which is multiplying in the blood, and becomes consequently a special or exclusive seat of characteristic tubercular changes. Even thus far the practical interest of Dr. Schüller's book is such as it would not be easy to overstate; but still greater interest attaches to the last chapter of the book, in which, confidently resting on the pathological facts which I have quoted, he makes proposals for the treatment of tubercle on the basis of its microphytic origin, and shows the successful result of such treatment as he has hitherto tried, from that basis, on animals artificially infected by him.

I venture to say that in the records of human industry it would be impossible to point to work of more promise to the world than these various contributions to the knowledge of disease, and of its cure and prevention; and they are contributions which from the

nature of the case have come, and could only have come, from the performance of experiments on living animals.

At this most productive epoch in the growth of medical science, our English studies have been interrupted. An Act of Parliament, passed five years ago under the title of the Cruelty to Animals Act, has made it difficult or impossible for scientific observers any longer to follow in this country any such courses of experiment as those which of late years, at the cost of relatively insignificant quantities of brute suffering, have tended to create an infinity of new resources of relief for the sufferings both of brute and man. The Act does not in express terms interdict all performance of such courses of experiment: it nominally allows them to be done under a variety of limited licenses which may be granted by a Principal Secretary of State; but the limitations under which these licenses are granted, and the trouble, delay, and friction which necessarily to some extent, and in fact often to an intolerable extent, attend the obtaining of any one of them, are practically little better than prohibition.

The Act apparently contemplates, as the chief subjects of its operation, an imaginary class of unqualified persons, who, with no legitimate relation to scientific research, would, under pretence of such research, torture, and (it is supposed) take pleasure in torturing, live animals; and against this devilish class of persons the Act is very indulgently framed: for, instead of expressly refusing licenses to unqualified persons, and perhaps hinting to such of them as would do wilful cruelty under pretence of study that the lash and the treadmill are for such scoundrels—instead of this, I say, the Act virtually confounds together that supposed class of unqualified and cruel persons, and, on the other hand, our professional class of *bonâ fide* scientific investigators, on whom the progress of medicine depends, and whose names are sufficient security for their conduct. What is counted good for the one class is also counted good for the other. The law will trust no licensed experimenter farther than it can provide for his being minutely watched and regulated by the Secretary of State: and in respect of the details of experimental procedure, the supervision of that high political officer is substituted for the discretion and conscience of the scientific investigator.

Consider for a moment what this means in regard of the members of our profession whom it affects. Contrast with it the almost unbounded trust with which the world, from time immemorial, has regarded the character of our profession. Consider the relations of inmost confidence in which members of our profession in every corner of the kingdom are admitted to share in the sanctities and tendernesses of domestic life. Consider our immense daily responsibilities of human life and death. Consider, for instance, that there is not a member of our profession to whom the law does not allow discretion that, in certain difficulties of child-birth, he shall judge whether he will kill the child to save the mother. And in contrast with all this, is it to be seriously maintained that society cannot trust us with dogs and cats? that our foremost workers—(for it is essentially they who are affected)—cannot be trusted to behave honestly towards their brute fellow-creatures, unless they be regulated and inspected under a special law in much the same prevenient spirit as if they were prostitutes under the Contagious Diseases Act?

I have reason to believe that, if that Act continues on the statute-book, one of two results will follow. Experiments, indispensably necessary for the growth of medical science in relation to the cure and prevention of disease, will cease, or almost cease, to be done in this country; or, as the alternative to this, persons who desire to advance the science of their profession, will be tempted to clandestinely ignore the law, and to run their chance, if the worst comes to the worst, of having to try conclusions with the common informer.

Let me illustrate this by two personal references. I have already mentioned Professor Lister, as an experimenter, whose name is now classical wherever science has reached, and whose work has been of signal advantage to mankind. Last autumn Mr. Lister wished to do some experiments in extension of the particular branch of knowledge with which his name is identified, and at a point which he considered of extreme importance in surgical pathology. He found he must either abandon his investigation or must conduct it in a foreign country, and, in his zeal for science, he chose the latter course. His experiments (which had to be on large animals) were done at the Veterinary College of Toulouse; and in stating this fact in a letter, from which I quote, Mr. Lister added that "even with reference to small animals, the working of the Act is so vexatious as to be practically prohibitory of experiments by a private practitioner like myself, unless he chooses to incur the risk of transgressing the law." A second name which I have mentioned is that of Professor Greenfield, who has so highly distinguished himself in developing, by means of experiments, the preventive medicine of splenic fever. Dr. Greenfield, in order to perform his inoculation-experiments, had, of course, to become a license-holder under the Act; and his experience of the hindrances which attach to that position is expressed to me in the following terms: "It is my deliberate conviction, as a result of my experience, that these hindrances and obstacles are so numerous and so great as to constitute a most serious bar to the investigation of disease, and even of such remedial measures as would by common consent be for the direct benefit of the animals experimented upon. When to this is added all the annoyance and opprobrium which are the lot of investigators, it is to be wondered at that anyone should submit to be licensed." Dr. Greenfield's experimental operations consisted only in inoculating the virus of animal diseases, and he says: "I have not been engaged in other investigations for the simple reason that, with the present restrictions, and the difficulty of obtaining a license, I regard it as almost hopeless to attempt any useful work of the kind in this country."

As I feel sure that the Act must at no distant time be reconsidered by the Legislature, and as I also very strongly feel that, quite apart from any question of legal enactments, there is the question of moral right or wrong to be considered in the matter, I would beg you to allow me to make my own public confession of faith (from which I dare say yours will not much differ) in that extremely important matter of controversy.

The question being, whether medical science can rightly use living animals as subjects for experiments which may be painful, and even, in exceptional cases, very painful to them, the answer may be sought in either of two directions: (1) What says the voice of the experimenter's own conscience?—and (2) What says the standard of common contemporary conduct in analogous cases?

As regards the first, if I may take the liberty of expressing my own feeling, I would say this. I do not in any degree regard it as matter of indifference that, in certain cases, by my own hand or by that of some one acting for me, I must inflict death or pain on any living thing. I, on the contrary, think of it with true compunction, but I think of it as good or bad according to the end which it subserves. Where I see my way to acquire, at that painful cost, the kind of exact knowledge which, either in itself or in contribution to our common stock, will promote the cure or prevention of disease in the race to which the animal belongs, or in the animal kingdom generally, or (above all) in the race of man, I no more flinch from what then seems to me a professional duty, though a painful one, than I would, in the days before chloroform, have shrunk from the cries of a child whom I had to cut for stone. If, in a case of the latter sort, the surgeon nerves himself to his work by the conviction of an indispensable usefulness in what he has to do, so does the pathologist in his, and surely in a much larger sense. The agitated parent of the child might sometimes be tempted to say—"Forbear giving

this cruel pain; let the poor little sufferer die;" but the surgeon's reply would have comforted her. And so with the physiological experimenter: except that he, instead of looking at one individual life to be saved, is looking at a race or at many races, and reflects how, in respect of some grievous physical misery, the whole of them, in all their multitudinous successions, may be redeemed through the suffering of the few. This is my personal view of the abstract right or wrong in the question. I state it because, in matters of right or wrong, no man ought to shelter himself behind authority. I believe I may add that if it, or something very like it, had not for centuries been the general view of the medical profession, our professional knowledge would probably be standing in this present age about where it stood in the days of the Plantagenets. Of Harvey and Hunter and Bell, we well know that such was the view on which they acted in rendering their immortal services to mankind; and I am not aware that any man whose opinion really counts in matters of medical science would express any material dissent from it.

The second standard to which I referred was that of the common conduct of men in analogous cases. I pointedly say "analogous," rather than "similar," because common life does not in fact give cases which, properly speaking, are "similar" to ours. But what, I ask, is the common *principle* of behaviour of civilized man to the so-called lower animals? He in every respect subordinates their lives to his own. If he thinks he can get an advantage to himself by killing or painfully mutilating an animal, he does so with apparently no hesitation. See, for one instance, the sexual mutilations which are inflicted on all but a small minority of the males of most kinds of domestic animals, and, as regards some kinds, on many of the females. When I appeared as a witness six years ago before the Royal Commission which was considering the question of our experiments, I particularly endeavoured to draw their attention to this view of the case; and in one of my answers I entered on it more fully than would be suitable to the present occasion. [See, at end of Address, Note B.]

Thus, either way, whether I look to what I may call the general conscience of the medical profession, or look to the principles by which men in general govern their conduct towards the brute creation, nowhere do I see fair ground on which exception from outside can be taken to a limited, a strictly economical, use of animal life for purposes of scientific experiment.

No doubt there can be found, outside the medical profession, excellent persons, and plenty of them, whose first inclination would be to dissent from that position of ours; and some such persons have (as I think, hastily) given public utterance to such first impressions, and done their best to promote legislation against us. Among names which I see identified with opinions different from ours, are some for which I have deep respect. Particularly, of one such man, whom I have the honour to know, I think it may be truly said that his own whole life has been one of practical beneficence, and I would not willingly incur the censure of any such man. But even to him I would fearlessly say, that I think he has not done justice to the case of our profession. To him, and such as him, I would confidently appeal to reconsider their first impressions. On him, and such as him, I would urge that the practice of scientific experimentation on living animals is but an infinitesimally small application of the license which common life claims for itself in regard of animals; and I would challenge such men to examine, with strict impartiality, what are their own responsibilities, direct and indirect, in regard of the infliction of pain on living animals.

I protest against any man's applying to this extremely important question a purely arbitrary standard of right or wrong. Those who pronounce judgment on their neighbours must be prepared to state the principle on which they judge.

"Compound for sins you are inclined to, by damning those you have no mind to," is the Pharisee's easy-going formula. Where would life be, if that were generally accepted? Suppose a *genus* of action; let men draw an arbitrary line across it—a line prescribed by no better rule than that which governed the lady's dislike to Dr. Fell; let them affix a nickname of praise to all on one side of the line, and a nickname of dispraise to all on the other: truly we should thus have the readiest of royal roads to unlimited mutual persecution.

And I protest against a standard of right and wrong being fixed for us on grounds which are merely sentimental. In certain circles of society, at the present time, æsthetics count for all in all; and an emotion against what they are pleased to call "vivisection" answers their purpose of the moment as well as any other little emotion. With such sections of society, our profession cannot seriously argue. Our own verb of life is ἐργαζεσθαι, not αἰσθανεσθαι. We have to think of usefulness to man. And to us, according to our standard of right and wrong, perhaps those lackadaisical æsthetics may seem but a feeble form of sensuality.

Of the mere screamers and agitation-mongers who, happy in their hysterics or their hire, go about day by day calumniating our profession, and trying to stir up against it the prejudices and passions of the ignorant, I have only to express my contempt.

I regret to have had to speak at so much length of the heavy cloud which at present hangs over the study of scientific medicine in England, and which, in my opinion, is likely to be of specially disastrous effect on the progress of preventive medicine. As a very old public servant in that cause, I should indeed grieve to see it brought to a standstill for want of the scientific nurture which, in truth, is its very basis of life: I should be indignant that the Samson, who would yet have fought for our people, had had his eyes thus put out by the Philistines: and I therefore speak publicly of the danger on this occasion, in hope that the occasion may give importance to what I say.

And now, gentlemen, from contemplating that cloud, which happily is but local, and which perhaps may be but temporary, I gladly turn to skies which have no cloud. If there exist in the social organism any function whatsoever for which development and eventual triumph may be foretold, surely it is that of State Medicine. Of the two great factors concerned in it—the two strong powers which within our own time have converged to make it the reality which it is—the growth of science on the one hand, and the growing stress of common humanity on the other, neither one is likely to fail. Of our science it is needless to say that it will grow. To the science of Nature, indeed, is allotted that one incomparable human day which knows no sunset. In the pure light of its ever-present daybreak, individual workers will pass away, generations will change, but the studies of Nature, and, above all, the gathering of such knowledge as can lessen man's physical difficulties and sufferings, will surely grow from age to age, and, as on Proserpina's sacred tree, one golden fruit will follow another: "simili frondescet virga metallo." And no less also in the other direction, the auguries are wholly for our cause. Popular education is gradually making its way, and will grow to be a force on our side. Masses of mankind, that now have to be humbly pleaded for by others, will then be strong to speak for themselves. Physical interests, now but little understood, will then be within grasp of all men's apprehension. Not only will health be recognized at its true value, and its elementary requirements be regarded, but also the frauds and villanies which are now committed against it will have become intelligible to the common mind; and the workman of the future will strike against being cheated in health as he would now strike against being

cheated in wages. As such times come to the world, the Science and the Profession which care for man as man will get to be better appreciated than now. And in proportion as educated nations grow into being true Bodies-Politic, State Medicine will surely have its due place in their regard, as chief minister to that ideal of Government-action which sets its standard of success in the "greatest happiness of the greatest number."

NOTE A.

The particulars of the experiments mentioned in the Address, as classical in the history of Cholera, were these.—(1) While cholera was invading Europe for the third time, in the years 1853–6, and when it was of immense importance for us all to know whether choleraic discharges are possessed of infective properties communicable to food or drinking water, Prof. Thiersch devised some experiments which he thought might enlighten us. He took mice for the subjects of experiment, and, having disposed them in couples under circumstances otherwise identical, he contrived that each of them should daily swallow with its food a small particle of filtering paper; and for a select number of the couples he provided that the particle of paper swallowed by each mouse should carry with it a minute inoculation of cholera-discharge. Careful experiments with cholera-discharge were thus done on 124 mice, and the results were seen to vary according to three different states of the discharge. With discharge quite fresh, or with discharge over-stale, the inoculation would produce no ill-effect: nor had any ill-effect been produced on the mice which swallowed the untainted paper; but infection-experiments, done with discharge neither too fresh nor too stale, gave marked results. The mice so inoculated were 56: of these 56 mice, 44 sickened with symptoms of a choleraic sort, and of the 44 which sickened, 14 died. Dr. Thiersch's experiments were accepted, not as exhausting the matter of inquiry, but as very strongly confirming suspicions, which had already been awakened, that cholera (in the human subject) spreads from sick to healthy by means of infectious discharges from the sick. (2) At about the same time with that experimental inquiry by Prof. Thiersch, a retrospective investigation was being conducted in London, as to the influence which had been exerted by the consumption of polluted water in affecting the distribution of cholera in the two epidemics of 1848–9 and 1853–4 among a large population supplied, in two distinguishable quantities, by two water-companies—the Southwark and Vauxhall, and the Lambeth. Each of those companies, during the epidemic of 1848–9, and one of them (the Southwark and Vauxhall) during the epidemic of 1853–4, had supplied its customers with sewage-polluted water from the Thames; and the miscellaneous sewage with which the water was polluted had included, of course, choleraic discharges. The statistics of the two outbreaks in relation to the two quantities of local population were, as nearly as could be estimated, these:—The quantity of population supplied by the Southwark and Vauxhall company suffered in the epidemic of 1848–9 at the rate of 118 cholera-deaths for each 10,000 of population, and in the epidemic of 1853–4 at the rate of 130 for each 10,000. The quantity of population supplied by the Lambeth company suffered in the epidemic of 1848–9 at the rate of 125 for each 10,000 of population; but, in the epidemic of 1853–4, only at the rate of 37. And what was the single discoverable difference of condition in favour of the one quantity of population, which suffered comparatively so little when the second epidemic befell? The company which supplied it with water had, in the interval since the first epidemic, improved its supply to comparative excellence; whereas the other company was distributing water of perhaps even filthier quality than before. It was my duty in 1856 to report the results of that inquiry, and one passage of my report may be considered side by side with the record of Dr. Thiersch's "cruelty" to mice:—"An experiment, at which mankind would have shuddered if its full meaning could have been prefigured to them, has been conducted during two epidemics of cholera on 500,000 human beings. One-half of this multitude was doomed in both epidemics to drink the same fæcalized water, and on both occasions to illustrate its fatal results; while another section, freed in the second epidemic from that influence which had so aggravated the first, was happily enabled to evince by a double contrast the comparative immunity which a cleanlier beverage could give. By this experiment, it is

rendered in the highest degree probable that, of the 3,476 tenants of the Southwark and Vauxhall Company who died of cholera in 1853–4, two-thirds would have escaped if their water-supply had been like their neighbours'; and that, of the much larger number—tenants of both companies, who died in 1848–9—also two-thirds would have escaped if the Metropolis Water Act of 1852 [under the provisions of which the Lambeth Company had changed its source of supply] had but been enacted a few years earlier." Yet—did the thousands of human beings who died under the auspices of commercial experiment add more definitely to our knowledge of the mode of propagation of cholera than did the 14 mice who died under Dr. Thiersch's experiment?

I may add that, in the cholera epidemic of 1866, commerce and science were again contributory from their respective fields of experiment. On the one hand, Dr. Sanderson and Dr. Thudichum (in scientific investigations made for the Lords of the Council) repeated the experiments which Thiersch had made, and they gave valuable corroboration to his results. On the other hand, it having happened that public attention was drawn to an extreme predominance of cholera in the Eastern districts of London—a predominance such that those districts suffered more than two-thirds of all the cholera mortality of London—Mr. J. Netten Radcliffe investigated for the Privy Council the local conditions of that predominance, and the gist of his report was this:—"The sudden and virtually contemporaneous development of the outbreak over the entire area of prevalence indicated a medium of propagation common to, and capable of rapid diffusion over, the whole area; its sudden declension indicated the temporary efficiency to this end of such a medium. The area of prevalence approximated with remarkable closeness to a particular field of water-supply; and there are facts which seem to prove that this approximation was not accidental. It is known that, immediately prior to the outbreak in the east districts of the metropolis and neighbouring districts across the Lea, impure water was distributed over this field of supply, and it is highly probable that this water was charged with choleraic poison." The water-supply to which Mr. Radcliffe referred, was that of the East London Company.

Note B.

The terms in which, in my evidence before the Royal Commission of 1875, I commented on certain proposals then before Parliament for a law to restrict physiological experiments were the following:—"I take up, for instance, one of these Bills, and I find this: 'Whereas it is expedient to prevent cruelty and abuse in the *experiments made on living animals for the purpose of promoting discoveries in the sciences of medicine, surgery, anatomy, and physiology.*' Now, suppose this preamble to be before either of the Houses of Parliament—(I speak of course with great respect to those Houses)—do you suppose that one of Magendie's experiments, the worst of them, is more cruel to the sufferer of it than hare-hunting to the individual hare, or fox-hunting to the individual fox? And is animal life less to be protected against 'cruelty' when the life is sacrificed for purposes of sport than when it is sacrificed for purposes of science? You are proposing that physiologists shall be treated as a dangerous class, that they shall be licensed and regulated like publicans and prostitutes. What I would venture to put before you is, that this would be fancy-legislation, touching the relations of man to the lower animals at a little bit, and a comparatively unimportant little bit, of the subject matter; and that society would come to such legislation with unclean hands. Who is the accuser of the physiologists? Society assumes a universal right to slaughter animals for its food, to cut their throats or wring their necks at its discretion, and neither stints its luxury (much less its hunger) in reluctance to take life, nor troubles itself much about painless methods of killing. To kill particular animals in particular ways is a considerable branch of national amusement; and the wealthy breed certain animals on a large scale exclusively to have sport in killing them, and for the unpractised often to mangle where they do not kill. Also with a view to slaughter, or with a view to other service, society inflicts sexual mutilation on nearly all the males of the cattle, horses, sheep, and swine, which it controls; and as to swine, on many of the females. You never eat a [male] mutton chop that does not come from an emasculated sheep: you rarely use a male horse that has not had its testicles cut out.

Now, when the common habit of society treats domestic animals in this way, slaughtering and mutilating them at its will, for use, for convenience, for luxury, and while battues and wager-slaughtering and hare-hunting are elements of national sport, it seems altogether monstrous to put into a separate category the extremely small use which physiology (for great human interests) makes of the lives of brute animals, and to have as the preamble of a Bill that cruelty is to be prevented in that one relation, while all the other relations are to be left loose."—Evidence, Q. 1491.

I.—MEASURES BY WHICH TO PREVENT THE DIFFUSION OF DIFFERENT COMMUNICABLE DISEASES FROM COUNTRY TO COUNTRY, OR WITHIN THE LIMITS OF ANY SINGLE COUNTRY.

The Experience of the United States in recent years with regard to Asiatic Cholera and Yellow Fever.

Surgeon JOHN S. BILLINGS, United States Army.

The subject which has been assigned me, is the experience of the United States in recent years with regard to Asiatic cholera and yellow fever; having special reference to the prevention of the introduction or spread of these diseases.

With regard to cholera, the opinions of the majority of American physicians agree, in the main, with the conclusions of the Cholera Conferences of Constantinople and Vienna. The Government Report on the cholera of 1873 maintains what Pettenkofer terms the origin by contagion of cholera, as opposed to the theory of its local or endemic origin in America.

It does not, however, maintain the personal contagion of the disease, and the difference between the two views seems to be of small importance so far as practical measures of prevention are concerned.

When cholera exists in Europe, whether it be in the sea-ports or not, it may at any time be carried to the United States with the tide of emigration, and the conditions under which emigrants arrive in America are such, that if cholera can be introduced by fomites, such as bedding or clothing, these may readily pass, without being suspected, through the port of entry, and even far into the interior, and then produce their specific effect.

It is probable that cholera was, in this manner, introduced in the United States more than once during 1873, although such transmission has not been positively proved.

The most important means of preventing the introduction of cholera into the United States are:—

First, those which tend to restrict its spread in or from India.

Second, those which might be employed at those European ports from which emigration to America chiefly occurs.

Third, proper systems of quarantine at American ports, which should include, among other things, the thorough aëration, cleansing, and disinfection of all probable fomites.

With regard to the first means referred to, I leave its discussion to others. The second means would form a part of an international system, to which I will refer hereafter. The third also comes under the general head of quarantine, which will be alluded to in speaking of yellow fever. I will only say in this connection that the present quarantine systems of the United States are now probably unable to

prevent the introduction of this disease, as they have been heretofore. Unless the disease had actually occurred on board ship, very little precaution would be taken, and very few of our ports have the necessary facilities for properly dealing with a large passenger ship, having cholera on board, in such a manner as to obtain a reasonable amount of security without causing unreasonable delay, involving unnecessary suffering and danger on the part of those not actually sick.

Much can be done to check and stamp out an outbreak of cholera by securing pure water supplies, and by the proper use of disinfectants, and it is probable that much more can be effected in the way of personal prophylaxis in this disease, than in yellow fever.

The experience of our Army medical officers in checking the spread of cholera among the troops, by careful supervision of their water supply, and the results of the use of disinfectants in New York, and on Blackwell's Island, illustrate these statements.

With regard to yellow fever, I shall speak only of the experience of the last four years.

The epidemics of 1878 and 1879 produced a large crop of yellow fever literature, but two of the most carefully prepared and extensive reports on this subject are still unpublished—viz., that of the Congressional Yellow Fever Commission of 1878-79, and that of the Havana Yellow Fever Commission of 1879-80.

The chief conclusions of the authors of these reports are, however, well known, and, indeed, so far as they relate to practical measures of prevention, have been made public. Before summarizing them, and to show the effect of the recent epidemics upon professional opinion, I will refer to the prevalent opinion of twenty years ago, as indicated in the reports of the National Quarantine Conventions of 1859 and 1860.

In the Convention of 1859, the following resolution was adopted by a vote of 70 to 4: "That in the absence of any evidence establishing the conclusion that yellow fever has ever been conveyed by one person to another, it is the opinion of this Convention that the personal quarantine of cases of yellow fever may be safely abolished, provided that fomites of every kind be rigidly restricted." Upon this I will merely remark that the premises do not warrant the conclusion, whatever may be the truth of either. The report of the debates in the conventions referred to, show that the views of Dr. La Roche were in the ascendant, and as these views are still urged by some American writers, it may be well to summarize them. Dr. La Roche held that the poison of yellow fever cannot be produced or reproduced in a man in such a way as to affect other men who approach or touch him; that epidemics of this disease are of local origin, and that, therefore, its introduction and spread cannot be prevented by quarantine. As he believed that the cause of the disease was not in any way produced by the sick, he taught that although a vessel may introduce the poison in a locality, yet on the removal of the vessel, the disease would cease to spread, and that individuals could have no agency in developing or locating the cause of yellow fever in a vessel or in a locality.

These views accord in many respects with those of Pettenkofer, who would, however, allow that an infected person might be the immediate cause of the development of cholera or yellow fever in a given locality, provided the locality presented a certain unknown something, *a tertium quid*, which is necessary to the development of the poison.

Contrast with these views the conclusions of the Board of Experts appointed by Congress to investigate the yellow fever epidemic of 1878. They hold that the

cause of yellow fever is specific, particulate, and endowed with the vital properties of growth and reproduction; that the disease has never acquired permanent domicile in the United States, in which every epidemic can be traced with a high degree of probability to a fresh importation; that yellow fever patients are the most frequent cause of the spread of the disease from place to place, and that more epidemics have resulted from persons sick with the fever than from all other causes. They do not ignore the many cases in which persons sick with yellow fever do not communicate the disease, nor do they attempt to decide to what extent the body of the sick person, as distinguished from his clothing and baggage, is responsible for this result.

Infected vessels are a most dangerous source of importation, but only a small percentage of vessels coming from infected ports are themselves infected.

In this connection I will give one or two of the conclusions of the Havana Yellow Fever Commission, as reported by Dr. Chaillé, who was also one of the experts of the Congressional Commission just referred to.

Yellow fever is not inoculable nor personally contagious, but it is portable and communicable. The poison is not located in the water of the sea or of harbours, and the risk of infection of a vessel increases in proportion to its proximity to an infected shore or vessel—the danger ceasing within a few hundred feet. The harbour of Havana is exceptionally dangerous, because exceptionally small. Yellow fever is not exceptionally severe in the shipping of infected ports unless a relatively large number of susceptible sailors are present.

In Havana it is not the sailors but the soldiers who suffer most.

Cemeteries do not generate or favour the spread of yellow fever. The preservation and growth of the poison is favoured by places containing warm damp foul confined air, such as holds of ships, cellars, trunks, &c., and therefore, presumably, privies and sinks; but evidence is wanting that human excreta, even from those infected, have any special influence on the spread of this disease.

There is no evidence that the peculiar geology or physical geography of a place has any special influence on yellow fever. The influence of latitude is due to temperature, and the favourable influence of altitude is also due to temperature, to exposure to wind and to the sparse population, and the restricted intercourse of high localities. Frost does not occur in Cuba, but yellow fever declines in September and October at a time when the number of susceptible persons increases, and when both the humidity and the unsanitary evils are unchanged; hence the usual decline is not due to frost, nor to the absence of any condition known to be requisite. Dr. Chaillé concludes that these facts are unexplainable, except on the hypothesis that the poison of yellow fever has a limited period each year for reproduction, but that in certain unknown conditions, which are exceptional, the poison may produce a double crop; that within as without the tropics, it manifests an annual tendency to die out, thus annually presenting favourable opportunities to control it, and encouraging the hope that it might be eradicated from even its tropical strongholds. He also concludes that there is no reason to suppose that the direct influence of climate renders man unsusceptible to the poison of yellow fever, and that hence, to call his accession to immunity acclimatization, is an abuse of language; that the fact that the natives of a place apparently do not suffer from the disease is evidence that the poison is present; that the natives have been subjected to it, and have acquired immunity which is certainly not due to inheritance, and is, in an immense majority of cases, due to an attack of the disease during infancy and childhood.

These conclusions are accepted by a majority of the physicians of the United States, who have had much experience with yellow fever, and by almost all state and municipal health authorities, south and south-west, as the basis of their quarantine regulations.

Into the details and differences of these local quarantines I do not propose to enter. Theoretically, they, as a rule, err on the side of excessive requirement; practically, the error is the other way. Neither in America nor in Europe can one draw reliable conclusions as to what is actually done at a quarantine establishment from its printed regulations.

It is, of course, understood that the summary of opinions which has been given represents probabilities only, and that some of them stand much higher than others in the scale of probability. When we come to consider the practical measures which we should advise cities or states to take in order to prevent the importation or spread of such diseases as cholera or yellow fever, we must advise, in accordance with these probabilities, keeping in view certain other considerations which do not enter into the purely medical or scientific view of the question. This brings us to the subject of quarantine. I do not use this word quarantine in the sense in which it is ordinarily employed in Europe, but rather in the sense of the term "medical inspection," as used by the Vienna Conference; its precise significance being as defined by the National Board of Health—*i.e.*, "The administration employed to determine the presence or absence of the causes of contagious or infectious diseases, and to secure the removal or destruction of such causes, if present; and it does not imply detention for any specified time, nor for more time than is necessary for the above purposes."

No system of quarantine, however organized, can be expected to remain thoroughly efficacious for more than a very few years, for it will not be supported by public opinion. For the first year or two after a great epidemic, sanitary rules can be enforced; but, as Hirsch says, "It is human nature to soon forget past sufferings. We bury our dead; a little time will dry our tears; in another little time we dance over their graves."

I wish to call attention especially to one point in which yellow fever differs markedly from other diseases against which quarantines are established, and it is a point that is usually overlooked in discussions on this subject. As an epidemic of yellow fever is cut short by cold weather, it follows that, other things being equal, the later in the season the disease gains a foothold in a place, the less its effect; therefore, a system of quarantine, or, if you please, of medical inspection, which, though not complete, nor able entirely to prevent the importation of the disease, will prevent it in a certain percentage of cases, thereby tending to delay its entrance, even though it be but a few weeks, may be very useful and well worth establishing; while the same may not be true as regards such a disease as cholera, which, when once introduced, will run its course and do its work. It is for this reason that the conclusions of the Constantinople and Vienna Conferences, as to the inexpediency of inland quarantines, do not apply to yellow fever.

Prior to the establishment of the National Board of Health, all discussions with regard to maritime quarantine for the United States took it for granted that each port should have its own quarantine establishment. As such establishments are very expensive, both to establish and to maintain, this is practically imposible for many of the small ports. It is now generally admitted that twelve such stations will be ample for the Atlantic and Gulf Coasts of the United States.

When yellow fever has gained a foothold in the country, much may be done to

limit its spread, or to stamp it out altogether. The means advised for this purpose by the National Board are isolation, disinfection, and depopulation, as described in detail in the instructions given to its inspectors during the prevalence of the epidemic of 1879 (see *National Board of Health Bulletin*, Jan. 1, 1880, p. 4).

To what extent the systematic cleansing and disinfection carried out in the infected portion of New Orleans, during the summer of 1879, should be credited with the cessation of the disease, it is hard to say; but the results obtained certainly warrant the trial of similar means in a similar case; and the same remark applies to the means used to prevent the spread of the disease from Memphis.

It is necessary to bear constantly in mind that the course of yellow fever in a community depends so much upon the number of susceptible persons present, that, unless this be known, data as to other influences of the environment upon the disease have very little scientific value.

Thus far, yellow fever cities and yellow fever ships have been filthy cities and foul ships. Whether the disease would spread and become epidemic in a clean, well-drained, properly sewered city, we do not know, as there are no such cities in the yellow fever region. But there does seem reason to believe that yellow fever might spread under certain circumstances in a clean, dry, sound ship. At all events, no health authority would be justified in treating such a ship as if it could not become infected. There is great liability to failure in attempts to disinfect ships infected with yellow fever, as appears from the cases of the *Plymouth* and *Excelsior*. The *Plymouth* is often quoted as an instance of the failing of freezing to destroy the yellow fever poison, but this is an error. If the two cases which occurred on her second trip were really cases of the fever, the report of the examination of the ship shows that neither the cold nor the disinfecting gas employed had penetrated all the closed recesses of the ship, which contained masses of filth and many living forms of various kinds. To entirely destroy the vitality of the lower organisms, when dry, by means of sulphurous acid gas, requires a very much longer exposure than is ordinarily supposed. Probably, not less than forty-eight hours would be necessary to give entire security. From what is known of the effects of cold on yellow fever, it seems probable that a ship—at all events, a wooden ship—might be disinfected by artificial cold, and the experiment is worth trying. It is, however, an experiment which would be very difficult to make, owing to the absence of the test for the presence of the causes of this disease, other than the production of the disease itself in man. The same difficulty also applies to cholera. The great desideratum at present, from both a scientific and practical point of view, is such a test as I have just referred to. It has been sought for—for cholera in India, and for yellow fever by the Havana Commission—but without success as yet.

It will be seen at once, that if an infected ship arrives at New Orleans, and is exposed to the operation of the artificial cold for disinfecting purposes, that the health authorities of that port would not be justified in allowing the ship to proceed at once to the wharves without further treatment, since this might involve the greatest danger to the health of the population of the whole Mississippi Valley. They would certainly require that the vessel should be thoroughly cleansed, and probably fumigated with sulphurous acid gas; and if this were done, and no evil results followed, it would be impossible to say that the exemption was due to the effect of the artificial cold employed. The only method of testing it would seem to be to use such artificial cold at some port like New York, and to use it prior to and during the removal of cargo. At New York this removal is effected by

persons, the majority of whom are unprotected; and every year there are cases of yellow fever at this port among the labourers on infected ships. If, now, the use of this artificial cold would prevent the occurrence of such disease among the labourers for two or three years, it might be considered a sufficiently satisfactory test to warrant relying on it entirely as a *disinfectant*, although it certainly could not be allowed to do away with the necessity of thoroughly cleansing the ships in all accessible parts. Practically, difficulties in preventing the spread of yellow fever, when it has once been introduced, depend largely on the difficulties of effecting isolation in the earlier cases, when such isolation has its chief value; and these, in turn, are due mainly to the difficulty of ascertaining the existence of such cases. So far as this difficulty depends upon difficulty in diagnosis, or difference of opinion between physicians in doubtful cases, an attempt has recently been made by the physicians of New Orleans to formulate a system of diagnosis. To make a quarantine practicable, it must not interfere with commerce more than is absolutely necessary to obtain the security: and to effect this, a most important requisite is reliable information as to the existence of the disease in foreign ports, and also as to the sanitary history of ships coming from infected ports. Heretofore, the majority of American ports have had quarantine against all ports in inter-tropical Atlantic, from May to November, and have subjected all vessels coming from such ports to the same treatment; although not one out of ten of these ports was affected, and only a small proportion of the ships coming from ports which were infected presented special danger. This course has been rendered necessary, for the simple reason that bills of health, as given, have been worthless as evidence of the condition of either the port of departure or of the ship itself.

No doubt yellow fever might be brought into the United States by a vessel coming from a port in which the disease did not exist at the time of departure, for we have too many well authenticated instances of its being preserved for months in personal baggage to permit of giving positive guarantees of safety from it within the yellow fever zone; but we can only deal with probabilities in this matter.

The Quarantine Law of the United States, passed in 1879, provides that all vessels coming from any foreign port where contagious or infectious disease exists, shall obtain from the Consular Officer of the United States at the port of departure, a bill of health, to be made out in accordance with regulations to be made by the National Board of Health; and that no vessel unprovided with such a bill of health should be allowed to enter any United States' port under a heavy penalty.

This law was based on the theory that the only way to get a reliable bill of health is to have it given by an officer who is directly responsible to the Government of the country to which the vessel is bound, and that a Government is justified in thus indirectly requiring that the ships of other nations, as well as its own, shall be subject to inspection by its consular officers, if they desire admission to its ports. These are the essential principles of the quarantine system of Portugal. The law failed in its purpose, first, because it was practically impossible to prove that any one of the diseases defined as contagious and infectious for the purposes of this law—viz., small-pox, typhus fever, relapsing fever, yellow fever, cholera, and plague, existed at the port of departure at the exact time when the vessel sailed; and second, because it provided that the penalties arising under it should not be enforced until the law, and the regulations made under it, should have been officially promulgated for ten days at the port of departure. This official promulgation could not be made in a foreign country without the consent of the

authorities of that country; and this consent could not be obtained in the island of Cuba, which is the chief source of danger to the United States.

Under these circumstances, two courses were open to the United States if it desired to maintain the law. The first was, to annul the proviso requiring ten days official promulgation, &c., and to declare that the bill of health referred to should be required; either from all vessels coming from a foreign port, as is done by Portugal; or from all vessels coming from ports of certain countries, as is done by France and Spain; or from vessels coming from ports designated from time to time by the National Board of Health.

The second course was, instead of acting alone in the matter, to endeavour to obtain, by a conference between the nations most interested, an international system of prompt and reliable information as to the sanitary condition of ports and ships; which information should in part be carried by the ship itself, in the form of a bill of health, and in part, in the case of a presumably infected ship, precede it in the form of a telegram.

The second plan was tried first, and an International Sanitary Conference for this purpose, convened in Washington, in January of the present year. The scope of the conference was limited to the question of an international system of notification, because the United States would hardly become a party to any international sanitary organization which might interfere with the powers of the health authorities of the several States.

The questions laid before the conference were as follows:—

"A. The establishment of a reliable and satisfactory international system of notification as to the existence of contagious and infectious diseases, more especially cholera and yellow fever.

"B. The establishment of a uniform and satisfactory system of bills of health, the statements in which shall be trustworthy as to the sanitary condition of the vessels at the time of sailing."

The discussion of these points would involve, among others, the following questions:—

1.—Who should be the certifying officer or authority as to the sanitary condition of ports and places, and of vessels?

2.—How can the certifying authority obtain trustworthy information as to the actual sanitary condition of ports and places, and as to the presence of contagious and infectious diseases?

3.—When yellow fever or cholera exists at or in the vicinity of a port or place, what examination should be made of a vessel sailing therefrom to secure a trustworthy knowledge of her sanitary condition?

4.—To what extent, and under what conditions, should a clean bill of health be considered as affording satisfactory evidence that the vessel is free from danger of conveying infectious disease?

5.—In what way can trustworthy information be obtained from ports or places, in countries which have imperfect or unsafe quarantine and sanitary regulations, and which may be unwilling or unable to adhere to the proposed international system?

6.—Whether a schedule of graduated penalties could be fixed, to be exacted from vessels for various offences arising under the proposed international system?

The conclusions of the conference are in several respects not such as had been hoped by those who proposed it. On the one hand, it fell into the error which has to a great extent made the conclusions for previous conferences practically useless,

by trying to do too much, and to go into too much detail; and on the other hand, the answers which it gave to the questions proposed cannot be considered as satisfactory, since, in endeavouring to preserve a nominal and unnecessary independence, it has certainly failed to provide for satisfactory co-operation between the nations. The conclusions of the conference are herewith appended.

Upon these, I have only to remark that the provisions will almost surely never be carried out; and that, if they were carried out, the weekly statistical bulletins would not be trustworthy, would appear too late to be of much service, and would be very far from sufficient as a means of furnishing information as to the actual sanitary condition of suspected ports; while the proposed international sanitary agencies could certainly be of very little use. I must concur with Dr. Mouat, that an international sanitary body is not at all a fit and proper body to undertake an investigation into the phenomena of epidemic diseases in general, and of such epidemic diseases as cholera or yellow fever in particular. There is not only no guarantee that the best men would be selected by each Government as representatives, nor that the best men, if selected, would undertake the work; but the probabilities are all the other way. It seems much better that each country should collect its own facts and figures, adjust them, and formulate the conclusions, and pay its own expenses; for it is only by the earnest work in the school-room, hospital ward, and in the pathological and chemical laboratories, of many men in all countries, that any scientific solution of these great, and at present mysterious problems, will ever be arrived at."*

The conference did, however, effect one marked improvement, which had been suggested by the National Board of Health, and that was the International Bill of Health. This bill of health is given upon a different principle from any that have been heretofore proposed. All other bills of health close with the time-honoured formula that this vessel leaves in free pratique or quarantine, as the case may be. This would imply that the officer who certifies to the bill of health at the port of departure had some authority to state whether the vessel should receive free admission at the port of entry, which is, of course, not the case. The phrase has always been considered an advisory one, and its advice has come to be of very little importance. Clean bills of health are commonly given from infected ports to foul and dangerous vessels. I have heard one of our most experienced quarantine officers say that he had never in his life seen a foul bill of health. The bill of health proposed by the conference is a simple, straightforward description of the circumstances under which a vessel clears from a port, without saying anything about free pratique or quarantine.

If such a bill of health were given by the officers of the Government to which the vessel sails—and who would, therefore, be responsible to the parties most interested as to the correctness of the statements made in it—it would be not only a powerful means for preventing the importation of contagious or infectious diseases, but for securing a proper sanitary condition of vessels and their contents. There is a very great difference in the danger to be apprehended from vessels of different kinds coming from infected ports. Take, for instance, a clean vessel, having no decaying wood in it, coming from the harbour of Havana. If it is made certain to the authorities of the port of entry, that this vessel, while at Havana did not approach the wharves or docks, but lay in mid-harbour; that its

* Dr. F. J. Mouat, *Trans. of Epidemiological Society, London*, vol. vi. part 1, p. 113, note on Med. Statistics, &c.

crew were not allowed on shore at night, or that they were protected by having had the fever, it is evident that such a vessel might be treated very differently from a dirty ship which had laid along the wharves in the vicinity of an infected part of the city, even although no cases of disease may have occurred on either vessel during the voyage.

I will conclude by giving what, in my opinion, is the proper basis for an international agreement upon this subject, viz. :—

1st. Each Government should secure prompt and reliable information as to the existence of cholera, yellow fever, and plague, within its boundaries, especially in the seaports. Precisely how this is to be obtained, or in what form the information is to be published, each Government should be left to judge for itself. Of course uniformity is desirable; and of course every civilized Government should endeavour to secure much more information with regard to the health of its people than I have specified; but none of those points should form any part of an international agreement.

2nd. Each Government should communicate this information to the other parties to the agreement, and especially to their consuls and consular agents at the seaports. In case of the occurrence of any of the above-named diseases, the communication should be made by telegraph with the least possible delay practicable. There should be nothing like a central sanitary body or central office, to which each Government is to report, and which is then to distribute the information. Such an organization will simply cause delays and expense, and be in every respect fifth wheel to the coach.

3rd. When bills of health are required by any of the contracting Governments, the bills should be signed by its own agents at the port of departure; and these agents should have the right to make such inspection of the ships as is necessary to enable them to certify to the bills of health.

If any useful result whatever is to be obtained from an international agreement, this proposition is absolutely essential.

No Government should be compelled to require bills of health; let each Government do as it pleases in this respect; and if it does require them, let it pay the expenses of obtaining them; but the certificates of the agents of a given country or port will never be satisfactory to the health authorities of ports of another country.

4th. The agents charged with the duty of signing the bill of health should also have the privilege, in case cholera, yellow fever, or plague exists at the port of departure, of making use of the telegraph to notify their respective Governments of the departure of ships from such infected ports.

5th. The bill of health should be of the form approved by the Washington International Sanitary Conference, It should be neither a clean bill of health, nor a foul bill, but a certificate in detail as to the sanitary condition of the port and of the ship.

If these five points could be included in a treaty between the great commercial nations of the earth, it would, in my judgment, result not merely in, to a great extent, preventing the importation of cholera and yellow fever by means of ships, but it would secure clean dry ships for our ocean commerce, which, for a series of years, is probably a much more important matter.

APPENDIX.

Resolutions adopted by the International Sanitary Conference of Washington, drawn up by a Committee composed of Messrs. Outrey, de Bille, Dr. Cervera, Lowndes, Dr. Turner, Prince de Camporeale, Bartholomei, and Rustem Effendi, Secretary.

RESOLUTIONS.

I.

Each Government shall have such an organized internal service as will enable it to be regularly informed of the state of the public health throughout the whole of its territory.

Yeas: Argentine Republic, Austria-Hungary, Belgium, Denmark, Spain, France, Hayti, Italy, Mexico, The Netherlands, Portugal, Russia.—12.

Nays: Chili, United States, Great Britain, Sweden and Norway, Turkey.—5.

II.

Each Government shall publish a weekly bulletin of the statistics of mortality in its principal cities and ports, and shall give such bulletins the largest possible publicity.

Yeas: Argentine Republic, Austria-Hungary, Belgium, Denmark, Spain, France, Hayti, Italy, Mexico, The Netherlands, Portugal, Russia.—12.

Nays: Chili, United States, Great Britain, Sweden and Norway, Turkey.—5.

III.

In the interest of the public health, the sanitary authorities of the countries represented in this Conference are authorized to communicate directly with each other, in order to keep themselves informed of all important facts which may come to their knowledge; but nothing herein contained shall relieve them from the duty of furnishing, at the same time, to the consuls in their respective jurisdictions, the information they are required to give them.

Yeas: Belgium, Chili, Denmark, Spain, United States, France, Hayti, Mexico, Russia, Sweden and Norway, Turkey.—11.

Nays: Germany, Austria-Hungary, Italy, The Netherlands, Portugal, Argentine Republic.—6.

IV.

A centralized international system of sanitary notification being deemed indispensable to the successful carrying out of measures for preventing the introduction of disease, it is advisable to create international organizations to be charged with the duty of collecting information in regard to the outbreak, spread, and disappearance of cholera, pest, yellow fever, &c., and of conveying such information to the parties interested.

A draft of a Convention to carry out this object has been voted by the Conference in the following manner. (Annex I.)

Yeas: Germany, Austria-Hungary, Belgium, Chili, Denmark, Spain, Hayti, Italy, Netherlands, Portugal, Argentine Republic, Russia, Turkey.—13.

Nays: United States, France, Japan.—3.

Abstained from voting: Mexico, Sweden and Norway.—2.

V.

Bills of health shall be in the form hereto annexed. (Annex II.)

Yeas: Belgium, Chili, Denmark, Spain, United States, France, Hayti, Italy, Mexico, The Netherlands, Portugal, Turkey.—12.

Nays: Germany, Argentine Republic, Austria-Hnugary, China, Russia, Sweden and Norway.—6.

VI.

Bills of health shall be delivered at the port of departure by the responsible sanitary agent of the central Government. The consul of the country of destination shall have the right to be present at the examination of ships made by the representative of the territorial Government, under such rules as may be laid down by international agreement or treaty, and the authority to authenticate the bill of health and to add thereon such remarks as he may deem necessary.

Yeas: Germany, Argentine Republic, Austria-Hungary, Chili, Denmark, Spain, Hayti, Italy, Mexico, The Netherlands, Portugal.—11.

Nays: Belgium, China, United States, France, Russia, Sweden and Norway, Turkey.—7.

VII.

Bills of health granted in compliance with international rules should be issued gratis.

Yeas: Germany, Austria-Hungary, Belgium, Chili, Denmark, United States, France, Hayti, The Netherlands.—9.

Nays: China, Italy, Russia, Sweden and Norway, Turkey.—5.

Abstained from voting: Argentine Republic, Spain, Mexico, Portugal.—4.

VIII.

A Temporary and Scientific Sanitary Commission shall be created by the nation most directly interested in protecting themselves against yellow fever, and by such others as may wish to take part in this arrangement, to be charged with the duty of studying all matters pertaining to the origin, development, and propagation of that disease.

A draft of a Convention to carry out this object has been voted by the Conference in the following manner. (Annex III.)

Yeas: Germany, Austria-Hungary, Belgium, Chili, Denmark, Spain, United States, France, Hayti, Mexico, The Netherlands, Portugal, Argentine Republic, Turkey.—14.

Abstained from voting: Italy, Japan, Russia, Sweden and Norway.—4.

ANNEX I.

The draft of a Convention mentioned in the Resolution IV. is as follows:

ART. I.—There shall be established in Vienna and Havana a permanent International Sanitary Agency of Notification. The respective Governments shall agree among themselves as to the formation of those agencies.

ART. II.—It will be the duty of the agency at Vienna to gather sanitary information from Europe, Asia, and Africa. The agency at Havana will extend its jurisdiction to the American continent and the islands belonging geographically thereto. This system to be subject to such modifications as may be rendered necessary by the state of telegraphic communication.

ART. III.—The contracting Governments shall have the right to establish, if necessary, a third agency in Asia.

ART. IV.—The Governments agreeing to this system of notification will send their sanitary reports to that agency in whose jurisdiction it is. Every agency shall send its notifications to those Governments sending sanitary bulletins. The different agencies will exchange the information they receive among themselves, in order that it may be made known to the respective countries belonging to the jurisdiction of each.

ART. V.—There shall be no exceptions to this system, save in cases of extreme emergency.

In such cases, however, a Government may enter into communication with an agency to whose jurisdiction it does not belong.

ART. VI.—In cases of doubt as to the correctness of the bulletins received, the agency is authorized to enter into communication with the respective Governments, who will be obliged to furnish as soon as possible the information asked.

ART. VII.—In those countries where international sanitary boards exist the agencies shall establish communication with them.

ART. VIII.—In those countries which have not a perfect sanitary organization, or which have not adopted the conclusions of this Conference, the consuls of the contracting powers shall meet in an International Sanitary Council in order to give to the said agencies the information which cannot be obtained from the local authorities.

ART. IX.—The Governments of Spain and Austria-Hungary shall fix the annual budget of expenses, which they will submit to the participating Governments.

ART. X.—The division of expenses between the different Governments shall be as follows:—

Half of the expense will be divided in proportion to the number of population, and the other half in proportion to the amount of tonnage of the merchant marine, combined with the commercial maritime value of every country.

ART. XI.—The Governments of Spain and Austro-Hungary shall submit at the end of every fiscal year the final accounts to every one of the interested Governments.

ART. XII.—The present Convention is concluded for the space of ten years. Any Government, however, is at liberty to renounce the Convention after three years. The right is reserved to make such changes as any one of the Governments taking part shall designate.

ANNEX II.

INTERNATIONAL BILL OF HEALTH.

I, ... (the person charged to deliver the bill), at the port of ....., do hereby state that the vessel herein named clears from this port under the following circumstances:

Name of vessel:.....	Nature (vessel-of-war, ship, schooner, &c.): .....
Tonnage:.....	.....
Apartments for passengers, No.	Guns:.....
Destination:.....	Where last from:.....
Name of medical officer (if any):.....	Name of captain:.....
Total number of passengers: 1st cabin,....	Total number of crew:.....
2nd cabin,.....; steerage,.....	Cargo:.....

Vessel.

1. Sanitary condition of vessel (before and after reception of cargo, with note of any decayed wood). Note disinfection of vessel: .....
2. Sanitary condition of cargo: .....
3. Sanitary condition of crew: .....
4. Sanitary condition of passengers: .....
5. Sanitary condition of clothing, food, water, air-space, and ventilation: .....

Port.

1. Sanitary condition of port and adjacent country—
   (*a.*) Prevailing disease (if any):
   (*b.*) Number of cases of, and deaths from, yellow fever, Asiatic cholera, plague, small-pox, or typhus fever during the week preceding—

Number of cases of—	Number of deaths from—
Yellow fever:.....	Yellow fever:.....
Asiatic cholera:.....	Asiatic cholera:.....
Plague:.....	Plague:.....
Small-pox:.....	Small-pox:.....
Typhus fever:.....	Typhus fever:.....

(*c.*) Population according to the last census: .....

(*d.*) Total deaths from all causes during the preceding month:.....

2. Any circumstances affecting the public health existing in the port of departure to be here stated: .....

I CERTIFY that the foregoing statements are made by ....., who has personally inspected said vessel; that I am satisfied that the said statements are correct; and I do further certify that the said vessel leaves this port, bound for..... in.....

IN WITNESS WHEREOF I have hereunto set my hand and the seal of office, at the port of....., this.. ..... day of....., 188...,

.....o'clock.

.....

[SEAL.] [Signature.]

ANNEX III.

ARTICLE I.—A temporary Scientific Sanitary Commission will be established by the nations most deeply interested to protect themselves against yellow fever, and also by such nations as will adhere to these proposals.

ART. II.—The duties of this commission will be—

A. The study and ascertainment of the principal and permanent centres of the morbigene germs of the disease.

B. The conditions which favour its extension, and also the etiology and circumstances which aid its propagation in the respective centres, and its diffusion to other countries.

C. The means to be employed to circumscribe more and more its effects, or to eradicate the disease entirely from the regions in which it originates, and from the countries lately attacked.

D. How to prevent the transmission by vessels.

E. The best and most practical method of disinfection of ships, their cargoes and passengers.

F. Also everything relating to the prophylaxis and treatment of the disease.

ART. III.—The countries which desire to concur in the organization of this scientific commission will agree between themselves as to the instructions to be given to their delegates in order to facilitate their labours.

ART. IV.—After having studied on the spot the different questions submitted to its investigations, this commission shall present a collective report indicating the most practical means to attain the end proposed.

Ueber die nöthigen Maassregeln um die Ausbreitung der Pest zu verhindern.

Dr. ALADÁR v. RÓZSAHEGYI, Buda-Pesth.

Die Pest, welche in vergangenen Jahrhunderten ganz Europa zu überziehen pflegte, welche vor kaum 40 Jahren vom europäischen Boden scheinbar endgiltig verschwand, doch in der jüngsten Zeit, vor 3 Jahren die Grenzen unseres Welttheiles neuerdings überschritt—diese Krankheit, welche in einzelnen Ländern des Orients und an der Nordküste Afrikas auch in unseren Tagen das menschliche Geschlecht verheert und durch ihr wiederholtes Auftauchen Schrecken erregt, die Gesundheit der civilisirten europäischen Völkerfamilie bedroht, und diese in ihrer

materiellen Wohlfahrt arge schädigt, ist in ihren ätiologischen Momenten derzeit noch nicht so weit aufgeklärt, dass man hoffen könnte unser Endziel : die gänzliche Ausrottung dieser Seuche, sei unmittelbar, leicht und rasch zu erreichen. Desshalb müssen wir uns heute darauf beschränken diejenigen Maassnahmen festzustellen, mit deren Hülfe es zu erhoffen ist, dass, erstens die Pest überhaupt vom europäischen Boden ferngehalten, immer mehr und mehr auf ihre Brutstätte zurückgedrängt und zum Schlusse hier vernichtet werden kann, zweitens, dass Europa selbst bei einer unerwarteten Invasion der Pest, dieser keinen günstigen Boden zum Festsetzen und zur Verbreitung bieten wird.

Beide Gruppen von Maassnahmen haben sich auf die Verbreitungsart der Pest zu stützen, setzen also eine Kenntniss der letzteren voraus. Man darf wohl mit Recht behaupten, dass zur Erklärung dieser Verbreitungsart nur diejenigen Erscheinungen zuzulassen sind, welche die Pest in unseren Tagen, d. h. in den seit ihrem erneuten Auftauchen vor 25 Jahren wiederholt ausgebrochenen Epidemien der Beobachtung darbot.

Diesen Erscheinungen nach, giebt es Localitäten, wo die Pest, autochthon entsteht; als solche sind uns bekannt: gewisse Districte im Irak-Arabi (Hindié, Dhagara), in persisch Kurdistan, die Provinz Benghazi in Nordafrika und das Gebirgsland von Assyr in Westarabien. Alle übrigen Orte, wo die Pest im letzten Vierteljahrhundert ausgebrochen war, erhielten sie durch Verschleppung.—Das Mittel der Verbreitung ist ausschliesslich der menschliche Verkehr, und zwar vor Allem und ganz vorwiegend der umherziehende Mensch selbst, dann, menschliche Leichen; von Kleidungsstücken ist uns nur eine auf kurze Distanzen beschränkte Ansteckungsfähigkeit bekannt geworden; für die Verschleppung durch Waaren liefern die genannten Epidemien kein einziges Beispiel.—Die Verbreitungswege sind überwiegend auf dem Festlande situirt; für die Verschleppung durch die Seeschifffahrt liegt aber auch ein Beispiel vor (Benghazi).—Sowohl in den Orten mit autochthoner, als in denjenigen mit verschleppter Pest bedarf es aber zur epidemischen Ausbreitung der Krankheit einer unverkennbaren örtlichen, zeitlichen und individuellen Disposition.

Bei der Ausschliesslichkeit des menschlichen Verkehres als Verbreiter der Pest wird unter den Schutzmaassregeln gegen die Ausbreitung der Krankheit die Beschränkung des Verkehres zwischen verseuchten und gesunden Localitäten die Hauptrolle spielen.—Auf europäischem Territorium sind Landsperren (Militärkordone) wegen der langgestreckten Grenzen und der Lebhaftigkeit des Verkehres unausführbar. Seequarantainen hingegen sind beizubehalten; die Dauer aber darf auf 10 Tage (mit Einrechnung der Ueberfahrt) herabgesetzt werden, da kein Fall mit längerer Incubation bekannt ist. Kleidungsstücke sind überhaupt auszuschliessen; Waaren können nicht mit Grund beanstandet und zur Desinfection verhalten werden.—In den aussereuropäischen verseuchten Gebieten sind die Sperrmaassregeln (Kordone und Quarantainen) mit der äussersten Strenge zu handhaben und werden dann auch von durchschlagendem Erfolg begleitet sein.

Neben diesem, den seuchenfreien Gebieten zu gewährenden Schutz ist auch für die Verminderung der Morbidität und Mortalität in den verseuchten Orten Sorge zu tragen. Die ärztliche Behandlung weist bisher keine nennenswerthen Erfolge um die Pestkranken auf; hingegen ersehe ich, dass die Evacuirung der verseuchten Ortschaften, die Zerstreuung der Bevölkerung in der Umgebung in mehreren Epidemien (1871 Kurdistan, 1874 Benghazi, 1881 Nedscheff) ein rasches Erlöschen zur Folge hatte, also in der Zukunft vor Allem anzuwenden sein wird.

Zum Schlusse handelt es sich darum erneuerte Pestausbrüche zu verhindern.

Die zu diesem Zwecke übliche Desinfection entbehrt überhaupt eines nachgewiesenen Erfolges, und kann nur dann als erfolgreich gemuthmasst werden, wenn sie in der Vernichtung (Verbrennung) der ganzen Umgebung der Pestkranken besteht; als ganz sicheres Schutzmittel aber kann nur die Auswanderung der überlebenden Bevölkerung auf ein erfahrungsgemäss immunes Terrain vorausgesetzt werden.

In dieser Verhinderung neuer Ausbrüche liegt der Schlüssel zur ganzen Pestfrage. Ist sie auch derzeit noch unerreicht, so hat sich doch das Bestreben zunächst darauf zu richten, dass ein neuer Ausbruch rechtzeitig erkannt und eingedämmt werde. Und eben diesbezüglich ist das in den Pestheerden bestehende System ganz und gar unzulänglich. Keine einzige Pestepidemie wurde in den letzten Decennien rechtzeitig erkannt; keine konnte rechtzeitig, nach den ersten Fällen abgewendet werden.—Es ist vor Allem anzuerkennen, dass die ersten Fälle, welche jeder Epidemie vorausgehen, gutartige Bubonen sind, und dann dafür Sorge zu tragen, dass sie auch gleich erkannt, und dass prophylactische Maassnahmen auch sogleich ausgeführt werden.

Bei der Gemeinsamkeit des gesundheitlichen Interesses aller Völker in dieser Frage, empfiehlt sich zu letzterem Zwecke das folgende System: Die Schutzmaassregeln sind auf internationaler Basis zu organisiren und haben in einem nach den Heerden der Pest verlegten Beobachtungs- und Controllsdienste zu bestehen. Jede als Pestheerd bekannte Gegend habe in ihrem Verwaltungscentrum eine ständige Commission, deren ärztliche Experten die verdächtigen Orte fortwährend bereisen, frische Pestfälle der Commission melden, die von dieser den localen Behörden empfohlenen Maassnahmen überwachen. Die Berichte dieser verschiedenen Commissionen laufen in den Händen eines, bei einem der europäischen Regierungen zu creirenden Centralorganes zusammen, welches lediglich ihre Vervielfältigung und ihre Vertheilung an die interessirten Regierungen besorgt.

Bei dieser Organisation ist zu erhoffen, dass die zur Sicherung der Immunität Europa's gegen die Pest hier in Ausführung zu bringenden Maassregeln (als Reinhaltung der menschlichen Wohnstätten, von Luft, Boden und Wasser in ihnen, dann Verbesserung der Erwerbs-, der Wohnungs-, Nahrungs- und Kleidungsverhältnisse)—so dringlich sie im übrigen wegen anderer Krankheiten zu fordern sind—keine Gelegenheit finden werden, gegen die nach Europa verschleppte Pest je in Wirksamkeit zu treten.

On the Measures to be Pursued to Prevent the Communication of Yellow Fever from Country to Country, or within any one Country.

Inspector-General Robert Lawson, London.

There being no re-agent for the yellow fever poison known at present, except the human body, or those of animals subject to the disease, the occurrence of yellow fever among persons in any given locality merely proves that the poison of the disease was active there. The outbreak of the disease may have been preceded by the arrival of cases from elsewhere, and those first attacked having been in communication with them; but unless the influence of local causes can be excluded,

this does not justify the conclusion that the disease was derived from the imported cases.

In every instance in which the action of local causes could be excluded, it has been found that extensive and continued importations of yellow fever cases failed to communicate the disease to those who visited them, however frequent or prolonged their exposure to the emanations from the sick, or their discharge.

Yellow fever, then, being closely connected with locality, can such places acquire the power of giving off the emanations which cause the disease, without the previous introduction of the poison, either by persons or shipping, or articles they have brought from some other point where the disease was already prevailing? There are many facts to show that localities have acquired this power without its being communicated from other places where the disease was already in existence, by the means indicated; and this having been established for some localities, it is clear that it may apply to any one where the disease arises; and the possibility of this being so must be excluded before another mode of origin can be admitted.

The localities which acquire the power of emitting this source of yellow fever may be either places on shore, or ships. The former always present a certain amount of moisture, either on the surface or some little distance below it, in which case it is covered by an open porous soil; the latter have always had a quantity of fetid mud in their bilge; and if this be not removed, and the ship thoroughly cleansed, susceptible persons exposed to the emanations from it in warm weather, may continue to be attacked by yellow fever for months, or even years after it first displayed the power of generating that disease, and at points far removed from that where it was originally acquired.

The localities which emit yellow fever poison on land are not unfrequently small, and distinctly circumscribed; and during the night, when the emanations from these are not rapidly diffused upwards, they may be conveyed a considerable distance by currents of air sufficiently concentrated to cause the disease among persons exposed to them, though those out of the direct course of the current remain unaffected. The same has been observed in connection with ships: and the question naturally rises whether vessels with a foul bilge actively emitting such emanations—if in the vicinity of the shore—could impart to a locality there suitable for the reproduction of the material, the germ from which it proceeds. Though often alleged, there is nothing worthy of the name of scientific investigation on this point.

Can clothes or other porous materials, exposed to the emanations from a locality emitting yellow fever poison, take up and retain a sufficient quantity of the material to cause the disease in persons at a distance who first open them out? Cases of this description are often mentioned, but, as with personal communication of the disease, those who put them forth seldom or never think of excluding the possibility of local causes having excited it; the facts, therefore, do not fairly establish the conclusion.

From the position here laid down it follows that:—

Yellow fever can pass from country to country without the intervention of man, and he can neither arrest its progress, nor indicate what countries it will affect.

A ship with a source of yellow fever in her, may retain the power of exciting the disease in those who come within range of the emanations from this source for months or years, and may in the meantime have gone far from where she was originally infected; but the sick from her may be landed and treated without risk to their attendants or the population generally.

It has not been established that a vessel with a source of yellow fever in her, can impart the germ of the disease to a locality on shore suitable for its development, near which she may lie, and so produce a shore epidemic.

Persons who have contracted yellow fever in one locality may proceed to another, where the disease runs its course without any of those around them being affected, provided there be not a cause for it in operation at the spot.

To prevent the formation of spots capable of emitting the poison, thorough drainage of the ground in the neighbourhood of occupied buildings is required, covered drains and sewers should be thoroughly flushed and disinfected, bush removed from the vicinity, and there should be no garden ground which is frequently watered near dwellings.

When yellow fever appears, it is prudent to remove the population from the locality, if this can be done; if not, then they should sleep in apartments as much raised from the ground as can be obtained; and every night, at sunset, the windows looking towards any unhealthy ground should be shut, and kept so until after sunrise next morning.

When a ship arrives with a source of yellow fever in her, she should be anchored where the emanations from her may be diffused without injury to the surrounding population. The hatches should be opened, and means taken to disinfect the bilge, and the crew removed from her. It is not clear whether the clothing and bedding of the crew can convey the poison in sufficient quantity to prove injurious to others, but there can be no harm in opening the chests, and exposing the contents fairly to the air, and in washing such as require it. The necessity for burning the clothing and bedding of persons who have died of yellow fever is not justified by experience.

DISCUSSION.

Surg.-Gen. Dr. JOHN MURRAY, London: said his opinions on cholera coincided closely with those of Dr. Billings. There is now no question as to the transmissibility of the disease by human intercourse, as was clearly demonstrated by tracing it step by step with the returning pilgrims after the Hurdward Fair, of 1867, when the epidemic spread with them for 300 miles in the N.W. Provinces, and for 1,000 miles in the direction of the Punjaub and Scinde. The ravages of the disease have attracted the attention of Governments in all parts of the world, and quarantine regulations are the result. Now we know, from experience in India, that absolute protection is unattainable; but we also know that relative exemption has been secured by restrictive measures in the military stations and jails of India. In regard to quarantine, he entirely agreed with Dr. Billings as to the impossibility of guarding every avenue to the admission of the disease, but he would not, therefore, cast aside all precautionary measures. The acceleration of transit by steamers and railways renders it very possible that cholera in its incipient form may be unconsciously conveyed many hundred miles; but there is no reason for admitting recognizable cases of diarrhœa and collapse into a country free from the disease. When such a risk is apparent, quarantine regulations are advisable to secure the examination of suspected crews and passengers. The isolation of cholera cases could then be effected, and the disease stamped out as approved by Dr. Billings.

Insp.-Gen. Dr. A. FAUVEL, Paris: said he was hardly prepared to speak *ex abrupto* on the questions which had been so ably dealt with that morning,

but he felt bound to obey the President's call. He had, indeed, much that he could wish to say, and he found difficulty in summarizing all in a few words. His opinion did not materially differ from that expressed by Drs. Billings and Lawson as to the conditions which favour the spread of yellow-fever and cholera, but he thought they spoke too generally on subjects which were true only for certain points of territory in America and Europe. That which was true for America and England as regards the prophylactic measures to be adopted against the importation of these diseases, was certainly not so, to the same degree, for all parts of Europe. Thus, yellow-fever, for the propagation of which certain conditions of topography, temperature, and local hygiene were necessary, did not meet in England with circumstances as favourable for its development as obtained in France, Spain, Portugal, &c. Thus, the measures needed to cope with yellow-fever in England were but trivial; they became of more importance in France, where the climate was more favourable to its invasions; and they must be even more rigorous still in Spain and Portugal, where the disease met with climatic and hygienic conditions which favoured its propagation. It was only necessary, in this connection, to recall the epidemics of 1870 at Barcelona, of 1858 at Lisbon, and of 1861 at St. Nazaire, in France. Thus, it was not possible to formulate identical rules as regards yellow fever, which should be applicable to all countries, whether as to its propagation or to the necessary prophylatic measures. According to opinions entitled to much respect, yellow fever was not communicable from the sick to the healthy; that might be true in England, but it certainly did not hold good according to observations made in France, at St. Nazaire, or in Portugal, or in Spain. In England, the measures against the propagation of yellow fever took no cognizance of its spread from the sick, and still less from the hitherto healthy from an infected vessel. There was, also, properly speaking, no quarantine against the invasion of the disease. He was far from blaming this practice, for it was in accordance with the slight tendency of yellow-fever to propagate itself there; but he maintained that it would be dangerous in different degrees, according as it was applied to the centre of Europe, France, Portugal and Spain. It could not, therefore, be pretended that these countries ought to imitate England in this matter. Besides which, prophylactic measures proportioned to the danger to be met, were applied to the several British possessions in the Mediterranean, at Gibraltar, and at Malta, where more severe quarantine measures were adopted against importations from countries where yellow-fever and other pestilential diseases prevailed. These measures, of a needless severity, were doubtless intended to meet a considerable danger, but they also met a commercial interest, which required that the English markets should remain in free communication with those ports with which they had commercial relations. It was the same with the French at Marseilles, where the sanitary interests, which were of first importance, were not opposed to the commercial interests, as is the case in the North of Europe. When he spoke of severe prophylactic measures, he meant measures which were practicable, and which bore a proper relation to the end to be obtained. In a word, the remedy must not be worse than the disease. On the Western coast of France the practice in this matter was very similar to that of England, because the interests and the conditions there were also similar. It was, however, different at our southern ports, where the danger was great, and the prophylactic measures must be more severe. Passing by the question of plague, which disease appeared to him to afford little ground of fear for Europe, owing to the measures adopted by the countries where it reigned, he now came to deal with cholera, which incessantly threatened us. We were all agreed that cholera was not dependent for its propagation upon conditions of temperature and

climate in the same manner as yellow-fever was; and that, when it was imported from the East, no measures could stay the invading march of the epidemic, where conditions favourable to its development were found. Hence the uselessness of such measures as *cordons sanitaires* between the several European States. But still, it had been recognized in France that it was necessary to make an exception with regard to maritime relations between infected and healthy countries, especially where these countries were wide apart; for experience had taught us that an infected ship could impart cholera into a port situated at such a distance as would forbid importation by land. Thus, when in 1873 infected vessels imported cholera from Naples into the Lazaret at Marseilles, the town remained unaffected, although communication by land with Italy remained entirely free.

As far back as 1866, the speaker proposed to the Conference assembled at Constantinople a system for the preservation of Europe as a whole. This system, founded on a knowledge of the routes by which cholera entered Europe, consisted in quarantine measures to stay its march at certain convenient spots. One road which the disease followed was to the north, along the shore of the Caspian; the other, to the south, was by Egypt and the Red Sea. The latter route was especially that by which the disease was imported by ship from India, used as it was by the Mecca pilgrims. The conference adopted this plan Unfortunately, however, the 1865 epidemic was not yet stamped out in Russia, and following on its recrudescence there in 1869, part of Europe was again invaded. I was, however, different as regards the Red Sea route. There, a barrier, although incomplete and badly organized, had on several occasions since 1867 preserved Egypt, and consequently the south of Europe, from cholera in its passage from India; as for example in 1872 and 1878. If fresh efforts were made to arrive at a general understanding on this subject by the several European Governments, the object could not be reached unless count was taken of the varying interests and of the different measures demanded by those interests.

Dr. Carlo Cuturi, Pisa: in criticizing one portion of Dr. Fauvel's speech, quoted opinions expressed by him on the subject at the National Congress held at Pisa, 1878, and the conclusions arrived at by him, and unanimously adopted by the State Medicine Section; these conclusions being in harmony with those maintained at the International Congress of Hygiene held in Paris at about the same date.

Dr. F. de Chaumont, Netley: wished to say a few words with reference to the communicability of cholera, referred to by Dr. Murray, especially as regards the epidemic of Hurdwar. The epidemic, which took place in 1867, was considered by Dr. J. Cunningham as a proof of water communication or personal communication: but by 1879 he had changed his opinion, but admits that, "unfortunately," cholera again broke out in the same way. The sanitary department in India is in the hands of men who disbelieve in personal communication, or communication through water of cholera. Dr. Douglas Cunningham states, at the end of his topograpical report in Calcutta, that communicability of disease is demonstrable in small-pox, but not in cholera, and supports the local theory of Pettenkofer. It seems, however, impossible to look at the experience of India itself without coming to the conclusion that the communicability of cholera through water is possible.

Dr. M. T. Colan, London: having given details as to an outbreak of yellow fever on board H.M.S. *Boxer*, in 1879, said that the origin of the disease in and

from certain influences in a locality, admitted of no doubt; but that it may also be acquired from the body of an infected person—as indeed was, he thought, the case with himself—was also a matter of fact. But the subject under discussion at present was the diffusion of infectious disease. He himself organized a yellow fever hospital, at the R.N. Hospital, Port Royal, Jamaica, where he had the sick isolated and quarantined, and attended by coloured nurses. Every article connected with the sick was disinfected by either carbolic acid in solution, boiling water, or by heat in an oven, where also carbolic acid rose in vapour.

Dr. ACLAND, Oxford: spoke in support of the main practical conclusions of Dr. Billings and of Dr. Fauvel. The former pointed out that it was the duty of each Government to collect the most precise information on its own borders as to the existence of epidemic diseases, and immediately to communicate the same to other countries. Dr. Fauvel's statesmanlike observations explained the reason of this. Each country was able to know with precision the existence of a disease—and each country ought to be able to find, as far as science permits, the local causes of this disease—and, second, the best local means of prophylaxis. But the cause and the prophylaxis may vary in different countries, and also, as in France and the United States, in the borders of the same country. There were two countries which had special means of obtaining this information as to etiology and prophylaxis—namely, first, the United States, through the instrumentality of the several States which had such different climatic conditions, and the admirable central National Board of Health in Washington; and second, England, in conjunction with the admirable medical and statistical services of India. There was good reason to hope that the adoption of the principles of Dr. Billings and Dr. Fauvel, would lead, before long, to a thorough understanding between nations as to what was desirable and what was possible in the international prevention of disease, to the advantage of all humanity.

Dr. C. R. DRYSDALE, London: thought that it was now clearly made out that cholera was an infectious disease. In London, a few years ago, there were not wanting some distinguished observers—and among others he would cite Dr. Greenhow—who thought that cholera might arise occasionally sporadically, even in London. But he, Dr. Drysdale, after a long perusal of the evidence given by M. Fauvel, had come to the conclusion that Europe was always being infected with cholera germs, directly from India, by the the medium of pilgrims; and he considered that Europe owed a deep debt of gratitude to that gentleman, for having saved it from that terrible plague, by the strict quarantine that had been carried out of the Moslem pilgrims from India on the Red Sea.

Dr. F. H. BLAXALL, London: said, that with regard to detention in quarantine as a means to prevent the spread of cholera by ships, the following facts came under his own observation in the year 1851, when serving as an assistant surgeon on board H.M.S. *Caledonia*, of 120 guns and 1,000 men. Cholera was introduced into the island of Malta from Tunis, in May, 1851, and continued till the end of October following, causing upwards of 1,600 deaths. The Fleet, consisting of some four or five line-of-battle ships, was at anchor in Malta Harbour at the time. The first case of cholera in the Fleet occurred in the *Caledonia*, on the 15th of June; 4 other cases followed in that month, and the other ships were also invaded. In all, some 40 or 50 cases occurred before the 3rd of July, many of them proving fatal. On the 3rd of July the ships went to sea. Fresh cases continued to occur in the

Caledonia till the 8th—*i.e.*, five days after sailing: while in other of the ships fresh cases continued for about ten days; after this there was no fresh case, and the ships got absolutely free of cholera, continuing so for some two or three weeks. On the 30th of July the *Caledonia* re-entered Malta Harbour, but left there again on the 1st of August; and on the 4th, a fresh case of cholera appeared on board, followed by 6 others between the 4th and 14th; then no fresh attack till the *Caledonia*, for the second time, went into Malta Harbour on the 2nd of September, remaining till the 4th. On that day a fresh case appeared, followed by 4 others, but ten days after this, the ship being at sea, cholera had disappeared. To cite another ship: the *Queen*, 1,000 men, after leaving Malta Harbour on the 3rd of July, had between that date and the 13th of July, some 25 cases and 9 deaths; then no fresh case till after she re-entered Malta Harbour on the 14th of August; sailing thence again on the 16th, between the 19th and 23rd of August 13 cases occurred, and then a complete cessation of attack. But on the 4th of September the ship again went back to Malta, remaining there till the 8th, when she went to sea; and from that date to the 18th some 50 or more cases occurred, and 31 deaths. Thus it would be seen from the foregoing that the ships of the Fleet were invaded with cholera while lying in an infected port, and continued to suffer whilst so exposed, but on going to sea fresh attacks ceased ten days after leaving port. Twice afterwards the ships were subjected to similar exposure with like results, cholera appearing within a day or two of such exposure, and ceasing ten days from date of leaving and going to sea. These facts would seem to indicate that accurate knowledge of the state of health of the port any ship has left, and efficient medical inspection on her arrival in another port, were better calculated to prevent the spread of cholera by means of ships than the adoption of mere quarantine measures.

Measures by which to Prevent the Diffusion of Dengue.

Dr. James Christie, Glasgow.

The author dealt with the following points:—1.—The dengue epidemic of 1870-75, its prodromata, origin and diffusion.—Prior to the outbreak there had been a visitation of cholera at Zanzibar; the city was in a thoroughly bad sanitary condition, and within the city, and in its immediate precincts, there were about 20,000 imperfectly buried corpses. Dengue was not imported, but originated *de novo*. The principal diffusion was northward, towards the Red Sea ports, and from thence to British India, &c., &c. The native designation was "Ki-dinga."

2.—The dengue epidemic of 1824-28, read by the light of the epidemic of 1870-75.—The main facts were as follows:—In 1821, cholera was epidemic in East Africa. About 1822 or 1823 dengue was also epidemic, its native designation being "Ki-dinga." In 1824, the disease appeared first in the Bombay Presidency, afterwards in Rangoon, and finally in Calcutta. In 1827, it appeared in St Thomas, where it was called "Dandy fever;" but in the Spanish possessions it was termed "Dunga," and afterwards "Dengue." On the hypothesis that it originated at Zanzibar, the northern line of diffusion was identical with that of 1870; and the diffusion southward towards the West Indian islands, was possible by means of a slave-trade, which then existed, between the Portuguese settlements in the Mozambique Channel and Cuba and Brazil. "Dandy" is evidently a corruption of the original designation, "Dinga," the Spanish term, "Dunga," being but a slight alteration of the original.

3.—The epidemics of break-bone fever in the West Indian Islands and South America.—These epidemics seemed to bear the same relation to yellow fever epidemics that the former bear to cholera epidemics.

4.—The relationship between dengue and break-bone fever.—They seemed to be distinct varieties of the same disease, the infective virus of cholera in the one case, and of yellow fever in the other, being so modified by the products of human decomposition as to give rise severally to these diseases.

The only mode of dealing with epidemic dengue consisted in prevention—viz., by improved sanitary conditions in tropical districts, and by more careful modes of disposal of the dead, more especially of those dead from virulent epidemic diseases. During the epidemic isolation was impossible, and quarantine could be of little avail.

DISCUSSION.

Dr. EZRA M. HUNT, New York: desired to say a word on item 4, which spoke of the infective virus being so far modified by the products of human decomposition as to give rise to related diseases. The fact of the modification of infective material by cultivation seemed now quite certain. If so, it was rendered all the more probable that in Nature such modifications took place. Recent experiments go further, and give credence to the hypothesis that low forms of life, known as benign, suddenly become malign. Were we not now entering an epoch in which hybridization of disease, both accidental and experimental, was to be recognized as occurring, and in which we should have to study, not mere variations in type, but the production of allied and yet distinct diseases, from the special cultivation or pabulum which the original disease may receive, either from the locality or the person invaded?

Dr. NORMAN CHEVERS, London: stated he had witnessed the dengue epidemics of 1852 or 1853, and partly that of 1872, in Bengal, where cholera was constantly epidemic. It was not, he believed, considered there that it was at all connected with cholera. He regarded it as being wholly the result of a wave of morbid atmospheric influence. It did not appear communicable from man to man.

II.—MEASURES BY WHICH TO PREVENT THE DIFFUSION OF THE HOME CONTAGIA.

De la Fièvre Typhoïde au Havre; ses causes, et les précautions pour la prévenir.

Dr. GIBERT, Havre.

J'ai à présenter à la section une communication que je crois importante sur les causes d'une épidémie de fièvre typhoïde qui a ravagé le Havre, depuis le mois de Septembre, 1880, jusqu'au mois d'Avril, 1881.

Pour bien faire comprendre le sens et les conclusions de mon travail, je suis obligé d'entrer dans des détails sur la géologie et le régime des eaux du sol du Havre, et c'est pourquoi j'ai fait dresser les cartes qui sont sous vos yeux.

L'épidémie du Havre a fait 173 victimes (sur 92,000 habitants). Mais ce chiffre de 173 décès ne rend pas compte de l'importance de cette épidémie de fièvre typhoïde.

Il faut y ajouter plusieurs centaines de cas qui n'ont pas été, heureusement, suivis de mort. Leur nombre exact n'est pas connu, car dans notre pays les médecins ne sont pas obligés de faire connaître à l'autorité les cas de maladie contagieuse qui surviennent dans leur clientèle.

Causes de la maladie :—

Les causes banales, habituelles de la fièvre typhoïde ne peuvent pas être invoquées.

(*a.*) L'eau potable du Havre est à l'abri de tout soupçon et ne peut pas être contaminée.

(*b.*) La distribution du lait au Havre n'a jamais été la cause de la fièvre typhoïde, et d'ailleurs à l'époque où cette fièvre régnait au Havre elle n'existait pas dans les fermes d'où les habitants du Havre tirent leur lait.

(*c.*) Les vidanges qui pourraient être incriminées dans la basse ville, ne peuvent l'être dans la partie de la côte où la maladie a fait de grands ravages, car dans ce quartier les vidanges se font au moyen de tinettes mobiles qui ont été très-bien surveillées dès le commencement de la maladie.

(*d.*) La contagion n'est pas non plus la cause de l'extension de la fièvre typhoïde au Havre ; car dans les quartiers élevés, les maisons ne se touchent pas comme dans une rue; elles sont généralement isolées, et toutes ont un jardin. D'ailleurs dans ces quartiers la population y est généralement ou riche ou fort à l'aise et accoutumée à observer de bonnes règles d'hygiène—les maisons, qu'on appelle pavillons, sont habitées par des commerçants qui vont chaque jour à leurs affaires, et n'ont aucune relation habituelle les uns avec les autres. La contagion de maison à maison n'est pas possible.

Si donc les causes communes de cette épidémie nous font défaut, il faut nous adresser à un autre ordre de cause, celle qui a illustré Pettenkofer—celles des eaux souterraines—mais il ne s'agit pas seulement de la nappe d'eaux souterraines, il s'agit de l'état du sol imprégné de matières organiques et détrempé par l'eau qui ne s'écoule pas convenablement.

Il s'agit, pour moi, de faire une démonstration de cette cause, et j'ai besoin pour cela de recourir à une étude du Havre.

(*A.*) La ville du Havre est formée :—

1.—Par le terrain crétacé de la côte allant depuis le Havre jusqu'à Graville.

2.—Par le talus d'éboulement.

3.—Par une longue couche très-étendue, et occupant une grande partie du sol du Havre, appelée argilette, ou terre à renards.

4.—Par des alluvions maritimes et fluviaux, dont la coupe représente une succession de couches d'argile et de tourbe.

Il résulte de cette courte description que le sol du Havre n'est pas absorbant.

(*B.*)—Il existe dans toute l'étendue du Havre, et spécialement de la côte d'Engouville, une quantité de puisards appelés des "bétoires" dans le pays, le des boit-tout—dans lesquels chaque ménage jette toutes les eaux ménagères de la maison. Par conséquence, cette funeste habitude imprègne le sol d'une quantité de matières organiques.

(*C.*) La ville du Havre, pour assainir la ville, a construit un réseau d'égouts qui n'est pas achevé. Ce réseau est constitué par:—

1.—Une artère principale, le grand égout du boulevard de Strasbourg, qui entraîne à la mer toutes les matières d'égout. Cet égout a constitué un premier barrage et un obstacle à l'écartement des eaux de la côte, et dans une certaine mesure a produit un mauvais état sanitaire des rues située au nord de l'égout.

2.—Un second égout parallèle au premier, mais placé à mi-côte et qui traverse ute la partie du sol du Havre qui a un fond d'argile.

Il suit de la disposition fâcheuse de cet égout, que les eaux ne peuvent pas écouler librement, puisqu'elles rencontrent un vrai barrage.

Nous avons maintenant tous les éléments pour arriver à une conclusion; et oici comment les choses ont dû se passer, et, selon toute vraisemblance, se sont assées.

Les bétoires ont imprégné le sol de matières organiques qui ont saturé le sol.

Les eaux de la côte coulant sur un terrain non absorbant, et ne trouvant plus n facile écoulement vers la mer, grâce aux deux barrages constitués par les égouts ont j'ai parlé, ont rendu le sol particulièrement humide. Dès lors le sol humide, mprégné d'ailleurs de matières organiques par le fait des bétoires, est devenu le iège, la cause productive de miasmes délétères qui ont engendré la fièvre typhoïde.

Cette cause a d'ailleurs eu sa contre-épreuve d'une façon bien remarquable et attendue:

Le 13 Janvier, 1881, une épaisse couche de neige a couvert le Havre pendant lusieurs semaines. La maladie s'est arrêtée, et dans le quartier à mi-côte en parculier il n'a plus eu un seul cas de fièvre typhoïde—la neige avait mis fin aux manations du sol.

Si mes observations sont vraies, il en résultera pour le Havre la nécessité de ire des travaux d'assainissement tout spéciaux: de supprimer les bétoires et d'établir des drains qui facilitent l'écoulement des eaux vers la mer, en détruisant ainsi effet funeste du barrage des égouts.

Si en Angleterre de pareilles circonstances se sont rencontrées, je serai bien eureux qu'on me les fasse connaître.

On the Principles that should Guide us in Attempting to Prevent the Diffusion of Disease.

Prof. DE CHAUMONT, Netley.

In discussing the question of prevention of disease we have to consider two points z.: "The Origin of the Disease," and "its Diffusion." We desire to prevent sease arising in a locality, but once it has arisen we desire to prevent its ffusion. At present, our knowledge of the nature and origin of disease-poison is o vague and uncertain to enable us to separate those two points, and we must, erefore, be prepared to treat them together.

1.—Various divisions of diseases have been proposed, involving hypotheses of fferent kinds. It would be out of place to dwell upon these in detail, and therere I shall proceed to state the division which seems the most convenient—that with reference to preventible diseases. The first main division that suggests self is into:—

A.—Those which are *contagious* or *infectious*—that is, communicable from person person, or through the agency of persons.

B.—Those which are *preventible*, but which, so far as is known, are not communicable in the way described in group A.

Of this group B, the typical example may be taken to be *paroxysmal fevers*, a rm of disease arising from certain conditions of locality, but not capable of trans-

mission from individual to individual; although there is evidence of the poison of the disease being transported to other localities—for example, through the medium of drinking water.

Class A.—The contagious or infectious diseases may be divided into :—

(*a.*) Those which are *directly* or *immediately* contagious—that is, those in which the disease-poison propagates and multiplies in the body of the patient, from whom the disease-poison may be transmitted to another individual, who, if he be susceptible (or *Epinosic*),* may take the disease.

This class corresponds to the so-called *contagious* diseases of older writers, and to the modern *entogenous*, or *entogenic*, of some.

Typical examples of this class are :—small-pox, scarlatina, measles, whooping-cough. To these we may perhaps add diphtheria, although the obscurity surrounding this disease makes it a little doubtful where its position ought to be.

It is obvious also that syphilis, although belonging to this class by definition, is separated from the diseases enumerated by its known modes of communication.

It is further a question, which might be much debated, whether or not typhus should find a place here. That it is eminently communicable is, I think, clear; but it is not so clear that its poison is an entity capable of preservation for an indefinite time in a potentially active condition, as is the case with the poison of small-pox or scarlet fever.

(*b.*) The second sub-division of the communicable diseases corresponds in some degree to the class *infectious*, as distinguished from *contagious*, diseases : it is also called by some modern writers *ektogenic*, or *ektogenous*, signifying diseases the poison of which is propagated or developed chiefly, if not entirely, external to the body of the patient.

The typical examples of this class are enteric (or typhoid) fever and cholera.

In placing those diseases (especially enteric fever and cholera) in such a sub-division, I do not wish it to be understood that I entirely agree with the views of Pettenkofer and his followers as to the etiology of those diseases. At the same time, it has long been recognized that the modes of their propagation differed in a marked degree from that of small-pox, for instance, or scarlet fever; that personal communication of them was, if existent at all, at least rare, particularly with reference to enteric fever. But the absolute non-existence of personal communication has not only not been proved, but in the case of cholera for instance, there even seems evidence to show that it does at times occur. It may, however, be admitted generally that the complete development of those particular disease-poisons does take place outside the body, in the sense that the fresh evacuations seem less fitted to communicate the disease than they are after some time has elapsed, and some change in them has presumably occurred.

What is the nature of disease-poison ?

There is a growing tendency to look upon the poisons of the diseases we are considering as entities of an organic nature, probably organized, and capable of reproduction. These are, most likely, very low forms of vegetable life, either of independent existence or stages in the life of higher forms. Whatever they are they must be extremely minute, seeing that they have so successfully baffled our researches up to the present time; and if they themselves are minute, how much smaller must their spores or reproductive particles be ?

Here we touch upon a fertile source of fallacy. From time to time discoveries

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* *Epinosic;* see my "Lectures on State Medicine," p. 191; from ἐπίνοσος, morbus obnoxius.

are announced with reference to the true causes of cholera, enteric fever, relapsing fever, and the like. Now it is a fungus, next a bacterium, and later a bacillus. Sooner or later the views have to be modified, and some of them at least abandoned.

2.—*Means of Prevention of Disease.*

Assuming, then, that preventible diseases are due to specific poisons—probably minute vegetable organisms—there are certain ways in which those poisons may find their way into the system. These are, by air or by water; in the former case, by being inhaled into the lungs, or, perhaps, more likely by settling on the mucous membrane of the mouth and fauces, or of the nostrils: perhaps, also, by contact with the conjunctiva; or it might in some cases be by absorption through the integument. In the latter case, through water, it will, of course, be almost entirely by direct ingestion. Others might be inclined to add food and clothing as possible vehicles; but the case of food is implied in that of water—viz., it is ingestion; and the case of clothing resolves itself into infection through air, as the poison must leave the clothing to reach the body of the individual.

The means of prevention may be:—

(*a.*) *Direct*: that is, when a disease is only taken once in a lifetime, the establishment of a mild or modified form of the disease, or a disease of a mild type cognate to it, may act as a prophylactic; or an internal medicament may be exhibited, which counteracts in some way the action of a disease-poison. Of each of these we have but one example, and they are the only examples we yet know—viz., vaccination against small-pox, and quinine against ague. The recent researches of Pasteur seem to hold out a hope that we may find prevenient means similar to vaccination for some other diseases; and the diseases most likely to be successfully prevented in this way are those which as a rule are taken only once in a lifetime. But it would be plainly useless to expect that such a prophylactic should be found against diphtheria or cholera, where one attack gives no immunity against a second, or further attacks.

(*b.*) *Indirect* means of prevention.

Two things are admittedly necessary for the spread of disease—viz., a disease-poison or germ, and a congenial soil, nidus, nutrient, host, or "substrat," as Pettenkofer calls it. We must either fight the germ or sterilize the "soil." By the use of quinine we probably fight the germ; by the employment of vaccination we probably sterilize the soil. Two means present themselves for general application—viz., disinfection and general hygiene. Of disinfection I cannot say much that is favourable, and its generally efficient application is obviously impossible. There remains, therefore, general hygiene; and here, I think, we must seek our only safe refuge. By insisting on pure air, pure water, a pure soil properly drained, complete and efficient removal of all refuse and excreta from our habitations, clean dwellings, good and sufficient food, and attention to points of personal hygiene, we shall do our utmost to sterilize the "soil," so that no disease germ can live or propagate upon it. But attention to these points of hygiene must be constant, universal and unremitting.

3.—*Isolation* and *Quarantine.*

If disease has broken out, how far is isolation practicable or useful?

In such diseases as scarlet fever and small-pox, the value of isolation is undoubted as preventing spread, because it can be carried out effectually. The same cannot be said with regard to measles or whooping cough, the poisons of which seem to be so much more easily carried, and so much earlier actively dangerous. Isolation in diphtheria ought to be attempted, but the result cannot be depended

upon. It seems less necessary in enteric fever; although the question in this, as in other diseases, is complicated by the condition of life of the patient, and the possibility of proper attention to hygiene in his own home.

With reference to quarantine, as originally understood, I think we may look upon this as a relic of barbarous times. It is impossible to carry it out strictly and absolutely, and anything less than that only inflicts commercial loss and personal hardship, besides, in some cases, increasing the danger it seeks to prevent. If vessels are carefully inspected, and the actually sick removed to hospital for treatment, all others who show no sign of illness, as tested by the thermometer, &c., might be allowed to leave, with the precaution that each should be stripped of his clothing and take a general bath. The clothing in the meantime should either be disinfected by heat, or destroyed, the owners being compensated. Steps should, of course, be taken to cleanse and fumigate the vessel, and to disinfect everything on board. With proper hygienic measures at home we need not fear the importation of disease from abroad.

The Importation of Infectious Disease into Liverpool; its Diffusion and the Measures of Prevention.

Dr. J. Stopford Taylor, Liverpool.

The object of this paper is to bring before the Congress a brief account of some of the calamities which have been inflicted upon the inhabitants of Liverpool by the importation of infectious disease, to describe the limited means of prevention now adopted, and to elicit from the members an expression of opinion, how far and in what way these measures can be improved.

Liverpool, from its geographical position on the Mersey, is the great highway of all nations; travellers, whether of business or pleasure, pass through its portals, and a stream of emigration constantly flows through its port. In any measures that may be devised, it must not be forgotten that in these days of rapid intercommunication between one part of the world and another, any serious interruption of the flow of commerce would not be tolerated; and that, while shipowners would gladly welcome any means by which any outbreak on board their vessels could be obviated, they would strenuously resist any attempts to enforce a rigid quarantine, which would entail great inconvenience to business, and the infliction of a heavy pecuniary loss. To meet these requirements, all measures should be of a preventive rather than of an obstructive character.

The first instance to which I would direct your attention was the introduction of typhus by a crowd of emigrants from Ireland, in the year 1847.

It is estimated that above 400,000 Irish landed at Liverpool, and from 60,000 to 80,000 located themselves in the town, occupying every nook and corner of the already crowded lodging-houses, and forcing their way into cellars, which had then been recently closed under a local Act as unfit for human habitation. Under such conditions as these there can be no surprise that the "fever" brought by the starving people had an abundant harvest. The number of deaths registered was 21,219, of which 5,845 were from fever, and 2,589 from diarrhœa, giving a total death rate from all causes of 63·5 per 1,000.

There are two points to which I would especially direct your attention; first, the thorough inability of the local authorities, both in Ireland and Liverpool, to cope with so great a scourge; and that, even with our present means, it would be impossible to control or limit an evil of such magnitude. It should be the duty of Government, who are supposed to be in possession of all information as to food supplies and the health of the people, to be prepared to supplement, or, it may be, to take entire charge, and direct the various local authorities, so that by combined action a uniform and general system of relief could be adopted before poverty had produced its terrible effects.

The second point is one that may not at first sight be so apparent, but, so far as Liverpool is concerned, is of the greatest importance. The sudden importation of hordes of starving people into a city already overcrowded; people entirely unaccustomed to town life, without the means or knowledge for obtaining a livelihood except by mere physical labour, and for which the market was already overstocked; these poor people, from their extreme poverty and ignorance, constituted a constant danger to the health of the community, and furnished material ready for any epidemic influence that might prevail. The result of this poverty was manifested during what is known as the Cotton Famine, consequent on the Civil War in America. The diminished demand for labour at the docks was severely felt by the poor. Typhus became epidemic in 1862, in the district occupied by them, where there were 730 deaths recorded from the disease. It then gradually spread to other districts, causing 1,304 deaths in 1863, 1,774 in 1864, 2,338 in 1865, and 1,523 in 1866. Four years later—that is, in 1870, there was another illustration of the poverty of the people. Some emigrants from the Continent passed through London to Liverpool, carrying with them from their place of departure relapsing fever. This disease found a congenial soil and spread rapidly, and though not very fatal, caused 355 deaths in 1870, 207 in 1871, and 25 in 1872.

I will now draw your attention to the cholera epidemic of 1866. The steamship *England* sailed from the Mersey on the 28th of March, 1866, with 807 passengers, of whom 465 were foreigners. At Queenstown she received 393 more, principally Irish. On the sixth day after sailing, a Dutch boy, aged twelve years, was found dead in bed; the boy's father was next attacked, the symptoms being clearly diagnostic. About three hours after, an Irishman, whose berth was near, became ill, and died in eight hours. The disease now became general, and when the vessel arrived at Halifax, twelve days from the Mersey, 92 deaths had occurred, including 6 of the crew.

The *Virginia* steamship sailed on April 4, with 721 passengers, of whom 260 were foreigners. On the eighth day after sailing, 3 of the foreign passengers died of cholera, on the next day 8 more died, and by the 22nd of the month, when the passengers were transferred to the quarantine vessel, the *Falcon*, there were 50 deaths. Between the 22nd of April and the 23rd of May, 55 of the passengers died on board the *Falcon*, making a total of 105 deaths.

On the 2nd of May, 2 fatal cases of cholera occurred among emigrants at Liverpool. Subsequently, 2 more emigrants, who had been staying at the same lodging-house, died from cholera. These people had been intending passengers by the *Helvetia*, which sailed on May 2, with 925 passengers on board, of which 527 were foreigners. Several of the latter had come direct from Rotterdam, the others from Germany and the countries of northern and central Europe. She was to call at Queenstown for more passengers, but before arriving there two deaths from cholera occurred. The vessel was denied admittance into the harbour, and had to

return to Liverpool, where she arrived on the 4th. It was necessary to telegraph to the Home Secretary, and ask for an Order in Council to prevent the landing of the sick, and the dissemination of the passengers who had been in contact with the sick in the lodging-houses of the town. The Order was granted. Two vessels were hired and fitted up; one as a hospital, the other for the temporary reception of the healthy until more permanent accommodation could be provided for them. The first cases of cholera on board the vessel in the river—those of a man and a child—occurred on the 6th. The disease then became epidemic amongst the passengers, and by the 23rd of the month 46 died, and also the surgeon of the ship. Several other vessels sailing from the Mersey for America had outbreaks of cholera on board. In each week after the return of the *Helvetia* to the Mersey, to the beginning of July, deaths from cholera were registered, but they were sporadic, not epidemic cholera. On the 2nd of July, information was received from the Registrar that a death from English cholera had occurred on the previous evening in a court in Bispham Street.

This was the commencement of the epidemic, which now spread over the whole city. There was no evidence how the diseased woman contracted the disease. Between the 1st of July and the 30th of November 1,763 persons died.

Having briefly described the epidemic of typhus and cholera, I will now proceed to show the means we possess for preventing the introduction and diffusion of infectious disease.

The rapid growth of Liverpool, and the dense population, have necessitated great and important sanitary proceedings. Powers have been obtained and enforced to open up crowded districts, by making wide and open thoroughfares, by pulling down houses which obstructed the ventilation of courts and alleys, by constructing an entirely new system of sewers, by increased water supply, and the abolition of cesspools and open middens. Overcrowding in houses has been much reduced by the inspection and registration of all houses taking in nightly lodgers (hotels excepted). By these means a large amount of supervision and control is exercised over a great portion of the general population, as well as over visitors and emigrants. It is impossible to give an estimate of the number of people who pass through the city in twelve months; but the Board of Trade affords returns of emigrants who sail from the port. The total number for the year 1880 was 183,502, on board 993 ships, and comprised 74,969 English, 1,811 Scotch, 27,986 Irish, 74,115 foreigners, and 4,621 whose nationality was not known. The shipping companies make no provision for the accommodation of British emigrants, who generally find their way to lodging-houses kept by their own countrymen. There is no medical examination of them on arrival, nor previous to their going on board; but the keepers of the lodging houses are bound by the regulations under which they are licensed, to give immediate notice to the health authority of any case of infectious disease. With the foreign emigrants it is different, the shipping companies having arranged a system of self-protection. On the arrival of the emigrants, under the charge of a guide who has accompanied them from the port of landing, they are taken direct to houses engaged by the companies, and as soon as possible visited by a physician; and should any case of infectious disease be detected, the sick are removed to a hospital, and the clothing also disinfected. All emigrants, British and foreign, are examined on going on board ship by surgeons appointed under the Emigration Act, who refuse all sick or doubtful cases.

The Corporation of Liverpool was appointed the Port Sanitary Authority in 1873, and it devolves upon them to make such arragements as may be requisite, to

treat all cases of infectious disease coming into the port on board ships, and to examine into the sanitary condition of all vessels frequenting the port. The medical officer of the city was appointed medical officer of the port.

About seven acres of land have been purchased, on the margin of the river, at New Ferry, three miles from the city, and a hospital capable of containing twenty-four beds has been erected, and can be furnished at a few hours' notice for the reception of infectious cases from ships. The quarantine station for vessels is immediately opposite the hospital. The sick, if in a fit state, would be removed to hospital, whilst the other passengers could be accommodated in huts or tents, instead of confining them on shipboard, or permitting them to land in the city. Of course, we have no direct power to carry out this arrangement; but indirectly there is power, more particularly with emigrants.

All cases of infectious disease on board vessels entering the port are reported to the medical officer by the Customs authorities, excepting yellow fever and plague, which are taken charge of by a physician appointed under the Quarantine Law of 1825. For the last few years there has been very little infectious disease on ships—only a few cases of small-pox, fever, scarlatina, or measles; and it has not been thought necessary to send them to the hospital at New Ferry, accommodation being provided for them at the Workhouse Hospital, or at Netherfield Hospital, an institution situated in the city, and supported by a grant from the corporation, payments by patients, and voluntary contributions. Vessels are either disinfected by the crew, under proper supervision, or else by the sanitary staff; the clothing, bedding, &c., being removed to one of the disinfecting establishments where they are submitted to dry heat in the stoves, in which the temperature can be raised to 260° F.

I have thus briefly placed before you typical illustrations of the importation of infectious diseases into Liverpool, to which, owing to its position and the character of its business, it is constantly liable; but, since the passing of the Public Health Act, 1875, we are better able to deal with such cases, in enforcing removal of the sick to hospital, and the conveyance of dead bodies to mortuaries. The powers we are now seeking for the compulsory notification of infectious disease will also be of great value, but you will readily perceive that, notwithstanding these powers, much mischief may be caused by interested and careless people, and I shall be glad to receive any suggestions or advice how we may more efficiently protect the health of our city.

On the Prevention of Scarlet Fever.

Dr. David Page, Kendal.

It may be stated at the outset that those precautions which, in respect of scarlet fever, prove successful for the protection of a neighbourhood or a household, must also be the real defence for the community which is immediately concerned, and therefore for other and more distant communities. All national or international preventive measures against the diffusion of such a disease as scarlet fever must, in their essence, be measures for effectual individual control. These measures ought to give practical effect to the questions:—

1.—What ought to be done with the infected individual or patient?

2.—How long may the patient continue to be a possible source of infection?

3.—What is the latest period after exposure to infection at which the disorder will show itself in a person who has received the infection into his system?

4.—How soon may a person who has been exposed to risks of infection be pronounced safe against attack?

Two main clinical facts are involved in these questions:—

1.—The duration of the incubation period of scarlet fever.

2.—The modes of exit from the system of the fever poison.

A knowledge of the incubation period is essential in disposing for the time of a person who, having been exposed to chance of infection, may have contracted the disorder. In this case an appreciation of the maximum period of latency is required. The literature of the subject is vague and untrustworthy.

The author's observations coincide with those of late Dr. Murchison, and may be summed up as follows:—

1.—The common duration of the incubation period in scarlet fever is from twenty-four to forty-eight hours.

2.—The period is occasionally longer, lasting from three to five days.

3.—In rare instances practically absent, the symptoms following quickly upon exposure to infection.

Based upon these conclusions, the author requires that a person who has been exposed to infection should, before being pronounced safe from its probable consequences, be kept under surveillance for a week, and only then, after change of clothes, and baths, be set at liberty.

Of even greater importance than this, is the action which ought to be taken in regard to the fever-patient himself. This action is rendered difficult by reason of the diversity and inconstancy of what are commonly looked for as the diagnostic features of scarlet fever.

An estimate of the real value of desquamation of the cuticle as a trustworthy guide in preventive measures, must be based upon knowledge of irregularity of process in relation to time, quantity of eruption, or intensity of attack. The author pointed out the error of regarding the infective process at an end before cessation of desquamation and for some considerable interval afterwards: also the importance of the pathology of scarlet fever as regards the nature of the eruption and the tendency to albuminuria, as a guide to preventive action and treatment.

The author's experience points to the necessity for isolation of the scarlatinal patient for a period of not less than eight weeks, and he would state the rule of Nature to be, isolation for this period as a minimum, and in cases of protracted desquamation or of relapse, until entire cessation of the process, and for a fortnight afterwards.

For the rest, official action should include:—

1.—Due provision for the separation of the sick from the healthy in the way of hospitals and convalescent homes.

2.—Notification of the occurrence of cases of infectious disease.

3.—Continuous supervision of all scarlatinal convalescents.

4.—Control of school attendance of children belonging to infected families.

5.—Disinfection and purification of infected houses and things.

How to Limit, and more or less practically to Nullify, the Force of Contagion and Infection by holding the Breath while in immediate contiguity with the Patient.

Dr. Henry Mac Cormac, Belfast.

Diseases termed infectious, and propagated by miasma from man to man, are communicated, and only communicated, as I believe, by inhalation. Avoid the inhalation of the miasma of disease, and you may prevent the transmission of disease. If we assume the aggregate area of the lining of lung air-cells to amount to forty square feet, we see at once what an enormous avenue to the admission of the germs of disease is thus furnished when brought into such close proximity with the circulating fluid. If we arrest for the moment the act of respiration, we may also, in so far, arrest the transmission of disease. If the proposition I set forth—namely, that infectious disease is communicated by inhalation, be proved correct, a greatly increased measure of safety for all who have to deal with such—that is to say, for physicians, nurses, relatives, and, in a sense, at one time or other, the entire community, is obtainable.

The poison of infection, when adequately diluted, does not by inhalation communicate disease. Take, for example, a case of plague, or typhus, or Asiatic cholera, or scarlet fever, or diphtheria. In the immediate proximity of the patient the poison is highly concentrated and highly communicable. At a certain distance, so that the poison shall be adequately diluted by admixture with pure fresh air, it ceases to be communicable. By sufficient ventilation, the poison of infectious disease is rendered, in fact, incommunicable. In a long experience, I never saw the poison of fever, when I had the control of the ventilation, transmitted from the sufferer to another person. I have seen typhus fever actually extend to nine persons in one household, when ventilation, or air renewal, was not attended to. Dr. Jackson, when dealing with the plague in Morocco, prescribed for the patients with entire personal immunity, from an open window, elevated but a few feet above them.

I have myself had much to do with Asiatic cholera, and always held my breath when in immediate proximity with the sufferers. I never experienced the malady, though every one associated with me, nurses and medical men alike, suffered more or less. For years, I had charge of a fever hospital, and was never attacked by typhus fever, though all the doctors, whether predecessors, associates, or successors, had it. The hospital nurses, without exception, suffered. In private practice, I enjoyed the same security, and I have attended typhus fever in the severest forms, and in the vilest and worst ventilated abodes.

A French physician, M. le Dr. Laval, médécin major des hôpitaux, it is stated in the *Moniteur de l'Armée*, August, 1874, finding himself on leave in the Regency of Tripoli, and, hearing that the plague was raging at Merdj, about 20 hours' journey from Bengazi, repaired thither and, without medical co-operation, devoted himself without remission to the aid of the terrified population, preventing the malady indeed from spreading, but, to the infinite regret of every one, falling at last a victim to his own superb humanity. This admirable person, I believe, would not have perished, and multitudes of others might have escaped likewise, had he and they, as I did, held in the breath, taking in a good chestfull beforehand, while immediately contiguous to the sick. I am anxious not to exaggerate the value

of the precaution whose adoption I urge. I have waited thus long in order, if I could, only to realise fresh assurance.

I simply state my own convictions and experience, and leave them for the consideration and adoption, should they haply approve, of the noble profession of which I am an humble member.

DISCUSSION.

Dr. EZRA M. HUNT, Trenton, N.J.: thought it an important question whether our direct means of prevention, as illustrated by vaccination, had not a far more extended application than at present. Much depended upon the mode by which an infection was introduced into the system. Of old, we thought the safety of inoculation depended on the handling of the patient. Now it is quite certain that its introduction by another method from that by which small-pox is contracted has most to do with the modified benignity. The juice from a pleuro-pneumonic lung in contagious pleuro-pneumonia, introduced under the skin of the animal, caused a protective disease, sometimes quite severe, but if so, not attacking the lung. We seemed thus able to divert a disease from attacking its favourite vital organ; to prevent its pathognomonic and fatal lesion by the mode of introduction, and, at the same time, to prevent the occurrence, and so the diffusion, of the disease. We did this where there had been no culturing of the virus, but merely by anticipating how it should make its approach into the system. This might be explained by regarding it as a dilution of the virus, or as resulting from a conveyance of less quantity than finds its way over the very large surface afforded by the air cells of the lungs. The fact that diseases are thus greatly modified, without any manipulation of the virus itself, must lead us to inquire and experiment in other directions, as well as that which has reference to the exposure of the infected material to air or its preservation for lengths of time. Not only quality and quantity of material, but mode of introduction, modified, perhaps, even the tissue into which it was introduced under general hygienic precautions. The speaker desired to draw attention to the fact that regard must be had, not only to hygienic surroundings, but also to the disinfection or clearing up of the person as much as of his vicinage. All persons, we say, have not equal susceptibility. This, we think, is only a parallel of the statement that all places are not equally susceptible. In both cases, was not the susceptibility the result of unhygienic conditions—the first being in the surrounding, the second in the person? Until we looked for the insanitary state of the individual and its cause, with the same diligence that we looked for the insanitary surroundings, we were only dealing with one half of the problem of prevention, so far as affected by localization. He thought the remark made by Mr. Simon in his opening address, that as to infections, we were to find out "what agreed or disagreed with their life," was closely to be studied in its reference to internal as well as external conditions. The idea was also to be extended to the abortion of epidemics and the prevention of contraction of disease, by the introduction of such substances as quinine, potassium chloride, tinctura ferri chloridi, &c., and maintaining them in small quantities in the blood, so that the infected particle could not set up those changes which constituted the disease. The principle was not that of introducing a specific, but of maintaining such an antiseptic condition of the system for the exposed period so as to render it resistful of an infection to which it had been exposed. By personal prophylaxis, and by the sustained presence of antizymotics in the system during what would be the incubative stage of an infection to

which an individual has been exposed, we might add to the methods of limiting and preventing epidemics.

Dr. Henri Henrot, Rheims: thought that whilst Dr. Mac Cormac's suggestion was useful, still it could not be applied without suspending respiration, and he drew attention to M. Fleurot's suggestion that air, like water, might be filtered through a proper medium. Since the labours of M. Chauveau, we knew that in the infectious elements of disease we had to deal with minute bodies; and to protect the respiratory passages against these, it would suffice to make the air inspired pass through a filter of cotton wool, contained in a perforated metallic box; antiseptic substances could also be placed inside the box. At the Hôtel Dieu, at Rheims, the pupils could not, during an epidemic of typhoid fever, perform the autopsies without suffering from numerous gastric and other symptoms, all of which ceased on using the respirator in question. After showing that the use of such an instrument was quite compatible with professional dignity, and was as much warranted as the use of grease to the finger in examining syphilitic patients, he also drew attention to the question of following up the results attained by Mr. Lister, and in destroying the atmosphere germs in the vicinity of infectious patients by means of vaporization, of giving protection to the attendant physicians. By adopting, then, the methods he had referred to, much effectual protection against infectious diseases might be realised.

Dr. Grimshaw, Dublin: called attention to instances of communication of diseases between Liverpool and Dublin, with the view of supplementing the remarks of Dr. Stopford Taylor concerning the importation of typhus into Liverpool from Ireland. He stated that the small-pox epidemic of 1871–72, in Dublin, had its origin in Liverpool, and also referred to a remarkable instance of the introduction of small-pox into Dublin from Brooklyn in America *viâ* Liverpool. Dr. Grimshaw insisted on the importance of the notification of infectious diseases to the local authorities, also the importance of having the occurrence of such diseases registered and the results published, with the view of notifying authorities of infectious disease from one centre to another, so that such authorities might take special measures to repel their invasion and prevent their spread.

Dr. G. Varrentrapp, Frankfort: wished to make a few observations with reference to the paper of Dr. Gibert, as to the causes of an epidemic of enteric fever in Havre. He had said that a defective water-supply or milk-supply was absolutely to be excluded, but that considerable pollution of the subsoil water had resulted from the existence of a very large number of *puisards* which, during recent years, had been filled up, or had at least not been used after the construction of sewers. These sewers, too, had done rather harm than good, because they had been laid in such a manner as to raise the level of the subsoil water, by arresting its flow. The speaker thought, that in treating such questions, particularly one which was so complicated, and which had led to so many different opinions with regard to the cause of the disease and its method of spread, we should not narrow the field of our investigations, but rather extend it as far as possible. He considered that the actual state of our experience did not allow us to say that enteric fever, or the outbreak of an epidemic of this disease, was the result of a pollution of the underground water, which in Havre had long been polluted. It rather seemed to him as proved that typhoid fever had a specific cause, a germ, which, if it take root and spread,

needed—(*a*) a soil in which it might easily find the conditions of further development, and (*b*) an individual predisposition. To this last point Dr. Stopford Taylor had already referred, teaching to how large an extent such individual predisposition existed among the poorer classes of Liverpool, particularly among the emigrants. Now, merely looking at the pollution of the soil, we had not only to look at the degree of pollution, at the different causes of pollution, continuous and accidental, but to the soil itself. We must also regard its permanent or changing humidity, the fall or rise of the "grundwasser," the temperature of the soil, and similar matters. In Germany, the influence of a falling subsoil water was more and more acknowledged as one of the conditions favourable to the epidemic prevalence of enteric fever, of cholera, &c. On the other hand, the period of the highest temperature of the ground, at a depth of several feet, seemed to be decidedly favourable to the seasonal fluctuations of these diseases. As to the arrest of the underground water at Havre by means of sewers laid in a wrong direction, he could scarcely suppose that this had really taken place, no detailed information having been given so far as actual measurements are concerned. He could not imagine that the sewers were such as to bar the efflux of the water; besides which, sewers constructed with brick always admitted of passage of some water through them, and a rivulet would generally form itself along their exterior.

Dr. R. Beveridge, Aberdeen: said that at Aberdeen, a town of over 100,000 inhabitants, a system of voluntary notification of infectious diseases had been in operation for three years. A fee of 2*s.* 6*d.* per case was offered, and with three or four exceptions all the medical men, numbering nearly fifty, agreed to notify the cases coming under their care. The result had been to a great extent satisfactory; the mortality from zymotic disease had fallen in the last three years from about 45 per 10,000 to about 24 per 10,000, and the expenditure of the Public Health Committee had fallen very materially, notwithstanding that a pretty large sum had been given to medical men as fees for notifications. As a consequence, there had been no difficulty in obtaining compulsory power of notification, which power was now coming into operation. With respect to the use of disinfectants, he desired to express his opinion that these had been much overvalued, that they were really of use only as a temporary measure, and that the real disinfectants were fresh air and clean water in abundance. His experience of hot air stoves had also not been favourable. He had tested several repeatedly, and had found that a single fold of thick cloth prevented the temperature rising within that fold. Thus, a thermometer placed in the stove showed, both before and after the experiment, a temperature of 300° F. outside an article, but when the thermometer was covered with a single layer of a blanket the temperature was not more than 190° F. till after an exposure of five or six hours, and even then the temperature did not rise above 210° F.

Dr. Dahl, Christiania: desired to refer to the danger arising from infected rags. In addition to small-pox being spread by this means, it has been reported that diphtheria has had a similar source in Norway. Anthrax, too, is communicated to those employed in paper mills. The difficulty in preventing diseases from being thus spread lies in the impossibility of properly sorting the rags after boiling —a difficulty which amounts to a prohibition.

Dr. Thorne Thorne, London: drew attention to the influence of a system of compulsory notification of infectious diseases in securing the isolation of patients

suffering from infectious diseases. Thus, as regards scarlet fever, he gave details proving that in the towns of Leicester and Warrington, where Acts regulating such notification were in force, and where that disease had heretofore periodically caused large mortalities, impending epidemics had been practically stamped out by means of the early information as to the presence of the disease in individual houses, and by means of the isolation in hospitals, which it was, under these circumstances, possible to secure before the disease had time to spread. He also desired to bear testimony to the efficiency of the Automatic Disinfecting Stove designed by Dr. W. H. Ransom, F.R.S., and manufactured by Messrs. Goddard & Massey, of Nottingham. In this stove a temperature of 250°-260° F. could be maintained, and could be made thoroughly to penetrate into the interior of dense articles, such as beds, without risk of damage. Owing also to its automatic action, due to a regulator controlling the gas which forms the fuel, the stove was in many places left whole nights quite unattended, after it had been filled with costly articles, such as furs, silks, satins, &c. and this without any damage accruing. The same automatic action prevented the consumption of any gas beyond that actually necessary to maintain the temperature indicated by the regulator.

Dr. William Ogle, London: had formed a much more favourable opinion as to the efficacy of hot-air disinfection than appeared to be entertained by their colleague from Aberdeen. Some years ago, when there was a severe outbreak of small-pox in a country place where he was Medical Officer of Health, he had the sick removed with their clothes and bedding to a temporary hospital. When they got well they were sent home, and the clothes and bedding with them, these having been previously disinfected by one of Ransom's gas disinfecting stoves. In no single instance out of the numerous cases thus dealt with was the return of the disinfected articles to the house followed by a fresh case of small-pox. He should be sorry if any discredit was thrown upon what he believed to be a most valuable agent in the hands of a medical officer of health. It has been said that a temperature sufficient to disinfect woollen articles would also spoil them. He had not found this to be the case. He had carefully examined many of the articles disinfected, and had found no recognizable alteration in their texture or fabric. Of course, there were stoves and stoves, but he simply spoke of the kind of stove of which he had himself due experience.

Mr. D. B. Balding, Royston: felt bound to state that he had observed a marked readiness on the part of patients and their friends to avail themselves of the advantages of a hospital for infectious diseases when they felt convinced that they would be properly taken care of, and when a judicious amount of perseverance had been brought to bear upon them, and this, his experience led him to note was not confined to the lower classes, but extended to the small, or even to the better-class tradespeople. He had, however, found a somewhat serious difficulty occur at a later stage, and that was, that during the period of convalescence; when all danger to the patient was over the friends would frequently insist upon their removal home, before the medical attendant was able to testify that this could properly be done without danger to others. He also stated that he had carefully watched the action of one of Ransom's disinfecting stoves for several cases, and he did not hesitate to speak as decidedly as Dr. Ogle had done in reference to its perfect action.

Dr. Tripe, London: said that as he had two hospitals for small-pox and one

for fevers in his district, he was desirous of making a few remarks on Dr. de Chaumont's paper. The Asylums' Board opened a small-pox and fever hospital in 1871, and as the former disease had been very prevalent and fatal in Hackney since then, even when compared with all London, he carefully examined the statistics respecting small-pox, and ascertained that during 1871–80 in the streets adjacent to the hospital, the annual mortality per 1,000 population reached the enormous figure of 4·1; in the streets within a quarter-mile radius, inclusive of those just mentioned, the annual mortality was 2·45 per 1,000; whilst amongst 37,000 other persons, of a class similar to those residing near the hospital, it was 1·60; and amongst the remainder of the inhabitants, estimated at 119,000 persons, it was only 0·21 per 1,000 persons. In 1861-70 the deaths from small-pox per 100,000 inhabitants were 28 in London and 17 in Hackney, whilst in 1871–80 they amounted to 46 in London and 64 in Hackney. He attributed a large proportion of this increase to the Homerton Small-pox Hospital, as the chief mortality occurred in its vicinity, but did not believe, if it had been properly constructed and conducted, that anything like the number of deaths would have occurred. The hospital was badly placed, and was too near surrounding houses. In 1871–72 there was a small-pox hospital at Clapton, which was within thirty feet of the gardens belonging to a row of houses, and near the foot-path (which was not much used), without any death, and only two cases occurring in the neighbourhood. It would therefore seem that the contagion spread by other means than the air. Great care should therefore be taken as regards the construction and management of small-pox hospitals, otherwise the great public benefit which arose from the removal of patients from their homes would be more or less counterbalanced. Scarlet fever and typhoid fever were not more prevalent in the neighbourhood of the hospital after its opening than before, showing that these fevers were not so infectious as small-pox.

Dr. DUDFIELD, London: had noted with satisfaction the unanimity of opinion in the Section with respect to measures of preventive medicine, and had thought the occasion favourable for strengthening the hands of those now engaged in the attempt to secure, by compulsory legislation: (*a*) notification of infectious diseases, (*b*) provision of free infectious disease hospitals, (*c*) powers of removal of the infectious sick when necessary for isolation or treatment, and (*d*) the severance of hospital treatment from any relation to pauperism. He had submitted the terms of a resolution with the above objects to their President, who, whilst ruling that no resolution could be put, had at the same time stated that the known unanimity of the Section on the points in question would have all the effects of a resolution. A great movement, recently carried to a successful issue, had been made to unite the health authorities of the metropolis in favour of more stringent sanitary legislation of a preventive nature, and an important deputation consisting of delegates from Vestries and District Boards of Works, with their Medical Officers of Health, had waited upon the President of the Local Government Board to request the introduction of a measure to give effect to their views. The Council of the British Medical Association, the Society of Medical Officers of Health, the Public Health Section of the Social Science Association, and the National Health Society, had been influentially represented on the occasion, and were practically at one with the health authorities; and, subsequently the Asylums' Board, and the Metropolitan Board, and the Metropolitan Guardians' Association, had expressed views identical with those laid before the President—views which he had tried to condense in the terms of

his intended resolution. And although in the state of public business the introduction of such a measure was impracticable during the present Session, he had confidence that so weighty an expression of scientific official and public opinion could not fail of its due effect, and must ere long lead to improved sanitary legislation. Addressing such an audience it was unnecessary to insist at length on the paramount importance of notification, which had been found useful even in places where no hospital had been provided for infectious diseases. People were more likely to obey the law, and be careful to avoid whatever should endanger the spread of disease, when they knew that the eyes of the official health guardians were upon them. Thus isolation was more likely to be efficient in the home of the sick when the case had been disclosed; and to be prolonged for a sufficient length of time. It was certain, moreover, to be followed by disinfection, which in present circumstances was often omitted, leading to the spread of disease. The disclosure of disease, however, would be of far greater value in places like the metropolis, which had provided hospital accommodation for infectious cases. But the hands of sanitary authorities needed strengthening by increased powers of removal of the sick when they could not be properly treated or safely isolated at home—such powers, namely, as had been conferred on certain provincial sanitary authorities by a well known amendment of the 26th Section of the Sanitary Act, 1866, and Section 124, Public Health Act, 1875. Many cases of such diseases as scarlet fever and small-pox were of little consequence as they affected the individual sufferers, but these perhaps were the most dangerous cases regarded from the public point of view, owing to the difficulty of securing their isolation for the necessary length of time—a matter of extreme importance in London, when so many hundred thousands of people lived in houses let out in lodgings—a whole family in innumerable instances occupying but a single room. When infectious diseases broke out in such houses, the air in the common staircase became infected, and there was inevitably more or less of contact between the sick, and the healthy, leading to the spread of disease. An incidental evil resulting from this state of things was the interruption to education, children not being allowed to go to school from an infected house; and in present circumstances, infection might linger for a month. Disease, moreover, might be, and often was, spread by the clothing of the relatives, and others who came in contact with the sick. He had said that they possessed in London fairly sufficient hospital accommodation, and this was practically free. He urged, however, the necessity of making it free by law, of dissevering it from the taint of pauperism, and of protecting its administrators from vexatious interference in the discharge of the duties entrusted to them by law. The treatment of small-pox should no more entail pauperism on the man who allowed his child to go to a hospital for the sake of isolation, and for the public good, therefore, than the vaccination of that child at the public cost—the object of vaccination being to protect both the child and the public against small-pox. His time was up, and he would conclude therefore by saying, "Give us compulsory notification; give us free hospitals untainted with the stigma of pauperism; give us increased powers to remove the sick to hospital when necessary, and a gigantic stride would have been made in preventive medicine."

Dr. F. DE CHAUMONT, Netley: had felt much interested in the remarks of many of the previous speakers, particularly in what had fallen from Dr. Blaxall with reference to the outbreaks of cholera in the Channel Fleet, as pointing out the futility of quarantine as usually carried out. With reference to Dr. Dahl's remarks on the

infection through rags, there could be no doubt that this did take place as well as through ordinary clothing. Cholera had been again and again introduced into America by means of the clothing of immigrants. The last case of cholera in Malta, in 1866, was also a case in point. After the disease had ceased for some time, a single sporadic case occurred, the history of which was as follows:—A soldier's wife attended a woman in cholera who died. She secreted the shift the patient wore, and, after keeping it for some time, wore it without washing it. She took cholera and died. No further case occurred. The remarks of Dr. Beveridge, with reference to the inadequacy of disinfectants in the absence of other general hygienic measures, were very interesting, and bore out the conclusions the speaker had arrived at from experiment. With reference to the hot-air method, he should not be inclined to depend upon it absolutely any more than Dr. Beveridge, but he thought it possible to get better results than Dr. Beveridge seemed to have done. At Southampton, in the Royal Hants Infirmary, the heat was sufficient to kill and render brittle any vermin (such as fleas and the like) even in the centre of bundles of bedding. In that institution a practice prevailed, which deserved general imitation:—after every case, of whatever kind, all the bedding, beds, clothing, &c., were disinfected, so that every patient had a positive assurance that he occupied a bed, and put on clothing, absolutely pure. There was a certain loss in the deterioration of the fabrics; but this was cheaply purchased by the perfect immunity assured. But it was necessary that the disinfection should be carried out in an intelligible way, and that the infected and disinfected materials should not be present together in the same apartment, as he had seen in an hospital in this country. It reminded him of a story of an officer who was obliged to take off his steel scabbard, &c., on entering a magazine, but omitted, as well as the sentry, to observe that he was smoking a cigar! It was in this way that processes were compromised, which were otherwise good and trustworthy.

III.—MEASURES BY WHICH TO PREVENT THE DIFFUSION OF SYPHILIS.

La syphilis en échec.

Dr. M. E. da Cunha Bellem, Lisbon.

Si contre la propagation de la syphilis il faut opposer la propagation des principes de la morale et de la conscience, si contre les penchants de la lubricité il faut opposer les conseils de l'hygiène la plus soignée et la plus rigoureuse, contre la tyrannie de la syphilis il n'y a qu'à opposer la tyrannie de l'inspection.

Certes, vous trouverez un peu bizarre nos idées à cet égard; vous y trouverez un attentat à la liberté personnelle; mais nous croyons que si chacun a le droit de ne pas se donner la syphilis, pas même par imprudence, personne n'a le droit de la transmettre aux autres; autant vaut dire que personne n'est libre d'acquérir cette maladie ou de l'entretenir chez soi, car tout le monde doit être affranchi des dangers de la contagion.

Nous avons assez de confiance aux moyens s'adressant à la moralité et à la conscience du peuple. Mais ça ne suffit pas.

Il y a des étourdis qui se donnent la syphilis par insouciance, par lubricité, par ignorance des dangers qu'ils vont courir. Il y a aussi des fanfarons du vice, des gens qui comptent les fois qu'ils ont contracté la syphilis, comme les vieux

soldats peuvent compter celles qu'ils ont été blessés sur le champ de bataille—ceux-là s'imaginant que leurs cicatrices et leurs difformités sont aussi honorables que les difformités et les cicatrices de ceux-ci.

Ces gens, en contractant la syphilis, ne s'abstiennent pas de la contagier sciemment; et ces gens en agissant de la sorte, commettent un crime, car c'est un crime prévu dans tous les codes, celui de nuire à la santé d'autrui.

Vous aurez beau nous dire qu'il faut respecter la liberté. Le respect de la liberté a une limite qui lui est marquée par le respect même de la liberté des autres.

En fait de maladies contagieuses, il y a quelques pays qui adoptent encore le régime quarenténaire; et s'il y en a d'autres qui préfèrent les avantages du libre accès, par pur égard pour les intérêts du commerce, ce n'est-là qu'une question de la communauté, les représentants de la majorité du peuple voulant courir la chance d'une épidémie plutôt que d'affaiblir son commerce. Mais si un homme frappé de la peste, contagié de la morve ou de la rage, si une femme atteinte de péritonite puerpérale voulaient contagier leurs maladies, on tâcherait d'en éviter l'approche, on chercherait à les isoler, afin qu'ils ne pussent devenir nuisibles aux autres.

Pour ce que concerne la variole, la pratique de la vaccination obligatoire gagne du terrain partout, et ceux-mêmes qui reconnaissent à chacun le droit de devenir varioleux ne reconnaissent à personne le droit de contagier cette maladie aux autres, et ils ne se refusent à accepter la vaccination obligatoire que sous condition d'isoler les sujets atteints de variole.

Pourquoi donc agirait-on autrement envers la syphilis? A quoi bon permettre aux syphilisés la liberté de communiquer librement cette terrible maladie, qui ravage les générations, qui fait le malheur du foyer, qui transforme la vie en un tourment, qui se transmet aux enfants par un malheureux héritage?

Ou ceux qui contagient la syphilis ont la conscience du mal qu'ils font, et, en ce cas ils sont des criminels, ou ils n'en ont pas la conscience, et ils ne sont que des fous, auquels il faut ne pas accorder une liberté dangereuse.

Le conseil prudent et autorisé sur les avantages de faire enrayer le progrès du mal, chacun cherchant à ne pas contracter la maladie, tout le monde ayant la conscience nette du devoir de ne pas la contagier, c'est beaucoup déjà, mais ce n'est pas tout.

La loi morale défend le crime; mais il faut que la loi pénale la complète en punissant ceux qui ne lui obéissent pas.

Il y va du bonheur et de la tranquillité générale.

Les allures les plus franches sont nécessaires en ce sujet, et la sévérité de l'inspection doit être la garantie de ce que la loi morale est respectée.

Vous soupçonnez que quelqu'un est syphilisé ou qu'il a transmis la syphilis à un autre, de même que vous pouvez soupçonner qu'un tel a commis une action criminelle envers son prochain.

Vous tâchez d'en établir l'évidence. L'examen direct est la base de toute procédure ultérieure.

L'inspection des gens soupçonnés d'être atteints de syphilis sera donc la première démarche à faire; et s'il est une punition douce et bienfaisante, c'est certainement celle que l'on peut infliger aux syphilisés; leur procurer la guérison.

Nous savons bien qu'il est très-difficile et très-grave, en plusieurs cas, de forcer les gens comme il faut à se soumettre à l'inspection qu'ils ne manqueront pas de considérer comme un humiliation, comme un déshonneur même. Il faut agir

tout doucement, avec la délicatesse la plus soigneuse, avec les plus grands ménagements : pourtant il faut agir.

C'est au sens-commun des autorités sanitaires, c'est à la prudence de leurs délégués de ne pas faire un faux-pas en ces démarches ; c'est à eux de respecter la pudeur, quand même ce ne serait que de la fausse pudeur.

C'est aux gens qui reconnaissent le besoin de se prémunir contre les malheurs qui affligent l'humanité et qui tournent en danger l'exercice de l'acte physiologique le plus noble et le plus attrayant, c'est aux gens qui ne se laissent pas vaincre par des préjugés trop enracinés, c'est à eux de donner l'exemple de la facilité et de la docilité à se soumettre aux examens, de donner le conseil aux autres de s'y soumettre aussi de bon gré.

Mais surtout c'est à la loi de punir impitoyablement toute fausse dénonciation en fait de contagion de syphilis.

Cependant toutes ces démarches n'auront pas de réussite si l'on ne parvient pas à les généraliser parmi toutes les nations civilisées.

Le danger est commun. Il faut que l'action soit commune aussi ; il faut que tous les pays s'empressent à l'envie de chasser la syphilis, d'enrayer le progrès de son mal envahissant.

Une convention internationale peut seule régler les moyens de combattre partout et avec efficacité la syphilis.

On s'en moquera d'abord. Il ne faut pas se décourager.

Si quelques années auparavant vous aviez osé dire qu'il n'y aurait plus de nationalité pour les médecins, pour les infirmiers, pour les brancardiers, pour les blessés même de deux armées ennemies on vous auriez ri au nez ; néanmoins la convention de Genève est parvenue à réaliser cet impossible.

L'utopie d'aujourd'hui sera la vérité de demain. La persistence de l'homme produit chaque jour des merveilles bien plus étonnantes que celle-ci.

D'ailleurs il ne faut pas croire que la providence ait donné la syphilis comme compagne inséparable du genre humain ; au contraire, il faut espérer qu'il y aura un moyen de s'en débarasser.

Ce moyen, quel qu'il soit, ne saura être trouvé, qu'au sein d'une convention internationale.

C'est pourquoi nous nous sommes hasardé à en présenter un projet devant le Congrès international d'Hygiène réuni à Turin, dont voici les conclusions :—

La syphilis est le plus grand des maux qui affligent l'humanité ;

Donc, la société a le droit, le devoir même d'employer toutes les mesures, encore les plus rigoureuses, pour la persécution de ce terrible ennemi.

Une convention internationale doit arrêter les moyens de combattre généralement et avec efficacité la syphilis.

Il n'y a point de maladies honteuses, il n'y a que des maladies dangereuses.

Les rapports sexuels n'étant qu'un des moyens de contracter la syphilis, l'enquête et l'inspection de ceux que l'on soupçonne d'être syphilisés n'implique nullement le déshonneur, l'humiliation ou le blâme.

Avoir contracté la syphilis, ou l'avoir héritée, c'est être malade d'une maladie, dangereuse pour soi-même et pour les autres, ce n'est point du tout être déshonoré.

L'inspection, l'examen, et le traitement forcé dans des hôpitaux spéciaux ou à domicile sont les seuls moyens à opposer à la propagation de la syphilis.

La surveillance la plus rigoureuse, et les examens les plus scrupuleux et aussi fréquents que possible doivent garantir l'état sanitaire des filles publiques, et

celles que l'on reconnaîtra infectionées devront être contraintes d'entrer sans retard dans les hôpitaux.

Le même examen, renouvelé à plusieurs reprises, doit être fait aux soldats, aux marins, aux agents de police, aux gardes de douane, aux ouvriers des fabriques, à toutes les corporations enfin, où l'on puisse rendre practicable cette mesure moyennant des réglements supérieurement surveillés.

Aussitôt qu'un régiment, un bataillon, ou un détachement militaire arrive dans une ville ou dans un bourg, inspection doit être faite à tous les soldats, caporaux, musiciens, sous-officiers et officiers.

Ce même examen subiront les marins, de la marine militaire ou marchande, de même que les sous-officiers et les officiers de la flotte et les capitaines des bâtiments lorsqu'ils entreront dans un des ports des pays engagés, par la convention internationale pour la persécution de la syphilis.

Tout malade, homme ou femme, qui entrera dans les hôpitaux spéciaux pour le traitement de la syphilis ne pourra en sortir que lorsqu'il sera tout-à-fait guéri, la guérison devant être constatée par une consultation de deux médecins, au moins.

Tout malade, des deux sexes, qui entrera dans les hôpitaux généraux pour le traitement de n'importe quelle maladie, sera d'abord examiné attentivement sous le point de vue de la syphilis, et il ne pourra en sortir qu'après constatation faite de ce qu'il n'est pas syphilisé, ou de ce qu'il est tout-à-fait guéri.

Tout médecin a le devoir professionnel de déceler tous les syphilitiques par lui reconnus, sauf le cas de ceux qui cherchent à se traiter régulièrement en donnant des garanties de ne pas propager la maladie.

Tout médecin a le droit professionnel de dénoncer à la police sanitaire les gens qu'il reconnaîtra, même en passant, comme des syphilitiques, à cause des marques bien évidentes au visage, aux yeux, aux lèvres, &c. Ces gens-là subiront immédiatement l'inspection, et ils seront envoyés à l'hôpital, s'ils ne donnent point l'assurance suffisante de faire enrayer le mal.

Tout pharmacien est tenu, sous peine d'amende et de prison, de dénoncer ceux qui cherchent des médicaments contre la syphilis, s'ils ne se présentent point munis de la prescription d'un médecin.

Toute sage-femme doit faire appeler sur le champ un médecin pour soigner aussi bien l'accouchée comme le nouveau-né, qu'elle reconnaîtra atteints de syphilis, et en même temps elle est chargée de surveiller les nourrices, et de les examiner à plusieurs reprises, pendant qu'elles allaitent leurs nourrissons.

Toute femme a le droit de dénoncer l'homme qu'elle soupçonne lui avoir contagié la syphilis, cet homme étant tout de suite examiné.

Tout homme a le droit de dénoncer la femme, demoiselle ou veuve, qu'il soupçonne lui avoir communiqué la maladie ; mais il sera tout de suite examiné, restera à l'hôpital si la maladie est reconnue, et la police sanitaire tâchera de suite de prendre connaissance de l'état de la femme dénoncée.

On aura recours aux femmes médecins pour ces enquêtes ; et si par hasard il n'y en avait point au pays, on chercherait à instruire en cette mission les sages-femmes et des infirmières, quoique ce personnel ne donne pas assez de garanties.

Par rapport aux femmes dont la bonne réputation puisse être ternie par de semblables démarches, on agira avec tous les ménagements, avec toutes les précautions et toute la prudence, l'autorité de police sanitaire devant disposer des moyens pour arriver à reconnaître tout ce que la dénonciation peut avoir de probable ou d'impossible.

Une loi est à faire pour punir de la commination des peines les plus sévères

tout homme qui fera de fausse dénonciations à l'égard d'une femme à laquelle il n'aura jamais eu affaire.

Il est absolument indispensable de créer des lois et des règlements qui autorisent à tenir sous la surveillance de la police sanitaire, pour être examinés régulièrement, tous les hommes qui s'adonnent au coupable vice de la pédérastie ou à quelqu'autre exercice infâme qui puisse communiquer la syphilis.

Personne ne pourra se marier sans présenter le certificat délivré par un médecin ou par une femme médecin, constatant la bonne santé à l'égard de la syphilis. De pareils certificats seront exigés pour être admis aux charges publiques, ou à l'avancement dans n'importe quelle carrière.

Les gens bien placés, les gens du monde rendront un énorme service à l'humanité en donnant l'exemple de la facilité et de la docilité de se soumettre aux examens, indispensables pour qu'on puisse leur délivrer les dits certificats.

Cela fait, avec persévérance et rigueur, il ne restera plus de syphilis que dans le mariage, et celle-là est insaisissable aux moyens dont la police sanitaire peut dignement disposer.

Pour ces cas-là il n'y a plus que la ressource morale de porter à l'esprit de tous les époux la conviction que, quand même ce serait une honte de s'avouer syphilisés, ce serait une honte bien plus blâmable encore que de transmettre à leurs enfants cette terrible maladie, de sacrifier toute une génération immolée à un préjugé de fausse pudeur.

Ce n'est pas un crime que d'avoir la syphilis, c'est un malheur; la transmettre sciemment, c'est ce que l'on peut nommer une action criminelle contre l'humanité, et un attentat à la santé d'autrui.

Chercher à chasser, à poursuivre cette terrible maladie, ce n'est humilier personne; c'est garantir le présent et l'avenir de la société.

Si quelqu'un se présentait atteint de la peste, de la fièvre jaune, du choléra, ou de la variole, on prendrait tout de suite des mesures pour éviter la contagion. Pourquoi agira-t-on autrement envers les syphilitiques, ou même envers ceux qui ne sont que soupçonnés d'être contagiés de cette terrible maladie, dont les ravages dépassent ceux de toutes les autres maladies réunies?

La société doit agir énergiquement, tous les pays se prêtant un appui réciproque!

Ce n'est pas du lyrisme, ce n'est pas non plus de la fantaisie.

Notre projet n'est pas irréalisable; il est très-difficile à réaliser. Nous le connaissons bien. Mais avec de la persévérance et de la bonne volonté, on en viendra à bout.

Tout travail commencé est sur le point d'arriver à sa fin. Ce qu'il faut c'est ne pas faiblir, c'est ne pas se décourager.

L'idée du christianisme, *si parva licet componere magnis*, l'idée du christianisme a eu de bien plus grandes difficultés à vaincre, elle fut bien plus farouchement persécutée que ne le sera l'idée de chasser la syphilis; mais, comme c'était une idée humanitaire, elle triompha de tous les obstacles, de toutes les entraves, de toutes les persécutions, de tous les ennemis, malgré leur puissance.

Soyons donc les apôtres de cette idée bienfaisante; et nous sommes sûrs qu'en agissant de la sorte on parviendra à chasser la syphilis, ou du moins à la faire reculer dans le sanctuaire du mariage où elle devient insaisissable, mais d'où la moralité pourra aisément la bannir.

Ne sera-ce pas un grand bienfait pour l'humanité?

Nous n'aurons plus à craindre pour nos enfants les ravages de ce terrible fléau,

nous saurons que nos arrière-petits-fils en seront indemnes. La race humaine reprendra sa vigueur première, et le doux rayon du soleil d'amour ne sera plus obscurci du nuage d'un soupçon.

La syphilis ne peut pas être l'éternel partage du genre humain.

Débarrassons-nous-en!

The Prevention of Venereal Disease.

Dr. A. L. Gihon, Med. Dir., U.S. Navy.

Day by day preventive medicine assumes greater prominence. Popular interest has removed it from professional seclusion, and the responsibilities of the State in its behalf are gradually becoming recognized.

Quarantines are established in every port against the vessel or traveller suspected of plague or yellow fever. Both hovel and palace, in which the first pustule of variola is perceived, are isolated by a sanitary cordon. Upon whatever the diphtheritic germ may have been deposited is buried or cleansed by chemical agents. The sanitary inspector is prompt to close cesspools, and wage war upon whatever else offends the senses. Why, then, should that more terrible poison which the infected woman disseminates from her well-known haunt be beyond his ken? Why should the diseased man be allowed to mingle unrestrained in every crowd, and have access to our houses, polluting whatever he touches with hand or lip? If the wretched harlot, licensed by public tolerance to ply her calling in the most frequented thoroughfares, or the heedless youth, untaught by parent, impelled by the physical cravings of his new manhood, attracted by meretricious allurements, which no one attempts to conceal, have no claims upon society, the innocent maiden who becomes that young man's wife surely has the strongest possible claim to be protected from that hideous evil, for the existence of which society is responsible.

Were it true that venereal diseases were only consequent upon illicit sexual congress, it might be well to let their victims reap the harvest of their sin. So long as the only remedy possible was believed to be the recognition, license, or regulation of the trade of prostitution, it was not strange that Christian people should shrink from contemplation of the horrible alternative.

But let the facts be known that the ruin wrought by venereal disease reaches far beyond the original transgressor and soils the pure and guiltless, and that no degree of personal rectitude can protect the dweller in a large city from chance defilement, and the question of protection falls to the same plane with the protection against small-pox, scarlet fever, or diphtheria, especially when the further fact is considered, that those who practise prostitution have been taught by personal interest the lesson of sanitary self-protection; so that the risk of infection by them is to-day less than by the thousand irregular or accidental channels of propagation.

The attempt to enumerate the possible ways in which a syphilitic may convey the poison to others has been derided and alleged as evidence of a groundless syphilophobia, but were every physician to contribute from his professional experience a single instance of undoubted chance infection, it is believed a longer list would be collected than has ever yet appeared. Since the cutaneous and buccal

syphilides are known to be communicable, it requires no effort to conceive of the dangerous defilement of sheets, towels, napkins, clothing, and whatever else may have been in contact with an eczematous surface; of that of drinking vessels, forks, spoons, and other articles pressed by diseased lips, or of the danger of succeeding a syphilitic predecessor in the hands of the barber, hairdresser, or dentist. Fournier has called attention to the "singular power of irradiation and expansion there is in the syphilis of nurses and nurslings," and he illustrates his statement that "nothing is more dangerous than a syphilitic child in a household," by numerous instances within the experiences of himself and colleagues:—"A syphilitic nurse came into a young family and infected a child, the nature of whose morbid symptoms was not at first recognized, as usually the case among those inexperienced in matters of this kind. The child infected, first, its mother; secondly, its grandmother; thirdly and fourthly, two servants of the house, who were absolutely respectable and virgins; and the young wife, in her turn, transmitted contagion to her own husband, a few months later."—"A young syphilitic married and infected his wife. The child born of this marriage was put to nurse and infected the nurse. She, in turn, transmitted syphilis to her own child, then to another nursling, and two months later to her husband."—"A young married syphilitic, whose case is related by Hillairet, infected his wife and newborn child, which latter infected its nurse, and when subsequently confided to the care of its maternal grandparents, it transmitted the contagion to both of them by means of the feeding bottle, which they were accustomed to apply to their own lips." And Drou describes ten syphilitic contaminations from one nursling, the nurse infected by the syphilitic child having transmitted the disease to three other children whom she suckled, each of these to its own mother, and each of these wives to her own husband.

Statistics having been given as to the large amount of syphilis in the army, navy, and mercantile marine of the United States, and in the British Navy, the author proceeded:—

The public charities of the great cities exhibit a sufficiently alarming, though by no means accurate, proportion of these diseases among the poorest classes, but where can the evidence be found of their prevalence among the higher orders of society? At the hot springs and other health resorts of Europe and America they may be discovered seeking relief for chronic rheumatism, for scrofula, for affections of the nervous system, for obscure cutaneous diseases; for everything, indeed, but the venereal fact. If every reputable practitioner of medicine could be induced to tabulate the facts of his professional experience, the public would realize that Gress and Sims, Sir Thomas Watson, Sir James Paget, Sir William Jenner, and Mr. Simon did not exaggerate when they stated of their own knowledge of the ravages of these diseases among pure women and innocent children, that their victims were numbered by thousands. What physician, indeed, has not known at least one chaste girl married to an unsuspected venereal leper, her own body made loathsome, her life tortured and sacrificed, and her disease transmitted to puny, deformed, and suffering offspring? In children's hospitals, and at the specialist clinics in any large city, these little unmistakable witnesses mutely testify to their inheritance. How extensively the youth of the country are affected by maladies of this nature is evidenced by the returns of the office of the Surgeon-General of the United States Navy, showing that of the applicants for enlistment as apprentices during the year 1879, twenty per thousand—these being lads under seventeen years of age—were rejected for venereal affections, twelve of the twenty

having syphilis; while, at an educational institution under the control of the Government, it was found that, within two years after graduation, not one in five had escaped some form of venereal lesion. Of what other human malady can this be said? If preventive medicine has any office, surely it is to eradicate this pestilence which is undermining the physical foundations of civilized society.

The Section of State Medicine has, therefore, wisely included the prevention of syphilis among the great problems which sanitary science has to solve. The solution of this problem has hitherto been hindered by the efforts of those who have stigmatized it as an attempt to foster prostitution, deliberately ignoring the fact that its actual aim is to make marriage safe, and to protect the ignorant and innocent and helpless from a danger they can in no other way escape. The committee of the American Public Health Association, charged with the duty of attempting this solution, have recognized the insufficieney of any system of mere registration and compulsory examination of prostitutes, since no account is therein taken of the men, who are often the original contaminators of the women, and the real propagators of the disease; and since prostitutes, except of the very lowest orders, may be safely trusted to take care of themselves. Contagious Disease Acts, involving the sanitary and police control of public women, have undoubtedly accomplished a large measure of good.

This is conclusively shown by the following summary from the last report of the Director General of the British Navy, published by ordor of the House of Commons, August 24, 1880:—

Ratio of Syphilis in the Navy of Great Britain from 1860 *to* 1870 *inclusive, at Ports under the Contagious Diseases Acts, contrasted with that at Ports not under these Acts.*

Period.	Ratio of cases per 1,000 Force.					
	At Ports under the Acts.			At Ports not under the Acts.		
	Primary.	Secondary.	Total.	Primary.	Secondary.	Total.
No Acts in force 1860–63	...	...	75·02	...	...	70·05
Act of 1864 in force . . . 1864–65	...	...	79·12	...	...	100·00
Acts of 1866 & 1869 in force 1866–70	34·72	12·47	47·19	59·14	25·6	84·74
,, ,, ,, ,, 1871–79	28·79	11·10	39·89	72·27	21·8	94·08

Public decency and public morals commend the restriction of the scandalous license-permitted abandoned women in American and British cities. Under the present system of inaction and indifference, juvenile whores of tender years hunt in couples in the crowded streets of Boston, New York, and Philadelphia, and schoolboys are enticed into spurious shops, where, for a few cents, they are initiated into the mysteries of vice and the miseries of disease. But the sanitarian is only incidentally a moralist, and must not be diverted from his legitimate mission of waging war against disease as the enemy of mankind. If it be accounted a moral wrong to shield an abandoned woman from the consequences of her sin, though a thousand were thus saved it would weigh nothing against the one marriage bed protected from venereal pollution, the one pure wife saved from physical ruin, the one little child spared a blighted, suffering body.

The proposition of the Committee of the American Public Health Association, endorsed by the very large majority of the members of that body, contemplates

"the enactment of a law constituting it a criminal offence to knowingly communicate, directly or indirectly, or to be instrumental in communicating, a contagious disease, such as small-pox, scarlet fever, or venereal disease; and giving to Boards of Health, and to the State and Municipal Health Officials under their control, the same power in the prevention, detection, suppression, and gratuitous treatment of venereal affections which they now possess in the case of small-pox or other contagious diseases." The committee have been subsequently charged with the suggestion of a project of State law and municipal ordinance which can accomplish this object, and are now in consultation with prominent members of the bar for this purpose. The practical difficulties in the way are not few, but they are not insuperable. Only a small number of offenders will probably ever be brought to punishment; but this is true of other violations of law. Abandoned women may be tempted to make false accusations; but this will operate to deter men from encountering this additional risk. Ignorance cannot be pleaded in excuse, since it will become the duty of the physician to instruct his patient of the harm he is capable of inflicting, and to exact of him that voluntary seclusion which may be necessary to protect others from the risk of contamination by him. It would not be desirable, if practicable, to require the confinement of venereal patients in hospitals, though these should be provided as voluntary resorts for gratuitous treatment. In the naval and military services it is possible to make the concealment of disease an offence, and to require medical officers in their sanitary capacity to be informed of the physical condition of every individual under their charge; and it is not exacting too much of every physician to require such a rigid inquiry into the antecedent history of every case of communicable disease as will assist in the discovery and extermination of its cause. In this there need be no violation of professional confidence. When it is understood that the communication of a venereal disease is a crime against society, no code of ethics will excuse the physician's neglect of his duty, any more than it does now in the case of small-pox; while the syphilitic who infects a woman, however degraded, or he who marries and contaminates a pure woman and begets a diseased child, has no right to cloak his infamy under a medical diploma. It will add to the responsibilities of the physician's vocation; but he who has accepted the mission of health among his fellow men in any other spirit, or for any other purpose than self-enrichment, will not scruple to labour zealously in battling against this most dread form of preventible disease.

On the Measures by which to prevent the Diffusion of Syphilis.

Mr. Henry Lee, London.

Dr. Ferguson, many years ago, observed in the Peninsula that the English soldiers suffered much more severely from syphilis than the natives. This he accounted for by the fact that syphilis was much more generally diffused among the natives than among the English, and that, consequently, they were, in some measure, protected either by hereditary transmission of, or by acquired, disease. Experience will, I think, fully confirm Dr. Ferguson's idea—namely, that those who are born of syphilitic parents, and those who have shown syphilitic symptoms in childhood, are less likely to contract the disease again; and if they do so it is in not nearly so pronounced a form. And it is now, I believe, universally admitted

that if a person catches the disease a second time himself, the symptoms are of a milder character. There is often, then, no enlargement of the inguinal glands, the characteristics of the primary sore are less pronounced, and the subsequent manifestations are modified. By one infection the patient is, in fact, more or less "syphilized" in the proper acceptation of the term.

For the purposes of this communication I use the word "syphilis" to mean a constitutional disease, commencing, when first acquired, by an increase of substance, assuming a very defined shape and outline, and very slowly, if at all, running on to suppuration. The characters of this primary affection are much better developed upon the skin than on the mucous membrane. It is accompanied, or soon followed, by a corresponding affection of the lymphatic glands, which show the same increase of substance, the same defined outline, and the same tendency to remain indurated, without the formation of matter.

In the local venereal sore, on the other hand, there is always some loss of substance. Ulceration and suppuration are among its characteristics, and it frequently leaves a permanent depression on the skin. I do not mean to say that in the primary syphilitic sore pus is not formed; but it is so, if at all, after a long interval and in small quantity. If a vaccine vesicle be allowed to run its course, after the scab is formed some suppuration will take place, but one of its essential characteristics is that in early stages it does not suppurate. Real syphilis may therefore, as I believe, be distinguished in its original from the local venereal sore, and this distinction must be borne in mind in considering how far one attack of syphilis may prevent another.

Now, physiologically, it is conceivable that a community might all be inoculated in their infancy with true syphilis, and in such a community we might have every reason to believe that if syphilis subsequently appeared among them it would be in a very mild and modified form.

If the extinction of syphilis were the only question, the inoculation of children might be reasonably entertained. The disease in them is comparatively so mild and so easily cured, that an individual might, as far as physical suffering is concerned, be a gainer by being inoculated in infancy. This is, in fact, the principle upon which inoculation with the small-pox poison was formerly practised.

No amount of inoculation with the secretion of the local suppurating sore will, in my opinion, give any immunity from syphilis: the evidence with regard to this I will not here repeat, as it has often been published. I have, however, no doubt but that the repeated irritation of the skin, either by blisters, or by a stimulating ointment, or by mosquito bites, will in time render the skin less disposed to take on any fresh action, and consequently less liable to be the seat of any fresh syphilitic manifestations.

When "syphilization," as it was then called, was first introduced, the inoculations were made with the secretions of the local venereal sore, and for this obvious reason, that the secretions of the uncomplicated primary syphilitic sore are not as a rule inoculable upon a syphilitic patient. I may here, perhaps, be allowed to allude particularly to this subject, as, I believe, I first pointed out that, although the secretions of the primary indurated sore are not, as a rule, inoculable upon a syphilitic subject, yet that if such a sore be irritated it will then produce an inoculable secretion. But the inoculation so produced partakes of the character of the affection from which it is derived. It is not the suppurating sore, with loss of substance above described, which is the result; but it more resembles a blind boil with increase of substance. It has not, on the other hand, the defined characters of the

primary sore, but it in some degree resembles it. The induration, instead of terminating abruptly, gradually diffuses itself into the surrounding parts, and is less distinctly marked than in the primary sore. The tendency is to run much more rapidly into suppuration and ulceration, and there is not the same period of incubation.

I have the drawing of some inoculations made from the mucous secretions of a syphilitic patient upon another patient also syphilitic. There was not the slightest appearance of any syphilitic disease in the part from which the secretion was taken, and the greatest care was exercised by Mr. Gascoyen and myself in performing the experiment. The inoculations showed a decided increase of substance, and resembled boils passing into a state of suppuration. The appearances produced were in these respects quite different from the sharply defined ulcerations which result from inoculations with the secretions of the local venereal sore on the one hand, and from the defined induration of the true primary sore on the other. The drawing illustrates two points :—

I.—The characters of the pustules produced by the inoculation of syphilitic matter on a syphilitic subject; and

II.—The inoculability of the secretions of a syphilitic subject, taken from a part where there is no apparent syphilitic disease.

This experiment shows, then, how readily the mucous secretions of a syphilitic patient may be the means of communicating disease, and throws great light upon Hunter's celebrated experiment, so touchingly and eloquently alluded to by our learned President in his opening address, in which Hunter inoculated himself with what he conceived to be the gonorrhœal discharge of another person. The result in this case was a primary syphilitic sore, which in its turn was followed by secondary symptoms lasting some three years.

The conclusions which he drew from this experiment did not coincide with the theories of some eminent syphilographers thirty or forty years ago, and many ingenious suggestions were at different times advanced in order to explain away Hunter's conclusions. But now I think we must admit that in this, as in so many other instances in which he has been supposed to be in error, he was accurate and correct.

Although gonorrhœa and syphilis are distinct diseases, yet they may co-exist, and it is often impossible in chronic cases to say whether a persistent urethral discharge is due to a syphilitic state of a patient's constitution or to simple gonorrhœa. Whatever the origin of the discharge may have been in the patient from whom Hunter inoculated himself, his experiment points to the same conclusion as the one to which I have referred; namely, that the mucous secretions of syphilitic patients may be the means of communicating syphilis.

Long continued and sustained efforts have been made of late years to prevent the spread of syphilis by the action of the Police des Mœurs in France and by the Contagious Diseases Acts in England. As far as the prevention of disease is concerned, these Acts appear to have been framed upon the supposition that by a local inspection a person could be pronounced either contagious or free from disease. Many papers were formerly published, in which "confrontations" were recorded, and the idea once held by Ricord that a primary sore was always the result of a primary sore, was very general. This, however, has now been fully demonstrated not to be the case; and I should say that fully one-half, if not more, of the cases of syphilitic infection, are derived from the secretions of patients who have long ceased to have any primary sores.

Inspector-General Lawson, in his evidence before the Select Committee of the House of Commons on the Contagious Diseases Acts, on the 4th of April last, states that, before the introduction of those Acts, there had been a continuous fall in the number of cases of venereal sores in the stations which are now subjected. The ratio of the fall was as 87 to 146. During two of those years the Acts were applied to a very limited extent; but, taking the six years from 1861 to 1866 inclusive, there was without exception a continuous fall in the cases of venereal sores (Questions 1,662–5).

In the succeeding six years the diminution of venereal sores was from 91 to 54—a diminution under the operation of the Acts practically the same as before the Acts were in operation. With more fluctuations, something like the same ratio has obtained in the districts which have never been under the operation of the Contagious Diseases Acts at all.

Mr. Lawson also says that there is a little more syphilis apparently amongst the females in the subjected stations than among those at stations that are not subjected (Q. 1,796); and that a man runs more danger of catching true syphilis in the subjected than in the unsubjected districts (Q. 1,804).

Mr. Lawson also mentions that in the two periods above mentioned there was a tendency in the relative proportion of secondary symptoms to venereal sores to increase in the stations under the Acts (Q. 1,815). After the introduction of Lord Cardwell's order, this proportion is greatly augmented, and the relative increase of secondaries to venereal sores is 18·6 against 11 in the unprotected districts per 1,000 men.

Now all this evidence goes to show that the diminution in venereal sores has taken place in the non-infecting class. Those who have had much experience in syphilitic affections will, I think, agree with me that a very great diminution in the number of non-infecting sores has taken place in every class of society. Whether this depends upon increased sobriety, upon greater cleanliness, or from the amount of attention that has been given to the subject, I will not at present stop to inquire; but from my own experience I should say that, whereas the non-infecting sore was formerly in the proportion of something like three to one, now the proportion is nearly reversed. But, in making this statement, I would be understood to include in the infecting class cases where, in consequence of previous disease, the later inoculations have produced modified results.

After a very long inquiry, the municipality of Paris has determined to abolish the action of the Police des Mœurs, and the natural inference is that they found that police supervision in Paris has not diminished syphilis. In England, whatever other advantages may be attributed to the operation of the Contagious Diseases Acts, the stern fact remains that under their operation syphilis has scarcely, or not at all, diminished. In comparing the Army Returns for 1866—before the earliest existing Acts were in operation—with the year 1878, we have an actual increase of 2 cases of secondary syphilis per 1,000 men in the whole home army (Q. 1,245). Before these Acts came into operation, the number of admissions for secondary syphilis of the whole home army had been reduced to 24·77 per 1,000 men. But, taking all the years from 1866 to 1878 inclusive, the average admission was 25·79; and, taking the admissions for the years 1876 and 1878, the average is higher than for all the years from 1866 to 1878 inclusive (Q. 1245–8). So far, then, as these statistics go, secondary syphilis would appear to be on the increase, taking the home army as a whole. Now, as primary and secondary syphilis necessarily bear a relation to each other, the inevitable conclusion is that, whatever other

advantages the operation of the Contagious Diseases Acts may have conferred, it has not diminished primary syphilis.

Inspector Anniss has given in evidence, that in Plymouth and Devonport the number of recognized prostitutes had greatly diminished, and the number of improper houses had decreased; and it probably is so in other districts, protected and unprotected. The greater decency and order which now prevail in many towns are probably the result of other laws not connected with the Contagious Diseases Acts; but, however this may be, we are still brought face to face with the fact that, in spite of the operation of Lord Cardwell's order, and in spite of the Contagious Diseases Acts, syphilis has not materially, if at all, diminished.

One explanation of this appears to be furnished by some further evidence of Inspector Anniss. He states that the fear of being brought under the Acts is a most valuable deterrent. In the case of young people they are ordered to depart. Now there can be no doubt that, with regard to a particular district, there would for a time be less apparent risk of infection under these circumstances. The fear of the police and compulsory emigration deter many from attempting to remain in a protected district where the regulations are thoroughly carried out. Still, the stubborn fact remains that no diminution of syphilis can be traced to the operation of the Contagious Diseases Acts.

The way in which we have legislated with regard to the cattle plague is to deal with the disease where it is found. If all suspected animals were driven from a particular district into neighbouring parts, such a proceeding would not tend to diminish the disease as a whole, although that particular district might for a time be comparatively free. The real way of preventing the spread of disease is to deal with it where it is found, and the first requisite for the cure of syphilis is undoubtedly sufficient hospital accommodation. It requires, I think, no lengthened argument to prove that women would much rather go into hospital voluntarily than under coercion, especially if they believe that they are to be forcibly detained. If there were sufficient hospital accommodation more women would, I believe, enter the hospital voluntarily than under the present system. At present, after a woman has been found to be diseased, a certificate to that effect is given her, and she is told to go to the hospital. If she does not like the police regulations she has simply to go into another district where she is compelled, or fancies she is compelled, to follow her usual calling.

Many women would readily submit to treatment voluntarily, to whom compulsory examination is most repugnant. This is particularly the case with regard to those who have lately taken up the mode of life, and who therefore are much more likely to have primary syphilis than others who have long passed that stage.

I conclude, therefore, that much of the apparent benefit which has been mentioned as having taken place in the so-called protected districts has been at the expense of other neighbouring districts not protected; and that if sufficient hospital accommodation were afforded the voluntary system would, on the whole, work better than that which obtains under the surveillance of the police.

The question has been asked, and will doubtless be asked again, "Why not extend the operation of the Acts?" But here we are met with another and a very grave difficulty, which would become very much more apparent if the Contagious Diseases Acts were at all generally applied—namely, the impossibility of determining in many cases whether a woman is contagious or not.

With regard to gonorrhœa, Hunter gave it as his matured opinion that it could not by inspection be always correctly diagnosed. Babington, who edited Hunter's

work on "Venereal Diseases," says that it cannot be recognized except by the persistence of the symptoms; and I will venture to say that there is no surgeon who can, on the first inspection, distinguish the inflammation produced by gonorrhœal matter from that which may be produced by other causes.

With regard to syphilis a similar difficulty exists. A syphilitic patient who has no contagious secretion at one time may became contagious at another, and a person who has a well-marked syphilitic eruption on the skin may possibly not be contagious at all. It is through the secretions of the mucous membrane of the vagina, as illustrated in the case already quoted, and through the uterine secretions, that syphilis is very frequently transmitted; and I know of no means by which a surgeon by inspection could say whether such secretion would be capable of producing infection or not. In garrison towns, the women who consort with soldiers may perhaps be pretty well known, but this would not be the case in other districts; and if any attempt were made to extend the operation of the Contagious Diseases Acts at all generally, the most cruel injustice might be produced. It must be recollected that a person once pronounced diseased by a surgeon has no appeal. And from the circumstance above mentioned it necessarily happens that occasionally a person is pronounced to be diseased who really is not, and that others who are diseased are dismissed as sound.

If the Contagious Diseases Acts were at all generally applied to the civil population, persons would constantly be taken up on suspicion who were not contagious at all, and in some instances even innocent married women might be condemned.

With regard to the liability of a person who is apparently at the time free from disease to become contagious from accidental circumstances, I would particularly note the following. It has been a matter of repeated observation with me that the admixture of even a small quantity of blood gives to all venereal secretions a virulence which they otherwise would not possess, and consequently a woman who has had gonorrhœa or syphilis is much more liable to become contagious if she is compelled to resort to her usual mode of life before her natural period of separation is quite concluded. The following cases afford illustrations of this fact:—

A gentleman had had constitutional syphilis before his marriage. Some years after he contracted a very superficial suppurating sore, which healed shortly with local applications only. Unfortunately he had intercourse with his wife before he had much noticed the sore or thought it of any consequence, and before she had perfectly recovered from her monthly period. He had no further trouble, but she had a large number of acutely painful suppurating sores on the labia, perineum, and sides of the rectum, and in fact in every part where the discharge from the vagina had been kept in contact with the parts. In this case the pain was excessive, preventing sleep at nights, and making her dread to pass anything either from the bladder or rectum. The glands in the groin were slightly affected, but subsided without specific treatment. There were no secondary symptoms.

This case affords a striking example of the virulence which may be imparted by the admixture of blood to the secretion of a local venereal sore, and the following in like manner shows the same with regard to secondary syphilitic secretions.

A nervous gentleman, who had never had syphilis, had contact, but not intercourse, with a lady who had not quite recovered from her monthly period. Violent irritation of the glans penis was the result. A lymphatic vessel on one side of the penis enlarged to the size of a common writing quill; one of the corresponding glands in the groin became as large as a chestnut, and painful. Some days after

the frænum on one side became indurated, but there was no induration on the glans penis. A copper-coloured eruption appeared on the patient's body, and this was followed by mucous tubercles on the lips and tongue. It was subsequently ascertained that this lady had had an old syphilitic affection. There had recently been a slight herpetic eruption on one labium, and on one part of the skin of the face.

The disease in this case was communicated by simple contact, without any abrasion of surface. The lady, some days after the contact, was carefully examined; there were then three very small red pimples on the chest, The vagina was perfectly healthy, but there was a copious viscid discharge from the uterus. This lady considered herself perfectly well, and had not the slightest suspicion of having had any disease for some years previously.

It was long before those who were engaged in the regulation of State Medicine were led to appreciate the importance of the admixture of a small quantity of blood with the lymph used in vaccination. The importance of the admixture of blood with venereal secretions still remains to be recognized. This is a source of infection which the Contagious Diseases Acts, as at present administered, do not touch.

In the treatment of constitutional syphilis, Mr. Pearson and Sir B. Brodie both insisted upon the continued and sustained action of mercury, not only during the existence of the symptoms, but for some time afterwards. The cases which I have given, and I may add, common experience, show that it is one thing to remove the symptoms and another to cure the patient. The treatment should, in my opinion, be continued after the disappearance of the secondary symptoms, and until there is a reasonable probability of the patient being cured of the disease. That patients are actually cured in this way, so that neither they or their families present any subsequent symptoms, I have abundant evidence. I now constantly see patients who have been under treatment in former years, who have never had any return of symptoms, and whose families have been perfectly healthy. This is then the most important means of preventing the diffusion of syphilis—namely, to cure each individual who is ascertained to be infected—and for this far larger hospital accommodation is required than exists at present; and, with regard to the operation of the Contagious Diseases Acts, it is my opinion that the women would much more readily submit to a prolonged treatment if they voluntarily go into hospital, than if they are kept there upon compulsion.

The conclusions which I have arrived at are:—1st. That, whatever advantages may be claimed in favour of the operation of the Contagious Diseases Acts in making a protected district apparently orderly, and in preventing annoyance to the public, they have not hitherto been the means of diminishing syphilis. 2ndly. That they have had very little influence, if any, in diminishing gonorrhœa as regards the whole home army. 3rdly. That the diminution of the so-called "primary syphilitic sore" in the army is due in reality to the continued decrease from various causes of the local venereal sore; that a similar diminution may be observed in all classes of society, and in districts where the Contagious Diseases Acts have not been in operation.

When any contagious disease affects a population for the first time it is generally more severe than at any subsequent period, and it is often more virulent in a place where it is not bred, than in a place where the people have almost trained themselves to an immunity from it. After a time, which may be longer or

shorter, the disease diminishes in intensity, and becomes of a milder character, and in some instances dies out. When syphilis was first recognized in Europe it was a very much more formidable affection than at present. Patients now may, and as a rule do, go through the disease without any ulceration or suppuration, and without any indication of it being left on their bodies. This is a state of things widely differing from the ravages described by those who first recognized and described syphilis. The complications which accompany or follow gonorrhœa appear to me also to be milder now than formerly, and the local venereal sore is now seen much less frequently than it was twenty or thirty years ago.

Similar changes of type may be observed in other diseases. Such changes may be noted in our own days, and probably have occurred in every age of the world.

"Ut variant morbi nos et variabimus artes
Mille mali species, mille salutis erunt."

La Surveillance de la Prostitution à Vienne (Autriche) et la Syphilis.

Dr. C. R. Drysdale, London.

On a vivement débattu, en Angleterre, depuis quelques années, la question de la surveillance de la prostitution. La syphilis, triste maladie, est encore beaucoup trop connue parmi nous. Dans le sein du Congrès International de 1875, à Bruxelles, M. Vleminckx père a parlé ainsi: "Je constate une chose: c'est la tendance qu'on a en Angleterre à abolir plutôt les visites des prostituées qu'à les multiplier. C'est, pour le Continent, un grand danger. On pourrait vous accuser, Messieurs les Anglais, d'être les importateurs de la syphilis."

Est-il vrai qu'il existe plus de syphilis parmi nous que sur le Continent?

Examinons les faits. Selon M. Jeannel ("Sur la Prostitution:" Paris, 1874), "L'Angleterre nous (les Français) dépasse de beaucoup quant aux institutions destinées à diminuer les misères et à réprimer les scandales de la prostitution, comme à restreindre la propagation des maladies vénériennes: c'est chez elle que nous avons à chercher les modèles. Chez nous, les bureaux des mœurs, les dispensaires, les hôpitaux des vénériens, dont nous avons eu pourtant l'initiative, restent livrés sans contrôle à la plus complète anarchie." La neuvième partie des prostituées à Paris peut seulement être soumise aux règlements de police. 50,000 cas environ de maladies vénériennes y sont constatés chaque année, et, sur 40,000 prostituées, 36,000 sont clandestines.

Même M. Vleminckx père, en 1875, au Congrès, tandis qu'il prenait la ville de Bruxelles comme "la plus pure en ce qui concerne les mœurs publiques," admettait pour la Belgique qu'il y reste "un point noir qu'il ne nous appartient pas de dissiper —la prostitution clandestine."

La vraie syphilis est de nos jours même beaucoup plus répandue à Paris et dans les autres villes du Continent, qu'autrefois. Voici (Mauriac, "Rareté Actuelle du Chancre Simple," 1876) quel a été chaque année, depuis 1861 jusqu'à 1875, le nombre des affections syphilitiques, tant primitives que consécutives, reçues et traitées dans l'Hôpital du Midi, à Paris. En 1861, 570; 1862, 572; 1863, 503; 1864, 840; 1865, 849; 1866, 799; 1867, 893; 1868, 985: 1869, 1442; 1870, ?; 1871, ?; 1872, 1,265; 1874, 1,578; 1875, 1,554. Comparez le chiffre de 1861 (570) avec celui de 1875 (1554).

"A Vienne (Autriche)," M. Sigmund disait au Congrès de Bruxelles, 1875, "nous avons des visites qui ne sont pas assez régulières ; mais la police fait son devoir d'une manière assez précise." Cependant M. Kraus, de Vienne, au Congrès International à Amsterdam, 1879, laissait voir une augmentation progressive dans les cas de maladies vénériennes à Vienne, dans les hôpitaux de cette ville, entre les années 1850 et 1875. De sa statistique il résultait, que le total de cas dits "syphilitiques," traités dans l'Hôpital Général, l'Hôpital Wieden, l'Hôpital Rodolphe, qui montaient à 3,087 cas en 1867, furent de 4,549 en 1875.

L'Hôpital Général en 1850 traitait 2,078 cas vénériens; et, en 1864, 2,189 cas. L'Hôpital Wieden, en 1850, 354 cas; et, en 1864, 2,030. "Un coup d'œil sur cette statistique," disait M. Kraus, "nous fait voir une augmentation notable d'affections syphilitiques dans les trois grands hôpitaux. On peut soutenir, qu'avec la plus grande probabilité, les médecins particuliers remarquent également dans leur clientèle une augmentation proportionnelle du chiffre de malades affectés de syphilis."

Quand aucune surveillance n'existe, comme actuellement à Londres, et quand les prostituées, comme c'est actuellement le cas ici, sont admises avec facilité dans les infirmeries (workhouse infirmaries), il existe, je pense, vraiment moins de maladies vénériennes que dans les villes comme, par exemple, Paris, où les prostituées craignent d'être mises en prison en cas de maladie. Même à Bruxelles il existe, selon M. Thiry, une assez grande quantité de cas de syphilis. Voilà mon expérience personnelle à Londres. Ainsi pour la diminution de la syphilis je conseille l'admission des malades dans les hôpitaux et l'abolition du système français.

Measures for preventing the Diffusion of Syphilis.

Dr. Henry Arthur Allbutt, Leeds.

In 1866 the Contagious Diseases Act became the law of this so-called free land. It was undoubtedly passed by the Government with good intentions, under the idea that syphilis, the scourge of modern society, would be checked in its progress, and reduced to manageable proportions. Has this hope been fulfilled? I believe not. The constitutional disease—true syphilis—appears actually to have increased. Before the Acts came into force there was a diminution in the amount of this disease, as shown by infallible statistics culled from the "Army Medical Report." This official annual shows that in the British army, from 1861 to 1866, syphilis diminished from 31·26 to 23·39 per 1,000 men. When the Act, however, came into force, in 1866, strange to say, syphilis increased among the soldiers, as demonstrated by a table (which was exhibited) from the aforesaid report.

The naval statistics point to the same result—no diminution of those diseases for which the Acts were specially framed. (Statistics to this effect were quoted.)

Now another strange fact will be noticed in the army statistics—viz., that before the Acts came into force syphilis spontaneously diminished in five years, 1861–1866, from 31·26 to 23·39 per 1,000 men. And yet we have spent over £500,000 of hard-earned public money in order to get rid of a disease which was actually diminishing of its own accord, with the result of having increased it in our army, and, in some years, in our navy.

My attention was first drawn to the uselessness of Acts intended to diminish venereal diseases by the fact that a patient of mine contracted syphilis in Copenhagen, and afterwards gonorrhœa in one of the best licensed house in Brussels. He was very much surprised that, after having taken precautions as he thought, he should twice become diseased. At that time, too, I myself felt rather surprised that it should be so. However, since then I have carefully read the evidence both for and against the Contagious Diseases Acts, and I have decidedly become convinced that these Acts are mischievous, useless, and thoroughly one-sided and degrading.

Dr. Mireur says: "Sad to say, that syphilis is especially caught from registered women." Dr. Potter informs us that, "at least five times out of six, soldiers contract syphilis from registered prostitutes." Lancereaux, in his work on "Syphilis," gives a table very conclusive, in which, if you refer, you will see "that, of 873 men who contracted true syphilis in Paris," 625 obtained the disease from registered women.

Now, if State regulations have been really ineffectual in diminishing syphilis and other venereal diseases, and notwithstanding our efforts they should still be on the increase, the question naturally arises, What is to be done to check them? Are we to allow them to attack our youth, and carry destruction and sorrow into innocent families? Are there no means which can be adopted which will insure the speedy removal of that disease, which is itself a source of other diseases fatal to human life? I believe myself that syphilis, widespread throughout the human race as it is, can be checked, and perhaps ultimately exterminated. This will not, however, be done by one-sided laws, but by a recognition of the true causes which lie at the root of every evil. Syphilis, like other contagious diseases, such as scarlet fever and small-pox, can always be traced. Any person, either male or female, who communicates it to another should be punished either by a fairly heavy fine or by imprisonment. This could easily be done. The medical attendant of the affected person should himself be compelled to report the case to the sanitary authorities of his town or district; the medical officer of health would then trace the case, and cause the person who communicated the complaint to be punished.

I would also prohibit quack practice, by putting a very heavy fine, say of £50, upon any individual unqualified, who treated such contagious cases without reporting them to the sanitary authorities.

DISCUSSION.

Dr. C. M. Giersing, Copenhagen: The laws which we are here to examine are based upon the opinion that a medical practitioner is always able to decide whether a woman is the subject of venereal disease, and whether she is still a danger to the public health; and the regulation of prostitution depends upon the certainty that the inspections suffice to secure this end. But, as the result of many years' study, it appears to me beyond doubt—first, that it is often extremely difficult to detect existing venereal disease, even by the most minute inspections. Second, that women suffering from secondary and tertiary symptoms are capable of infecting men, although free from any local syphilitic signs. Third, that many women, after having been dismissed as healthy, are, notwithstanding, a source of danger to public health. That medical inspection, though so fallible, does some good, by the isolation of a certain number of diseased women, none will deny; but this advantage is more than counteracted by the false sense of security which it

gives to licentious men. These laws somewhat resemble those of quarantine. But what would be said of quarantine laws which detained women on board whilst men were allowed to depart? And how can we expect materially to diminish the ravages of syphilis by isolating women, and allowing the men to go free and infect any number of women? The basis on which such regulation is founded cannot therefore be sustained. When scientific medicine fails by one method to cure and control a disease, that method is abandoned, and a new one is sought for; in this instance science should follow the same path.

Dr. Ladame, Neuchâtel: Though desiring to render homage to those illustrious *confrères* who have led to the adoption of the inspection of prostitutes, with a view to the regulation of syphilis; and though understanding the point of view which has led them to adopt such prophylactic measures, I am bound to maintain that they are in the wrong. In order to abolish venereal disease, regulated prostitution is tolerated, without its being perceived that the two are absolutely opposed to each other. Prostitution is the hot-bed of syphilis; and, through it, it reaches the innocent members of the commonwealth. "Rien n'est si contagieux que l'exemple," wrote La Rochefoucauld. If the several Governments were to adopt and apply the measures which have been recommended by previous Congresses, they would legalize the profession of prostitution, and, instead of arresting the progress of venereal diseases, they would, as it were, patent the very sources of those affections. What physician can, as the result of frequent inspections of the most minute character, guarantee that the women in a licensed house are absolutely free from the risk of communicating disease? Everyone knows how syphilis daily finds its way into these houses notwithstanding the police and the physician. The only prophylactic measures which are really efficacious, and which medicine should recognize or sanction, are those which energetically attack the numberless roots of this social malady which is called prostitution. All I ask is that the public should protect me against it (by its agents) in the same way that it does against the solicitations of a miserable being, who, in a public thoroughfare, clamours for bread.

Dr. Wm. Carter, Liverpool: said he would like to direct attention to two results which invariably follow the application of those laws which have for their fundamental principle the registration and examination of public women. First, a great increase of licentiousness on the part of men; second, successful evasion of the regulations on the part of women. The same principles govern this as other subjects. Increase the value of the thing wanted, you increase the demand for it. With an increased demand you inevitably increase the supply. The laws in question, by professing to secure men from the physical effects of their indulgence, stimulate them to indulge; women, to supply the demand, increase; but, by quotations read from an official report by M. Lanaers, Chief Commissary of Police in Brussels, it was proved that in that city and Paris, by his admissions, women will not submit to the regulations, and that their chief result is the creation of a widespread and most dangerous clandestine prostitution: on which ground chiefly, in the report in question, issued in 1877, he has characterized the regulations as being worse than useless—as being positively harmful. But these very regulations were in 1867 recommended by M. Vleminckx, as being, if not perfect, yet so nearly so as to require but the addition of a few insignificant details to make them so. Dr. Carter next quoted from the tables appended to the "Venereal Commission Report,"

which showed that in Paris a very small number of women could alone be placed on the register, while a large army of clandestine prostitutes—made clandestine by their determination not to submit to the regulations—were actively engaged in spreading disease. With this dangerous kind of prostitution the regulations were chargeable—they confessedly failed to register more than an insignificant fraction of the women, while the very fact of registration encouraged men to indulge. Dr. Carter insisted that the provision of voluntary hospitals would be more likely to do good than enforced registration and examination.

Dr. H. Schoenfeld, Brussels: said that, whilst sharing with his colleague from Neuchâtel the aversion with which every honest man must regard licensed houses of prostitution, he could not with him blame the intervention of the physician in his visits to the prostitutes; he would, on the contrary, enlarge the scope of his mission, and he considered that every man who visits a licensed house in order to satisfy his animal passions, should, in the first instance, submit to inspection. Such a measure would diminish to a large extent the visits to such haunts. In Brussels, the medical inspection which is carried out is admirably administered, and no uncured woman leaves the hospital. He felt, also, that it would be worthy of this Section to condemn by its voice the infected literature and plays of the day, which constantly furnished our youth with lascivious thoughts and allusions.

Dr. T. Carson, Liverpool: considered that if we favoured some of the conclusions which had been placed before us, we should discredit both the profession at large and also this Congress. They were such that no European minister or statesman could adopt.

Dr. Bell Taylor, Nottingham: said there was no necessity for any legislation on the subject; he said these regulations were devised in order to protect men who incurred the risks of promiscuous intercourse from the occasional consequences of their own deliberate folly. He maintained (1) That a proportion of such men escaped disease altogether, and of those who contracted disease the vast majority suffered only from gonorrhœa; and whereas the small proportion who contracted sores had in nine cases out of ten sores of a non-syphilitic character, those who contracted syphilis were in the great majority of cases readily cured; (2) That true syphilis had increased in the army under the operation of the Contagious Diseases Act, and that the Acts fail because only one-seventh of the women who propagate disease can be brought under regulations; (3) That those who are brought under regulations do not attend regularly; (4) That those who do attend, in a vast number of instances are unfit to be examined, owing to the monthly period; (5) That examinations once a fortnight are, in the words of Leon Lefort, "merely absurd;" (6) That it is impossible in the female to distinguish gonorrhœa, the most frequent form of venereal affection, from leucorrhœa, an affection from which the great majority of women suffer; (7) That in women, chancre, the initial lesion of true syphilis, is conspicuous by its absence, or, if present, is such a minute affair as to be constantly overlooked in examinations made expressly for its discovery; (8) That women who have had syphilis give that disease to others through their secretions, though they show no trace of the disease themselves, and that they may remain in this condition for years; (9) That a woman who has once had syphilis does not have it again: she is protected against future attacks, yet, continuing to associate with the same class of men as the one who originally infected her, spreads disease to all who

follow an infected man, though she shows no trace of disease herself, indeed is absolutely free from disease; (10) That disease is spread among the women who are not syphilitic, by their close contact with those that are, during months of hospital confinement, and by the speculum and other instruments used during the periodical examinations; (11) That men, deluded by the false security offered by these regulations, become largely infected, simply because they have been led to believe by the Government that the brothel is a safe resort.

Dr. NEVINS, Liverpool: showed from tables laid before the Select Committee of the House of Commons, and other papers communicated to the Admiralty, or published by the House of Commons, that hereditary syphilis was almost unknown amongst the children working in the manufacturing districts; that the average mortality from it was less than that from thrush; and that the mortality from syphilis at any age was almost the lowest among the sixty classified diseases of the Registrar-General; that the Medical Officers of Health scarcely ever allude to the disease in their official reports; that a report to the Admiralty on the amount of venereal disease in the mercantile marine in Liverpool showed a very small amount (one man in forty-eight), and that there had not been a death at sea from this cause in thirty years. He showed from a Parliamentary return of the Navy that secondary syphilis had risen, and gonorrhœa had more than doubled since the strict periodical examinations had been in force.

Dr. B. KRAUS, Vienna: regretted that up to the present time no approach had been made to any definite solution of the difficulties surrounding the question. The reason of this lay in the absence of the adoption by any Congress of any definite resolutions which might give the Legislature some fixed starting-point of action. Everyone knew that syphilis was spread by clandestine and by open prostitution. This needed no proof. Public prostitution was not so dangerous from this point of view, if there existed laws for securing the efficient registration and examination of all public women. But one thing ought to be definitely declared in the opinion of the Congress—viz., that the only means of accomplishing this object was the establishment of licensed brothels in large and in small towns; and not to allow such women to ply their trade except in these licensed houses. In this way the medical inspector can alone successfully accomplish his work, which was simply impossible when the women lived scattered about in many houses. The registration of women, and the distribution of certificates of health, have been proved by experience to be most efficacious in checking disease; therefore this should be adopted as a resolution of the Congress, and laid before the various Governments as a matter placed by experience beyond doubt. But far more dangerous than public prostitution was clandestine prostitution, for the suppression and regulation of which much had been said and written, but very little accomplished. In Hamburg, Amsterdam, and Vienna he (the speaker) had dealt with this question at the Meeting of the Congress in 1875, 1877, and 1879 ("Ueber die Regelung der Prostitution"). Senator Palassiani, of Naples, brought his proposals, made in Amsterdam, before the Italian Government, and there was reason to hope that his proposals will be carried out. These proposals are founded on the experience that the working classes of the population are the greatest propagators of venereal disease, and that the medical examination of such is not only possible, but necessary, if such disease is to be put an end to. (1) This is already done in the case of soldiers and sailors. But the examination of the man is useless unless

the source of his infection is purified. As soon as the soldier is cured he returns to his old mistress, and from her comes back again to the care of the doctor. This is the ordinary experience of military surgeons. (2) All large factories have already surgeons in attendance; and it would not be a difficult thing to have an examination made of all the hands employed every week or month, without special or any reference being made to the subject of syphilis. (3) The tradesmen who are engaged in the preparation of our food, &c.—should they not also be made free from venereal diseases? Ought it not to be made compulsory that no such tradesman should be allowed to carry on his trade unless he could be proved free from venereal and other infective diseases? (4) Our servants, male and female—should they also not be free from venereal disease? Is it to be tolerated that children should be nursed by girls who are at the time suffering from gonorrhœa or other infective diseases? (5) The most disastrous propagators of syphilis are the wandering domestics, who are to-day in one house, to-morrow in another. But if such knew that on entering into each service they must produce a certificate of health, they would take good care not to leave one house until cured, and thus the employers would be saved much anxiety and trouble. Such certificates should be supplied by official medical inspectors gratis, and by private medical men at a minimum cost. This was a severe means to employ, but there was no other way to avoid the widespread horrors of venereal disease.

Prof. D. G. Van Overbeek de Meijer, Utrecht: Syphilis is a contagious disease. What step should be adopted to abolish it? Simply that by which we deal with any other contagious disease—namely, seek out the infected individuals. If we have failed it is because we have not properly organized our action. But that is no reason for abolishing all sanitary measures against syphilis; it is, on the contrary, a reason for seeking some better organization than that involved in inspection. If you maintain that it is no part of the physician's duty to deal with the propagation of syphilis by the adoption of sanitary measures, you brand public health with impotency, and you forget that it is your duty to watch over the health of the abandoned as well as of the innocent. It is a fundamental error to inflict punishments in this matter. In the first place it will be impossible to attach the blame to the right person, and you will only check public prostitution by increasing clandestine prostitution. He thought that the police should endeavour to abolish the traffic of the *maisons de passe*, but they should, to a certain extent, protect the licensed houses, and see that the women in them did not propagate disease. In the meantime, moralists could seek to diminish the number of those who uphold prostitution.

Dr. Le Grósz de Csâtar, Buda Pesth: said that hygienic considerations should alone regulate our action. Measures must be adopted by which syphilis may be abolished, or at least lessened; and to this end it was necessary that prostitutes should be absolutely confined to licensed houses, where they could always be subjected to a really rigorous supervision.

Prof. G. Pacchiotti, Turin: thought we had lost sight in this discussion of the excellent paper read by that experienced and eminent syphilographer, Mr. Henry Lee. He also wished the Section could arrive at some definite conclusion on this subject. We could discuss all the important points relating to syphilis; but we had better limit ourselves to the subject of its prevention. Two parties stood here in

opposition to each other; one talked of liberty, the other upheld the preventive system which prevailed in Belgium, France, and Italy. This latter had done some good in the last fifty years. It might be that some alteration was necessary, and we are ready to adapt reforms to it. But he would ask Dr. Drysdale, who was here the chief of that party which opposed the prevention of syphilis, to tell us by what practical means he could stay its spread if he allowed all syphilitic men and women to go their own way. He liked liberty, but the health of the people was *suprema lex*. When we prevented small-pox by means of compulsory vaccination, it would be strange if we failed to prevent the spread of syphilis, at least to some extent, by means of vigilance, aided by some uniform, international, hygienic measures.

Dr. ROUTH, London: said that the disease abroad was more severe and fatal than in England, in proof of which he quoted statistics of Dr. Nevins as to the national amount of contagious venereal diseases in seventeen large seaports in Great Britain: Hull, 5·3 per 100; Liverpool, 4·3 per 100; Dublin, 10·4; Cork, under the Contagious Diseases Acts, 10·4; Plymouth, under the Acts, 8·0; Geneva, 9·1; Hamburg, 11·15; Copenhagen, for many years, 18·79; Christiania, 24·7. The disease had not been stopped or diminished by these Acts; on the contrary, under a supposed security, it had increased—extending to the prostitution of men, of which an example was given—so that the Acts, by their debasing effects, led to the commission of greater crimes. No plan had been devised to cure the extension of syphilis, even by foreigners. These only adopted a plan which, even in Paris, had been found so to fail, that it is to be abolished.

Dr. ELDER, Nottingham, said it seemed to him that the main obstacle to carrying out restrictive measures is the difficulty, and, in some cases, the impossibility, not only of diagnosis and differentiating disease when it is supposed to exist, but also of saying when a patient ceases to be a focus of contagion. It was not sufficiently recognized by the profession that, over and over again, both syphilis and gonorrhœa were communicated, when in the former there was not a single trace of the disease, and in the latter the discharge in no way could be distinguished from secretions common to most women.

IV.—MEASURES BY WHICH TO PREVENT THE DIFFUSION OF HYDROPHOBIA.

Des précautions pour prévenir la Diffusion de l'Hydrophobie.

Dr. H. VAN CAPPELLE, The Hague.

Combattre la rage c'est prévenir l'hydrophobie. En prenant cet axiome pour point de départ, je me propose de raconter la génèse d'une loi promulguée dans les Pays-Bas en 1875, et les effets de cette loi pour diminuer les cas de rage, qui se multipliaient dans ce pays avec une effrayante rapidité avant sa mise en vigueur.

En 1874 le Gouvernement hollandais, ému par la fréquence des cas de rage chez les chiens et par plusieurs cas d'hydrophobie, qui en étaient les conséquences, résolut de prendre l'initiative pour tâcher de mettre une fin, s'il était possible, à cette maladie terrible.

A l'école vétérinaire d'Utrecht 22 chiens enragés avaient été présentés en 1873.

tandis que dans les années 1864 jusqu'à 1873, c'est à dire dans les neuf années précédentes, seulement 79 chiens atteints de cette maladie avaient été présentés.

En 1869 deux cas mortels d'hydrophobie humaine avaient été constatés, en 1870 deux cas, en 1871 encore deux cas, en 1872 trois cas, en 1873 un cas, en 1874 cinq cas ; dans six années 15 cas de mort par suite de la terrible maladie, qui peut être la conséquence de la morsure par un chien enragé ! En outre quantité de personnes avaient été mordues sans que la morsure ait eu des conséquences fâcheuses.

Il n'existait pas de loi, qui donnât au Gouvernement la pouvoir d'agir avec succès. Dans plusieurs communes il y avait bien de vieilles ordonnances, mais elles étaient peu efficaces et mal éxécutées. Ici on ordonnait la muselière pendant la canicule, là il y avait un impôt sur les chiens ; partout on abattait les chiens enragés, mais les mesures générales faisaient défaut et par les mesures isolées, même si elles avaient été plus efficaces, on n'aurait pas atteint le but proposé parce qu'il n'y avait pas d'unité dans ces mesures. On ne pouvait espérer de combattre la rage avec succès que par le force d'une loi.

Mais quelles étaient les mesures qu'il fallait prescrire dans cette loi ? Les savants dans les différents pays n'étaient pas d'accord sur ce point. Personne ne niait que la rage ne soit une maladie contagieuse, dont le venin, introduit dans le sang d'un animal sain, produit la même maladie ; mais plusieurs vétérinaires, niaient que la rage se propage exclusivement par la contagion, et prétendaient qu'elle se développe par suite de certaines conditions, comme le climat, les saisons, l'orgasme génital, la soif et la faim, l'influence de l'alimentation avec des matières putrides, la colère et la souffrance.

J'avais été frappé par la lecture d'une étude trés-remarquable sur la rage, publiée en 1874 par le savant inspecteur des écoles véterinaires de la France, M. Bouley, dans le "Dictionnaire encyclopédique des sciences médicales." Dans ce traité M. Bouley cite des exemples de chiens, devenus enragés, qui, d'après leurs maîtres, n'avaient pas été mordus et ne pouvaient pas avoir été mordus, parcequ'ils avaient été enfermés et ne sortaient jamais seuls. Plus tard (dans un cas après dix ans) des domestiques firent l'aveu qu'ils avaient menti et que les chiens, devenus enragés, avaient été réellement mordus pendant la promenade.

"La contagion seule est prouvée," dit M. Bouley. "La contagion, voilà donc la cause certaine de la rage, et probablement la cause unique, et quelles que soient les dissidences qui peuvent encore persister sur ce dernier point, comme l'accord est unanime pour reconnaître qu'elle est la cause prépondérante, c'est elle surtout que l'on doit avoir en vue dans les prescriptions de mesures sanitaires que comporte cette redoutable maladie."

Frappé par la lecture du traité de M. Bouley et ayant appris par l'expérience qu'une autre maladie contagieuse, la pleuropneumonie contagieuse des bêtes à cornes, dont on avait aussi affirmé qu'elle pouvait se développer par d'autres causes que la contagion, se propage réellement par la contagion seule, j'osais affirmer que, selon toute probabilité, la rage, dans les temps où nous vivons, a pour cause unique la contagion et qu'il devait être possible de la combattre avec succès en exterminant tous les chiens enragés et en empêchant les chiens qui auraient pu être mordus par eux, de mordre à leur tour.

Je ne raconterai pas toutes les péripéties de la génèse de la loi hollandaise de 1875. Les mesures proposées ont trouvé beaucoup d'objections, surtout dans les Etats Généraux et la loi a été promulguée le 5 Juin, 1875.

Voici les mesures principales qu'elle prescrit :—

1.—Le propriétaire d'un chien ou d'un chat enragé ou mordu par un animal enragé, est tenu de tuer ou d'enfermer et d'attacher cet animal, et d'informer le bourgmestre (le maire) ou le commissaire de police de ce qui est arrivé. De même il doit informer ces autorités, s'il a été dans l'impossibilité de tuer ou d'enfermer et d'attacher l'animal.

2.—Lebourgmestre ou le commissaire de police fait examiner par un vétérinaire tout chien ou chat suspect de rage, ou mordu par un animal enragé, ou les cadavres de ces chiens ou chats.

3.—Si par l'examen il est constaté que le chien ou le chat est enragé ou mordu par un animal enragé, ou si le vétérinaire a raison de le suspecter, l'animal est abattu. Le bourgmestre ordonne que, pendant quatre mois, tous les chiens dans la commune, qui se trouvent hors des maisons, soient fournis d'une muselière, dont le modèle est prescrit par le Ministre de l'Intérieur. Le bourgmestre adresse une copie de cette ordonnance à ses collègues des communes adjacentes et au Commissaire du Roi (le Gouverneur) de la province, qui peuvent prescrire la même ordonnance—les bourgmestres pour leur commune, le Commissaire du Roi pour la province entière ou pour une partie de la province.

4.—Les agents de police ont le droit d'entrer dans les maisons, afin de prendre ou de tuer un chat enragé ou suspect.

5.—Les cadavres de chiens ou de chats enragés sont brûlés ou enfouis selon les règles prescrites pour les cadavres d'animaux morts d'une maladie contagieuse. Les objets, qui ont été en contact avec un animal enragé, sont brûlés ou désinfectés.

6.—Il est défendu de maltraiter des chiens ou des chats. Néanmoins le propriétaire ou locataire d'une maison ou d'un jardin a le droit de tuer ces animaux, s'il les trouve sans surveillance chez lui.

Le 1er Septembre, 1875, cette loi a été mise en vigueur. Voyons maintenant ses effets sur la diffusion de la rage dans les Pays-Bas.

Chaque mois le Ministre de l'Intérieur reçoit un rapport des autorités provinciales sur tous les cas de rage qui ont été observés, avec tous les détails possibles. C'est d'après ces rapports que j'ai marqué sur les cartes du pays, que j'ai l'honneur de joindre à cette note, les lieux où des cas de rage ont été constatés, en 1875 (les quatres derniers mois de l'année), et dans les années 1876–1880.

Depuis le 1er Septembre, 1875, jusqu' à la fin de cette année 41 cas de rage ont été observés dans 36 communes. On voit sur la carte que ces cas sont disséminés dans presque toutes les provinces, mais qu'ils étaient nombreux dans les provinces de la Hollande septentrionale et méridionale, avec leurs grandes villes, Amsterdam, Harlem, Leide, La Haye, Rotterdam, où, comme dans la plupart des grandes villes, pullulent les chiens vagabonds, parmi lesquels la rage se propage facilement. Dans cette population canine vagabonde les prescriptions de la loi ont fait une trouée énorme. Ces chiens vagabonds, qui ne portaient pas la muselière, ordonnée là où il y avait eu un cas de rage, ont été abattus en quantité. On en voit l'effet sur la carte pour l'année 1876.

Dans les douze mois de cette année on a observé 55 cas dans 49 communes, dont 9 seulement dans les deux provinces de la Hollande. Les cas se sont concentrés principalement dans les provinces limitrophes de la Belgique, le Brabant septentrional et le Limbourg.

En 1877, pas plus de 14 cas se sont présentés dans autant de communes, situées exclusivement près de la frontière belge et prussienne.

Sur la carte pour l'année 1878 on voit seulement 4 points noires, indiquant 4 cas de rage dans des communes peu éloignées de la frontière.

LA RAGE DANS LES PAYS BAS PENDANT LES QUATRE DERNIERS MOIS DE 1875.

Les 36 Communes. Les nombres indiquent les lieux cas de rage ont été observés. plusieurs cas ont été constatés re indique le nombre des cas.

NEDERLAND.

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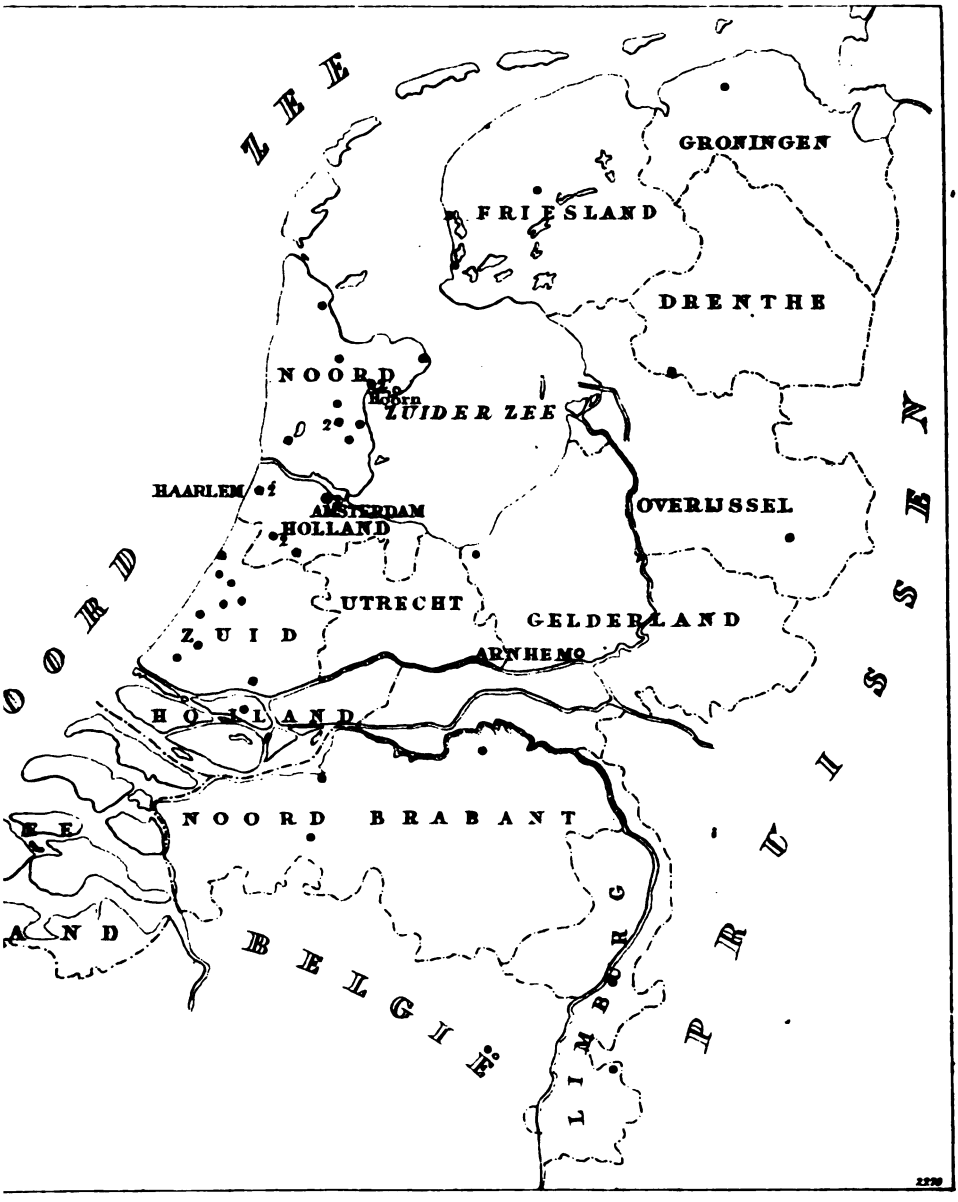

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NEDERLAND.

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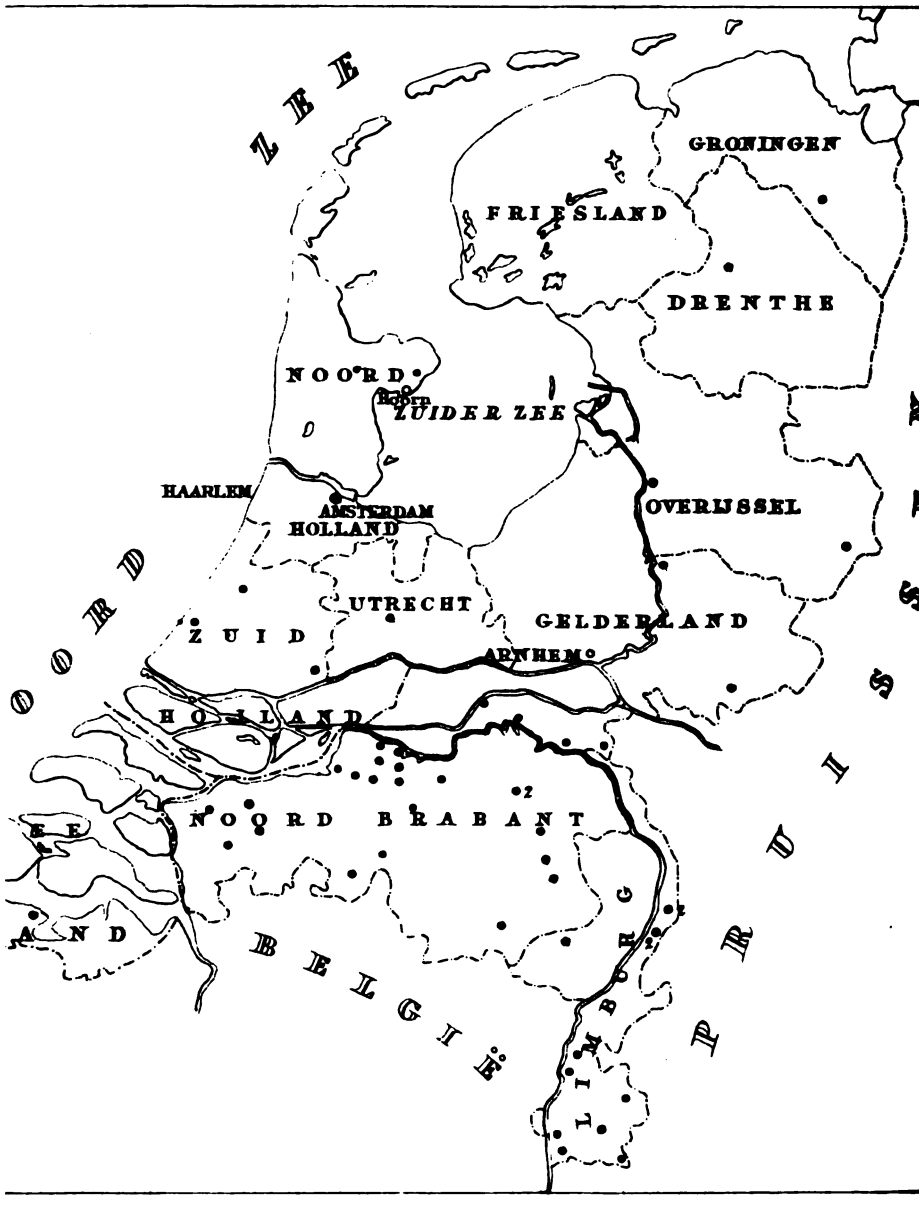

LA RAGE DANS LES PAYS BAS EN 1877.

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NEDERLAND.

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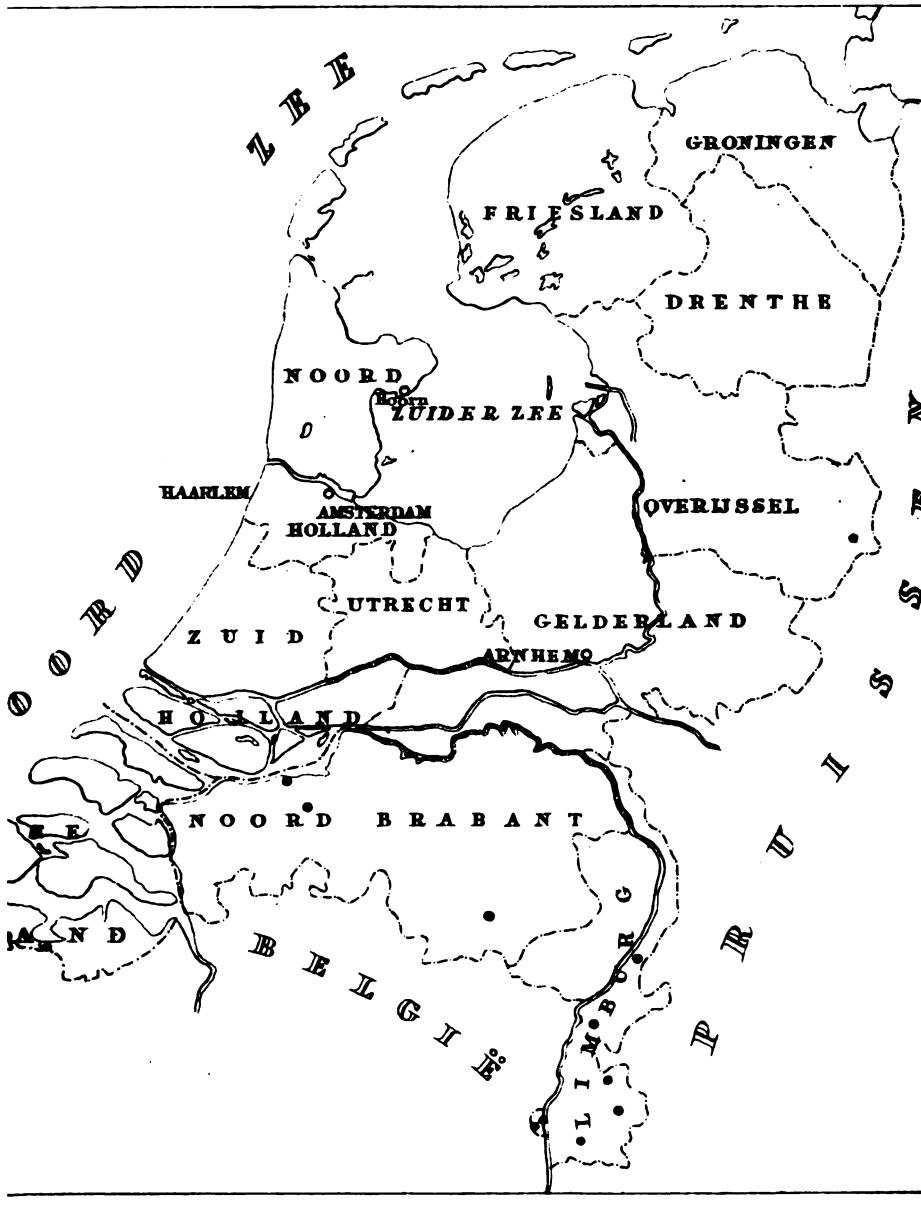

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LA RAGE DANS LES PAYS BAS EN 1878.

NEDERLAND.

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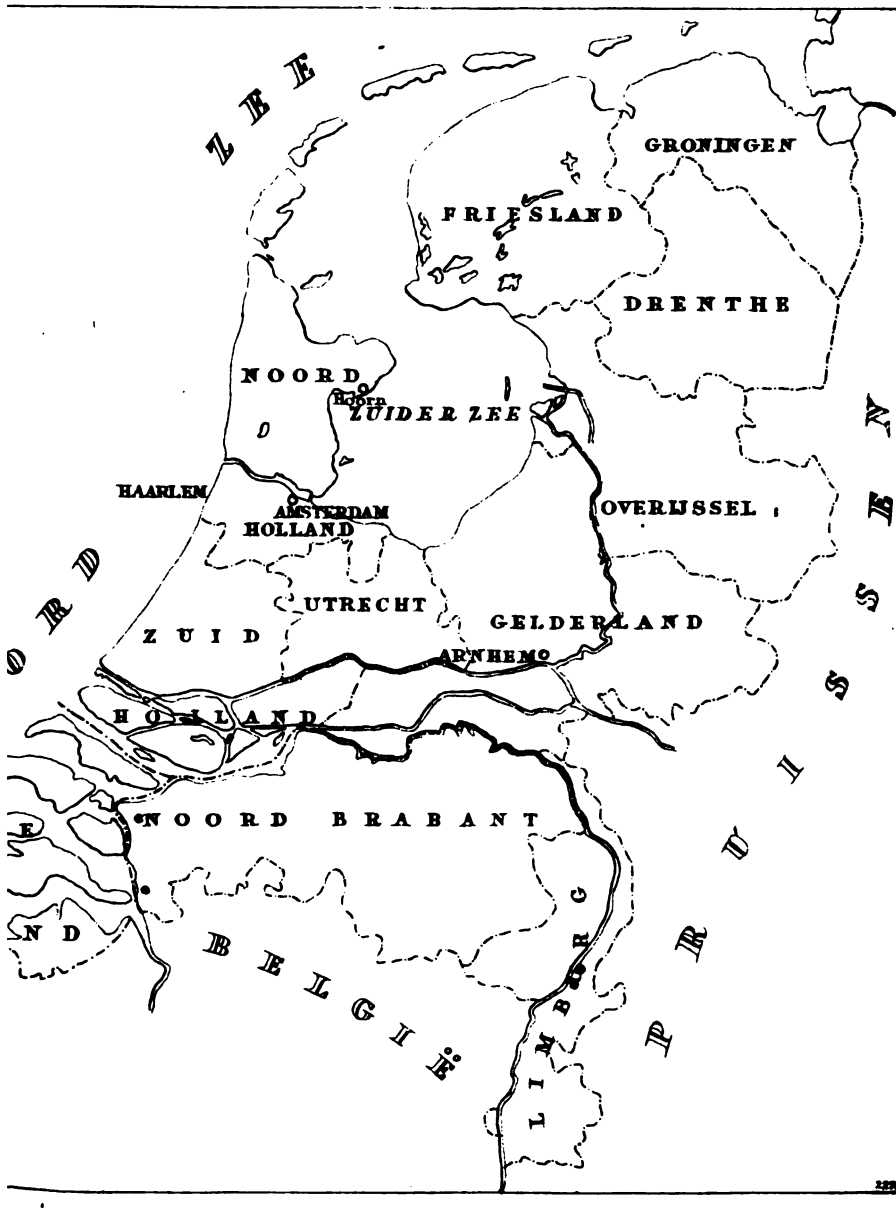

LA RAGE DANS LES PAYS BAS
EN 1879.

dans 7 Communes.
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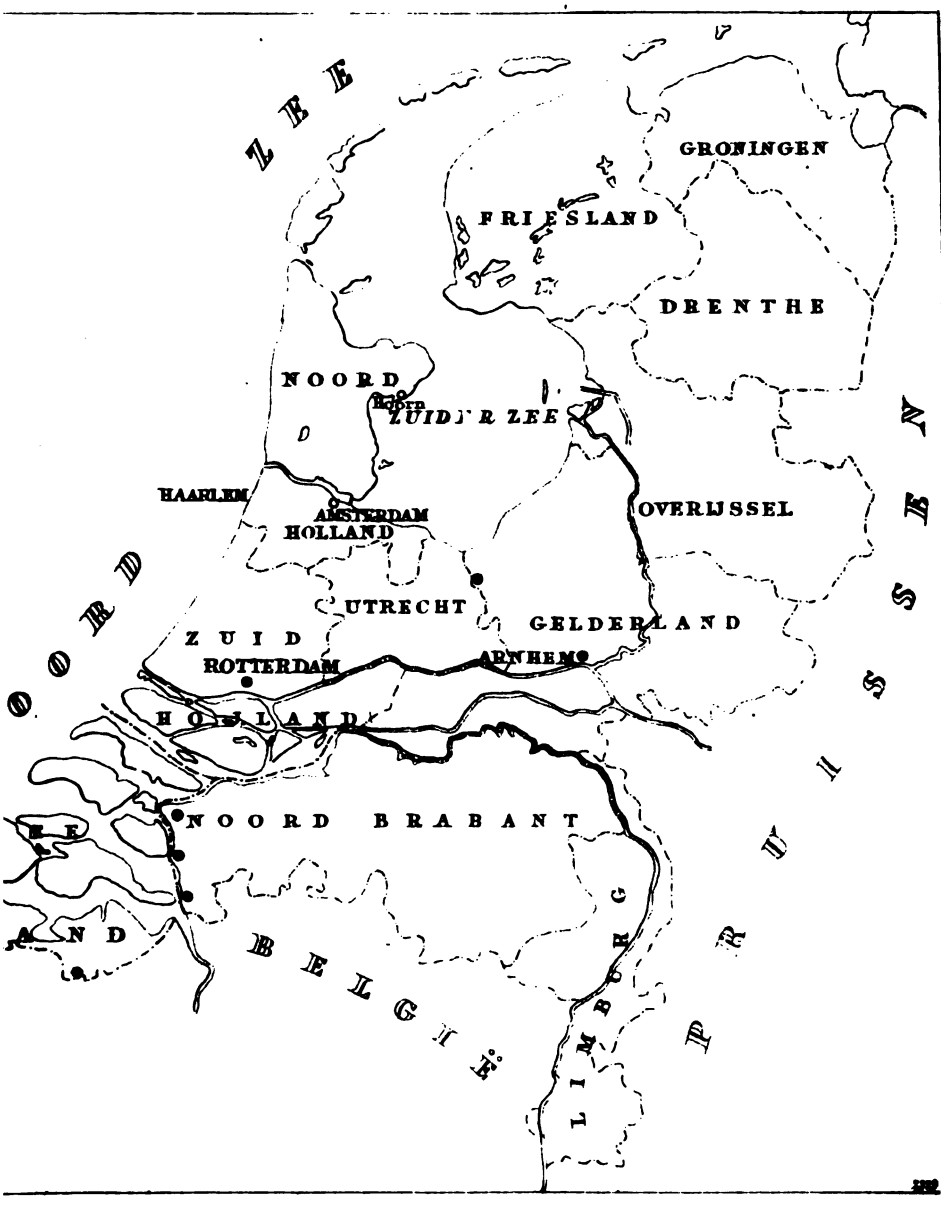
NEDERLAND.

N° 5

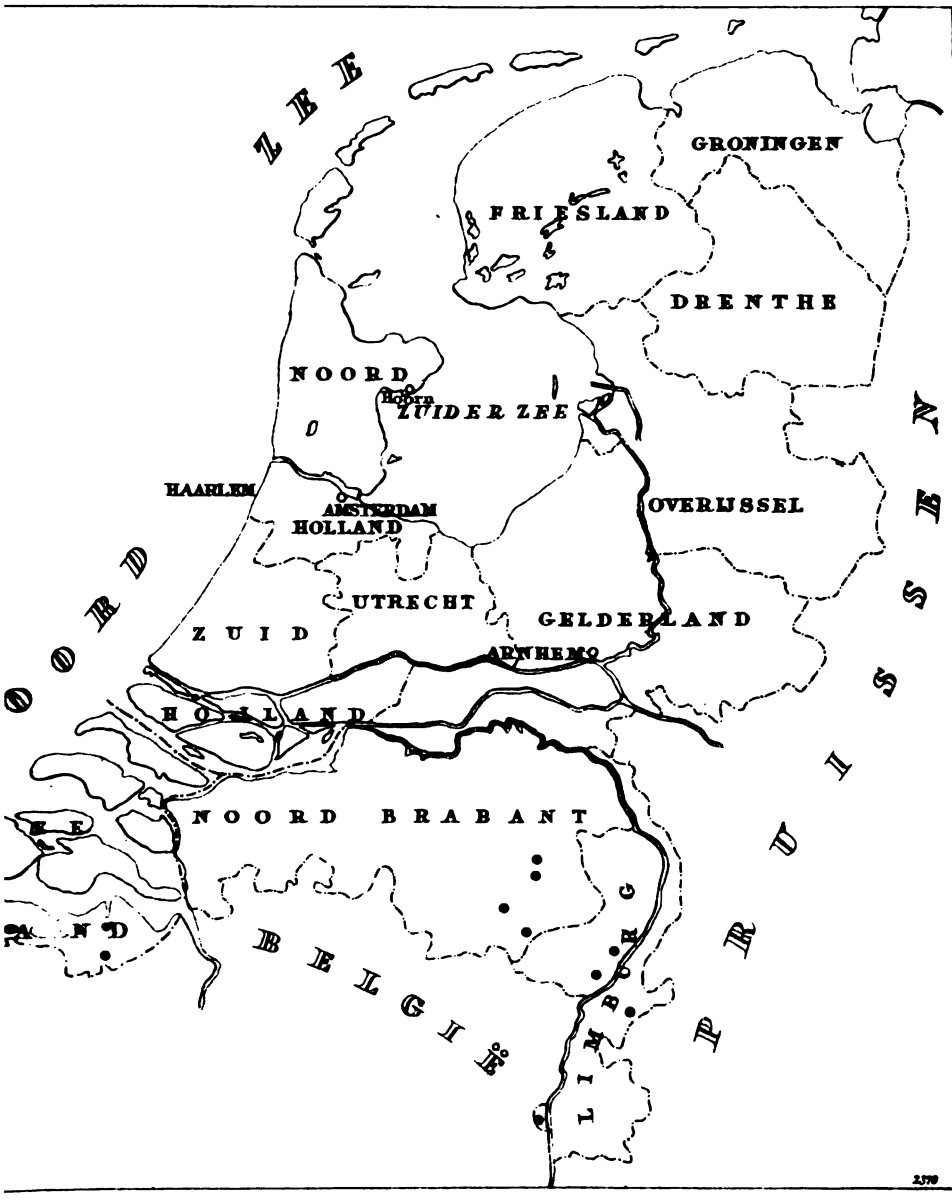


LA RAGE DANS LES PAYS BAS EN 1880.

Dans 13 Communes. Les noires indiquent ou ont eu des cas de rage observés.

NEDERLAND.

N°6.



'9, 7 cas ont été observés, dont 3 dans le centre du pays. Il y avait savoir d'où venait l'infection dans ces trois cas. Le cas observé à vait pour objet un chien d'une meute appartenant à une société de Le chien enragé avait été importé quelques jours auparavant re.

dans le village de Kralingen, près de Rotterdam, a été observé chez un onnu arrivé du côté de Rotterdam, où aucun cas ne s'était montré isieurs années. Probablement ce chien s'est échappé d'un des vais- se trouvent toujours en quantité dans le port de Rotterdam. Du as les renseignements manquent complètement.

cas de 1880 ont tous été observés dans des communes près de la elge.

semble que les conclusions sautent aux yeux. Une loi comme celle de n état d'éteindre les foyers rabiques avec une grande rapidité ; nos cartes rent clairement. On a été en état de prévenir la diffusion de la rage là luction de la contagion d'autres endroits n'était pas à craindre, mais le 'a pas été complet là où nos frontières étaient menacées par la Belgique isse, surtout par la Belgique. Il s'ensuit qu'il est à désirer que les ys de l'Europe prennent des mesures semblables à celles prescrites par andaise, que des mesures internationales chassent la rage de tous les péens.

honneur de proposer que le Congrès déclare qu'il est à désirer que les ments de tous les pays civilisés se mettent d'accord pour venir à bout ge, qui, d'après les résultats obtenus dans les Pays-Bas, s'est montrée une xclusivement contagieuse.

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og Disease, or Canine Madness of the Arctic Regions, ewed in Connection with Hydrophobia ; together with the Measures Used and Suggested for its Extinction, from Information Collected and Observations made in the Country.

Deputy Inspector-General Thomas Colan, M.D., R.N.

Greenland, or Esquimaux dogs, are more like wolves than dogs ; indeed, supposed to be domesticated wolves. They are the "horses" of the ux, without which the latter could scarcely live ; they are capable of great cold and much deprivation of food. These animals travel long s, and carry heavy weights. Now these dogs are subject to a kind of , much resembling rabies, though differing from it in some marked ars. The disease broke out about the year 1859, and spread over nearly le of the Inspectorate of North Greenland. It appeared all the year round, y so in spring ; it was violently contagious between dog and dog, and was posed to be so between the dog and fox ; foxes having been thought to mmunicated it to the dog, as they have been seen affected with the com- The virus is supposed to reside in the saliva, which flows from the mouth and and this can be propagated by means of deglutition, absorption through

the skin, and by inoculation through a bite or otherwise. As far as the author could ascertain, no human being was affected with true hydrophobia from the bite of the dog, though the natives have a great horror of being bitten, notwithstanding they sometimes use the skin, and even eat the carcase of infected animals occasionally. The disease caused much havoc amongst the dogs for several years. Medicine seemed to be of little avail in the treatment, and at length the Danish Government found it necessary to issue directions, in the Danish and Esquimaux languages, how to deal with the disease, which in reality amounted to "stamping out," and thorough disinfection of harness, feeding utensils, sledges, and even the ground on which the dogs had lain, by chloride of lime and carbolic acid; the carcases were to be taken out and buried at sea, or burnt. Separation of the infected dog, and excision, or cauterization of the diseased part, might be useful, but if many dogs were affected, the whole pack may be destroyed.

The disease gradually declined in extent and virulence. It may have been a form or modification of this sickness which attacked the dogs in Dr. Hayes' expedition to Smith's Sound, in 1866–67, and those also in the *Alert* and *Discovery* in Captain (now Sir George) Nares' expedition in 1875–76, further north into the great frozen sea. Doctors Kane, Hayes, and Ninnis, state that they each had a case of true hydrophobia in the dog. From observation and post-mortem examination no reliable cause was found for the complaint, but in all cases examined the autopsy revealed a "pitchy" substance in the intestines, and ulceration for four inches on either side of the ilio-cæcal valve. Symptoms were, on the whole, much the same as in the dogs further south, but there was some difference as to length of attack, &c. Medical treatment in the English expedition appeared to have some effect: but "stamping out" was in some cases adopted, and it may be necessary in the future to resort to such again. Is it not possible that the dog disease of the Arctic Regions is but ripening rabies, which, to the southward of 70° north, shows itself in the rabies which causes hydrophobia? Or is it that hydrophobia is modified in passing through the bodies of carnivorous animals going north, such as foxes and wolves, and becomes the disease we see in North Greenland? It is worthy of note that, when rabies first appeared in some countries, it was less virulent than after a number of years: if so, might it not now be ripening in Greenland? At all events, we must try to prevent its spread. The fact that some persons bitten by rabid dogs suffered no evil effects is no proof of its not being rabies, for many persons bitten in more southern latitudes by mad dogs have escaped.

DISCUSSION.

Dr. DOLAN, Halifax: was pleased the subject of hydrophobia had been brought before the Section. The subject must attract attention. The papers that had been read proved that rabies might be stamped out by vigorous measures. Does the subject interest us in England? Ought we to resort to measures of repression? Yes. 395 deaths from hydrophobia in England and Wales from 1871 to 1877 was appalling. 126 occurred in the north-western districts (Lancashire and Cheshire), 61 in West Riding. Living in an infected district his attention had been drawn to this subject. He published a work on it in 1877; and in the *Medical Press and Circular*, July 13 and 21, 1881, he published a Paper on the "Prophylaxis of Rabies," and a new Dog Act was included in it. The ground was somewhat similar to that taken by Dr. Van Cappelle. We wanted individual action and general action. The first was secured by making owners of dogs feel their responsibility in possessing a dog, by the hygienic treatment of dogs, &c.; the second would be attained by the

spread of information about dogs and their value, &c., by penalties for violation of rules in the period of the prevalence of rabies, by the wearing of collars, by muzzling, and by systematic destruction of suspected or useless animals. The present Dog Act was imperfect. The one published by the speaker in the *Medical Press* was stringent, compulsory, repressive, and would be effective. The time had come for action. Sentimentality must be cast away, stringent measures must be carried out. Contagion was our enemy; we cannot make war on it with rose-water. Abolish rabies in the dog; hydrophobia will be then unknown and a memory of the past.

V.—THE INFLUENCE OF VARIOUS ARTICLES OF FOOD (NOT INCLUDING WATER) IN PRODUCING PARASITIC, ZYMOTIC, TUBERCULAR, AND OTHER DISEASES.

Grounds for believing that the Tubercular Disease (Perlsucht) of Animals which supply Milk and Meat for Human use, is communicated by such Food to Man.

Dr. C. CREIGHTON, Cambridge.

There already exist grounds, of a certain kind, for suspecting that the tuberculosis of the bovine species may have communicated itself to man. Those reasons, which have been more or less familiar to us for the last twelve years, are twofold, and, taken together, they may be said to constitute presumptive evidence of the conveyance of tubercular disease from the one species to the other. The first ground of our suspicion or of our alarm is that tubercle, or, as it is called, pearl-disease, is quite common in the species of animals to which we trust so implicitly—one might almost say, so blindly—for a large part of our food. How prevalent the disease is, cannot be stated with any great approach to accuracy. It is an inherited and chronic disease, which may be present for years in the body of an animal and give rise to no symptoms. The distinctive formations of the serous membranes—the pearl-nodules of the disease—are sometimes found in animals that have been slaughtered in perfect condition. But the disease in its worst form, or so far advanced as to give rise to signs and symptoms during life, is mostly met with in milch cows, and more especially in old cows. The cow-houses in or near large towns are said to contain the largest proportion of diseased animals; the close confinement throughout the whole year, the artificial food, the want of fresh air, and the want of sunlight, all tending to bring out the disease. The cows are milked as long as it is profitable to milk them, and they are then sold out of the herd, probably to the butcher. One reads in the newspapers now and then of prosecutions for sending to the market the carcase of a tuberculous cow; but those are only a few of the very bad cases. Accurate statistics kept for a number of years at the public *abattoir* at Augsburg give about two per cent. of all cattle slaughtered as tuberculous, and of cows alone, about five per cent. But the amount of disease is generally put at a higher figure than that. Virchow quotes a Prussian veterinarian, who gives fifteen to twenty per cent. for his own district, and Virchow appears to accept that proportion for the cattle in Prussia generally. Some breeds are more liable than others, and it is said that there are breeds which are entirely free from

the disease. Without adopting the most alarmist estimates of the prevalence of the pearl-disease in the bovine species, there need be no hesitation in concluding that the milk of cows in a more or less advanced state of tubercular disease is constantly being consumed both by infants and by adults. Professor Virchow is inclined to think that the milk is chiefly deleterious when it is contaminated by the matter of tuberculous abscesses in the substance of the udder itself, and that it is, even under those circumstances, not to be held to be specifically poisoned, but only to be impure milk. The same authority would not condemn the flesh of all animals suffering from pearl-disease; and most people will probably agree with him. But there are the lymphatic glands and viscera, and inferior parts of the carcase, such as the diaphragm, or "skirt," which are especially liable to have the actual tubercular nodules adhering to them, or more or less intimately blended with them; the inferior parts of the animal are sold at a cheap rate to the poor, and there is neither popular prejudice nor legislative enactment to hinder the tubercular from being sold with the non-tubercular. Two days ago I sent a trustworthy person to certain slaughter-houses in London, with instructions to bring me specimens of pearl-nodules from as many animals as he could find. He brought specimens from four old cows which were slaughtered in his presence. The lungs were riddled with purulent cavities; the meat would be sold at about fourpence per pound, to be made into sausages and saveloys.

There is no doubt at all, then, that the species of domestic animals which is so much in our confidence that we even drink of one of its secretions, and eat of its flesh, and sometimes even of its viscera, is a species that is widely tainted with tubercular disease. That alone is fact enough to cause uneasiness. Add to that, the sort of evidence that has been obtained by experiments on animals, and we may seem to have the best grounds for believing that tubercle may come to the human species from the cow. The experimental side of the problem has attracted by far the largest amount of interest, as the chance of making an experiment is sure always to do. Some pathologists have proceeded by inoculating the tuberculous matter from the cow under the skin of the rabbit or other animal, or by injecting it into their veins; while others have experimented by feeding certain of the common domestic animals with the milk of tuberculous cows, or with the actual tubercle-nodules. If all the experiments have not succeeded, a sufficient number of them have, to prove that animals may be made tuberculous, either by inoculation with tuberculous matter from the cow, or by feeding with the tuberculous substance, and even with the milk of the diseased animal. Guinea-pigs are easily affected, rabbits more or less easily; pigs, goats, lambs, and calves can also be made tubercular, and in fact all the usual experiment-animals, except the dog. The numerous series of experiments have brought several facts to light as to the behaviour of the bovine tubercular virus when introduced into the body of a healthy animal. It is proved to be a comparatively slow poison, and by no means always a deadly one. Even the rabbit will go on being fed with pearl-nodules for three months before the infection takes firm hold; and even at the end of that period the rabbit may not die of the disease, but the disease will be found only when the rabbit is killed for the verification of the experiment. Such animals, again, as the goat, lamb, calf, and pig may not be visibly affected in health under the experiment, but they will nearly always show traces of tubercular disease when they are killed after a few weeks or months. The first experiments were made fifteen years ago by Gerlach, of the Veterinary School at Hanover (afterwards of Berlin), and those earliest experiments are probably still the best, both in conception and

results, inasmuch as the bovine pearl-disease was treated from the point of view of a specific disease, and the distinctive marks of the specific disease were looked for and were found in the experiment animals. His language is: "The milk of tuberculous cows, according to the experiments, is not only injurious in a general way; it is specifically injurious, it gives rise to the same pathological new formation; it is, therefore, infective" (*Virchow's Archiv*, vol. li. 1870, p. 305).

Such, then, are the established facts which create a presumption that the enormous consumption of cows' milk by infants and by adults, as well as the use of inferior kinds of meat by the poor, is not unattended with risk. We have had these facts before us for a considerable time, and I believe that we might contemplate such facts for an indefinite time, and yet have no practical certainty, no real assurance that any part of the tuberculosis of man comes from diseased cows. I took the opportunity of attending a meeting of the National Veterinary Congress in London, on the 22nd of July, 1881, when this question was discussed; and we heard from a veterinary surgeon from Peterborough a narrative which brought out the value of our present presumptive evidence. A cow, which he knew professionally to be in an advanced state of tuberculosis, was sold out of a large farm for five pounds; the purchaser kept the cow for the exclusive supply of his family with milk and butter. We were informed that the man's wife and one of his children had since then died of consumption, and that the man himself was now in a consumption. The veterinary surgeon, our informant, had discussed with the medical attendant of the family whether the tuberculous cow was not accountable for these cases of consumption. But observe the answer of the medical practitioner. He said that the family was predisposed to consumption, and he did not seem to think that there was any reason for going to the tuberculous cow for the explanation of an incident sufficiently common in medical practice. It is always easy, in any particular case of consumption, to allege a phthisical disposition, and it is always difficult to disprove it. One has only to read the article on tuberculosis by Professor Rindfleisch, in the current number of *Virchow's Archiv*, in order to see how far the doctrine of inherited tubercular taint may be made to go in explaining outbreaks of consumption, and how valueless for all practical purposes our suspicions, or our presumptive evidence of bovine, or other direct infection, may be made in any given case to appear.

There is something more wanting in the way of evidence beyond a certain circumstantial probability, before we can assert of any one case of tuberculosis, that it has been communicated from the cow; and Professor Virchow, in addressing the Berlin Medical Society, in March, 1880, appears to me to have pointed to the real defect in the evidence. After summing up the experimental evidence for and against, he said: "But we must, in any case, bear in mind that no man has ever yet acquired pearl-nodules by partaking of tuberculous flesh"—and, I suppose, one may add, by drinking tuberculous milk. I do not wish to go beyond Professor Virchow's meaning, but his remark seems to me to imply that the finding of pearl-nodules in the human body would indeed put the matter in a new light.

It is at this point that my own observations come in. About the same time as Professor Virchow gave his address to the Berlin Medical Society, but before I had had an opportunity of reading it, I had been collecting specimens from a number of tuberculous cases at the Cambridge Hospital, my suspicions being aroused by certain appearances that I observed in the post-mortem room, as well as by what I learned of the clinical histories. As I have elsewhere ("Bovine Tuberculosis in

Man," London, 1881) given a minute account of the first series of cases that I observed, it will suffice to say here that I professed to recognize the disease in those cases to be the bovine tubercular disease, for the reason that the morbid products had more or less of the distinctive characters of the bovine disease. What are the distinctive characters of bovine tuberculosis, or, indeed, has it any distinctive characters? I should be inclined to answer that it has well-marked distinctive characters, and no disease more so. The names by which it is known in various countries are derived from its characteristic features, and especially from the extensive and peculiar implication of the serous membranes. Nodular formations on the pleuræ, pericardium, and peritoneum, of whitish colour and firm texture, the size of lentils or peas, and sometimes much larger, often stalked or pendulous, often also adherent and confluent, occurring not unfrequently in the unmistakable form of pearls strung on a cord, vascular in their exterior layers and calcareous or caseous within, having the structure of small fibromas or sarcomas with the addition of giant-cells to their fibromatous or sarcomatous elements—such new formations on the serous membranes are distinctive of the tuberculous disease of the cow. Such formations are primarily the disease, but it nearly always involves the lymphatic glands, and in many cases it involves the lungs also. It is, in fact, a specific disease, and it may involve any organ or part of the body. I need scarcely say that I did not find in any instance in the human body a full and complete development of the pearl disease, as one would find it in the body of a cow. What I found on the serous membranes was a mere trace of the pearl disease, but the traces were distinctive of that disease, and not of anything else. The figure shows [figure exhibited] perhaps the best instances of the pearl-nodules that I have hitherto found. It represents two small groups of pearl-nodules from the under surface of the diaphragm, in a robust man, aged forty, a hawker, and previously healthy, who died of rapid phthisis, his illness dating six months back. I may say that I had no such perfect specimens of the bovine pearl-nodules before me at the time when I was working out the subject, and even up to the time when I had sent my final account of it to the press, the specimens drawn being taken from a case that occurred sometime after the resemblance to the pearl disease had first occurred to me.

These nodules are small, round, or flattened bodies situated in the midst of cord-like outgrowths from the serous membrane, or suspended on cord-like stalks, like small shot on a string. Sometimes the cord-like outgrowths may exist without nodular thickenings in them. At other times the outgrowths from the serous membrane may be in the form of soft, leaf-like formations; but these are as definitely bounded, of as uniform size, and as distinctly pedunculated as the pendulous nodules themselves. On the parietal and diaphragmatic pleura I have once or twice found small clusters of nodules and cords deeply pigmented. The favourite situations are the sharp margins of organs, the surfaces of the diaphragm—especially on the right side—and the surface of the spleen. I have seven instances of the occurrence of this formation on the under surface of the diaphragm, only one of these being from a case where the tubercular peritonitis was general. In that situation the nodules are generally flat, and tending to run together in broad lobulated patches. In the specimen produced, there is a tuberculated stratum in some places a quarter of an inch thick, and towards the back of the diaphragm there is a mass loosely suspended by a stalk or membrane, and as large as a filbert. The structure is sometimes hard and fibrous, at other times firmly cellular, and at other times of loose and soft texture.

Hitherto I have spoken only of the formations on the serous membranes. But the disease must be considered as a whole, and its manifestations studied in all the organs and parts of the body which are liable to be affected. The greater part of the inquiry, already published, upon which I base my present communication, is occupied with the histories of cases, and with the pathology of the disease in the lungs, in the lymphatic glands, &c., no less than with the formations on the serous membranes. As regards the lungs it is difficult to prove that any tuberculous process in the human subject is distinctive of the bovine form of tuberculosis; and I will merely say that smooth-walled vomicæ and definitely encapsuled nodules, having respectively a considerable resemblance to bronchiectasies and to their alleged inspissated contents, are probably distinctive, and that those conditions may be looked for where the process in the lung is of the more chronic sort. But the study of the disease as a whole brings out one thing, which is of more consequence than numerous identifications on single points; it is the discovery that the morbid process is practically the same in all situations—in the lymphatic glands, in the lungs, and wherever it occurs. The disease is found to be co-ordinate in every part; no one seat of the disease can be regarded as a focus from which a secondary disease elsewhere has originated. That co-ordination of the disease in every part is characteristic of an infective process set up by an introduced virus; and it is only because the serous-membrane new formations seem to point most clearly to the source of the virus, that they are so much dwelt upon. They were subordinate in amount or extent, and of no importance whatever for the fatal issue. They were rather the curiosities of the post-mortem condition, or the incidental accompaniments of a disease which had its chief seat in the lungs. And if we are to judge of them by their size, and the insignificant extent of their occurrence, we should rightly dismiss them as too trifling a part of the disorder to dwell upon. The nodules in the figure which I have handed round would at the most be a small addition to the many curious things that are found from time to time in the human body. But I am inclined to look upon them as signifying a great deal; and that, too, because of their striking resemblance to the pearl-nodules of the tuberculous cow. They resemble the serous-membrane tubercles of the bovine disease, not only in form and colour and texture, but also in their minute structure—fibrous or sarcomatous, with vascular periphery, giant-cells, and a more or less caseous centre—and, still further, in their development or in their early stages.

What, then, does that resemblance or identity signify? It may signify one or other of two things—either that the same causes which have led to the growth of peculiar nodules on the serous surfaces of the cow are sometimes at work to produce the same result, in a feeble degree, in the human body also; or that the disease in man *is* the bovine pearl-disease communicated by infection from the one species to the other, and carrying with it that *mimicry of form and structure* which infection always does carry with it. For the first alternative it may be said that there is a tubercular disease in the bovine species, and there is a tubercular disease in the human species, and why should not the one have pearl-nodules on the serous membranes as well as the other? There is, perhaps, no good reason, in the nature of things, why it should not; but we have at any rate been in the habit hitherto of regarding the pearl-nodules as proper to and distinctive of the specific disease of the bovine species. Let us by all means try to resolve their occurrence into natural or ordinary causes, and into the same natural causes in man as in the cow. But in that attempt we will find it advantageous to

begin with the cow, as having the formations most uniformly present, in largest abundance, and in greatest variety. I believe, then, that such an attempt to trace the origin of what is now a specific disease would be successful for the bovine species, and I think that there are certain indications as to how that problem should be approached; but I do not think that the rational account of the rise and progress of the disease, and of its distinctive formations, as determined for the ruminant species, would have the very slightest applicability to the human species. In man these serous membrane growths are few in number, faintly marked, and feebly developed, and they are merely an accessory of an infective process which has the lungs for its principal seat. In themselves they are utterly insignificant in amount and altogether unimportant in their effects. But when we see a few of them round the margins of the lungs, or on the surface of the diaphragm, we should read them as we read a signal. They are like the solitary footprint on the sand; they assure us, with the most perfect certainty, that some strange thing has arrived in the body whose species and whose specific properties we at once recognize.

DISCUSSION.

Dr. CARLO CUTURI, Pisa: recognized the light thrown upon this subject by the author of the paper, and he drew attention to the fact that Drs. Portal and Lebert, early in the century, had studied this disease, not only in the bovine species, but in she-asses, in connection with the use of milk by tuberculous persons. He could not undertake to confirm the view that the disease was due to a virus, as in syphilis.

Dr. GÜNTHER, Dresden: stated that the studies which had been pursued in the *Thierarzneischule*, in Dresden, as to the use of the milk and flesh of animals suffering from pearl-disease, had ended in negative results. He also added that the use of milk from such animals in a district of Saxony, where the disease was widely spread, had not been followed, amongst children consuming the milk, by any increase of phthisis.

Dr. ALFRED CARPENTER, Croydon: did not think tuberculosis was likely to be easily propagated by milk to man from tubercular animals. He founded his belief upon the fact which had been given in evidence in a court of law, that ninety per cent. of the animals which were slaughtered for the Metropolitan Meat Market were more or less infected with tubercle; a large portion of this meat was doubtless cow beef. It was shown that this was almost universally the case in cows which had become barren. There was also another strong inference to be drawn from the fact that tubercular disease was found to be most prevalent among that class of people to whom milk was not a common article of food—viz., the children of the poor in our manufacturing districts. These people do not get milk as a rule, but tubercular disease is specially common and fatal among them; whilst those children who do get milk in a good state are far healthier. At the same time, it must really be that meat and milk from diseased beasts cannot be healthy; that whilst they are kept in close, ill-ventilated and miserable sheds, disease will abound among them, and the time must come when they will be kept in the manner which Nature designed them to be—viz., in the open fields of the country.

On the Distinctive Characteristics of an Acute Specific Disease Produced by the Eating of Pork Infected with a Species of Bacillus. Illustrated by two Series of Cases.

Dr. EDWARD BALLARD and Dr. E. KLEIN, London.

The first series consisted of some scores of cases occurring among persons who had, in the course of the five days (June 15th to 19th, 1880), partaken of refreshments provided at a sale on the Duke of Portland's estate, Welbeck, Nottinghamshire. The illnesses were traced to the eating of cooked ham, prepared in England from pork imported in a salted state from America. Four persons attacked with the disease thus induced are known to have died after a few days' illness. A good many, however, who partook of the same kind of food on the same days escaped illness. There was, before the occurrence of the first symptoms, an incubation period of twenty-four to thirty-six hours. The illness commenced in various ways, sometimes with chilliness or rigors, sometimes with giddiness, faintness, or pain in the chest, back, or abdomen; but sooner or later there was pain in the abdomen, with fœtid diarrhœa and usually vomiting. Muscular weakness was an early and very remarkable symptom, and mostly there were headache and thirst. Various nervous phenomena were observed in some cases, such as cramps, convulsive flexions of the hands, or fingers, muscular twitchings, pricking sensations, or numbness of the hands, drowsiness, hallucinations, &c. Very imperfect post-mortem examinations were made in three of the fatal cases; and, in the only two in which the chest was examined, pulmonary congestion was stated to have been found. The only portion of tissue that we had an opportunity of examining microscopically was part of a kidney. In this was discovered evidence of parenchymatous inflammation, and the afferent arterioles and capillaries of the Malpighian corpuscles were found plugged with emboli formed of masses of *bacilli*. Portions of one of the cooked hams used at the sale, and of a raw ham of the same American consignment, were examined microscopically, and in both was found a species of bacillus with sporules of the same; the bacillus threads and sporules were in connection with the muscular fibres, and in the inter-muscular tissue. Experiments were made on animals by feeding, or inoculation, or by both methods, and also by inoculation after cultivation of the bacillus material in the incubator. In all but a few instances disease was produced. The morbid condition most uniformly observed after death was pneumonia or pulmonary hæmorrhage; other conditions were hæmorrhage into the liver, enlargement of the liver or spleen, pleuritis, peritonitis, &c.

The second series consisted of fifteen cases of illness, one of which was fatal. All these persons had partaken of hot baked leg of pork, purchased on two several, but consecutive, evenings in February, 1881, at a "cook-shop" in Nottingham. The illnesses which resulted from eating this pork (with which gravy, prepared from the bones, pigs' feet, and tinned beef, was dispensed) were of a precisely similar character to the illnesses in the earliest series, and were preceded by a similar period of incubation. In this instance also, many persons who ate of the food escaped illness, but when any family had partaken, all who in that family partook of the food were taken ill. In the fatal case, the following were the chief

conditions observed on examination after death: extensive hæmorrhage into the tissue and air cells of the lungs, slight interstitial hepatitis and inflammation of Peyer's glands. In the kidney, the capillary vessels of many of the Malpighian corpuscles were impervious, the nuclei of the glomeruli being increased; some of the tubuli contorti contained extravasated blood, others of them casts. Bacilli were found in the blood taken from the heart, in the lung juice, and in the blood extravasated into the air-cells, &c., in the tissues of the stomach, ileum, spleen, and kidney, &c., and in the vessels of some of these organs. Animals inoculated with the blood, lung juice, and pericardial fluid, became diseased, and some of them died. The morbid condition found after the death of these animals corresponded with those observed in the animals fed or inoculated with the bacillus material of the ham in the Welbeck series,—viz., pneumonia, with or without pulmonary hæmorrhage, peritonitis, pleuritis, or enlargement of the liver and spleen. Similar results also followed the inoculation of material obtained by cultivation of the blood and lung juice. Bacilli were found in the blood and exudations of some of the inoculated animals as well as in the purulent matter of tumours found in two of them at the seat of inoculation. Unfortunately, in this instance none of the food to which the illness was attributed was obtainable for examination.

It is submitted that by the description of these two series of cases an acute specific disease has been defined which has not been defined hitherto, and it is suggested that such cases of this disease as have been heretofore noticed by observers have been regarded in the light of cases of poisoning (as by sausage, &c.), whereas there are marked distinctions between the two classes of cases.

A complete account of these two series of cases will appear in the forthcoming Reports of the Medical Officer of the Local Government Board.

DISCUSSION.

Dr. Tripe, London, said: That he had seen numerous cases of poisoning from eating in one instance beef-sausages; and in a second, from eating dripping. The symptoms were the same as those described by Dr. Ballard, and lasted more or less for a considerable time. The evacuations were liquid, dark coloured, and extremely fœtid. Those suffered most who ate the sausages raw or underdone; a man and a cat dying from eating them raw. At the post-mortem examination of the man, who died on the ninth day, the stomach was found somewhat reddened and stained, but the other organs appeared healthy. No microscopic examination was made, but a careful chemical examination was made by Dr. Letheby of the unconsumed sausage, and no poison was found. The second set of cases were caused by eating dripping of beef and mutton, not pork. None of the employés who ate of the meat and gravy when hot, suffered in any way. The symptoms, which resembled those just mentioned, supervened in from eight to twenty-four hours. There were twenty-nine persons attacked out of thirty-one eating the dripping, and those suffered most who partook of the jelly under the fat; and no one, although living in the same house, was attacked who did not eat of the dripping. In two instances the dripping was used for making a pie, and although those partaking of it suffered from diarrhœa, there was no prostration. A very careful chemical and microscopical examination was made without any poison being detected; on mixing it with hot water, the fat rose to the top, and was found to be quite sweet, but the fluid was offensive, and contained nucleated and other cells resembling monads and

ceria. There were not any threads of bacillus or fungus growth. On examining jelly similar bodies were found in it. The short period which had elapsed from time the dripping was made and the selling—viz., less than forty-eight hours, is arkable; but from inquiries made, it is very possible that it was placed in a n which had previously contained dripping and had not been properly nsed. He would not express any opinion as to the precise cause of the out- ık, but believed that many instances of severe diarrhœa could be traced to a ilar cause.

Dr. BUCHANAN, London: had recently heard rumours of disease, perhaps similar ature to the disease of Welbeck and Nottingham, which had occurred among consumers of meat at a town in Norfolk; and the interest of this meat disease sisted in the fact of beef, and not pork, being the meat which had appeared to iarm. It was a joint of salted and boiled beef that was in question.

Dr. EZRA M. HUNT, Trenton, N.J.: In speaking of pork or of sausage as having ıe from the United States, it is desirable that evidence be taken as to the fact, as to the locality; and that the meat be traced to its place of production. This l also aid in the important knowledge whether the live meat was diseased, or ether certain processes of change have occurred, or whether it has in transport in exposure to other articles, become the nest for some foreign parasite. For tance, in the cases of sausage-poisoning from sausage brought from the United tes, we need to know all the history of the meat, when and where prepared, and various exposures, and not convict the foreign pig *ex necessitate*. Sausage was his country, and perhaps here, a very composite structure, representing various mals, and it may also undergo some unfavourable fermentive changes. The call State Medicine, and not less the comity of nations, now required that all ctions be traced with analytical exactness to their sources, and that the history he origin and exposures of the article consumed be as precise as that of the ıptoms produced. The Government of the United States now had in their ntry a competent microscopist and veterinarian seeking such evidence, and we uld ask that all those who report or examine into cases will also secure minute- s of detail as to the history of the articles consumed.

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luence of Various Articles of Food in Spreading Parasitic, Zymotic, Tubercular and other Diseases.

Mr. FRANCIS VACHER, Birkenhead.

n stating the subject, Mr. Vacher desired to emphasize the word "spreading." was well known, there was a large number of diseases which might fairly be to be produced by ingested food, as when scurvy followed a diet of salt pork ship's biscuits, or when eczema appeared in a subject whose diet had been gre and unvaried. The diseases, however, which there was any good ground elieving might be *spread* by means of food, were far less numerous; still, they

were diseases of frequent occurrence, and belonged to three distinct orders—miasmatic, parasitic, and tubercular.

Food might spread disease in three ways:—1. It might itself be in a pathological condition. 2. It might serve as a culture medium in which disease-germs were nourished and multiplied; or 3. It might serve simply as a nidus in which such germs rested.

The foods which alone could spread their own diseases to the subjects by whom they were ingested were necessarily meat or milk, or their derivatives. There was abundant evidence in support of the view that foot and mouth disease might be spread to the human subject by means of milk. Nor was this surprising, as large vesicles often formed on the udder, and these got ruptured in milking; the result being that the specific contagium liquid of the disease was mixed with the milk. There was also evidence not less abundant, though in some respects less satisfactory, of tubercle being communicated to children who had been fed on uncooked milk from tuberculous cows. As regards meat, there was evidence that a specific disease might be communicated to man by the ingestion of meat tainted with splenic fever or anthracoid disease; and erysipelas (a common symptom in many animal diseases) it appeared might spread to man by means of flesh from a diseased carcase. The only other diseases which could be spread by means of meat were those indicated in the animal by the presence of cysticerci and trichinæ. Latterly, there had been a report that cholera had been spread from swine to man by means of lard made up as butterine, but no facts in support of the statement had been produced.

It was probable that almost any perishable food might occasionally serve as a culture ground or fluid for disease germs—especially nitrogenous food. It might be that a little water, a little ammonia in a form readily assimilated, and a temperature not too low, was all that was necessary for the sustenance and multiplication of the low forms of life constituting the germs of some diseases. So little was positively known about the nature of these germs, and their manner of reproduction, that we had to be most cautious in receiving statements in reference to them; yet some recorded facts were scarcely explicable on any other theory than that certain germs were capable of growing and multiplying outside the body of the subject from which they were derived. The *materies morbi* of enteric fever and cholera, and kindred diseases, were contagia almost certainly capable of being nourished and bred outside the body; and other contagia doubtless possessed the same faculty, though the proof of its exercise was less clear. Considering that the organic matter present in nearly all water was sufficient for the maintenance during active generation of countless organisms, it was surely conceivable that animal fluids, such as milk and serum, should prove equal to the support in active life of bodies of so simple a form that they might be regarded as composed of undifferentiated protoplasm. That disease-germs, ordinarily so-called, should live and propagate on or in vegetable food, seemed less likely from analogy; but it was a known fact that the spores of parasites affecting the human subject may be developed on plants.

The influence of various foods in spreading disease was far more considerable when food was simply a passive agent, carrying contagion. Not only meat and milk and their derivatives, not only bread and confections, but the large class of supplemental foods coming under the term groceries, were doubtless, occasionally, in this way carriers of disease. This appeared to be partly due to the ignorance and uncleanly habits of small shopkeepers, partly to people in indigent circum-

stances being constrained to resume their ordinary avocations at too early a time after suffering from infectious disease, and partly to the apathy of the whole community as regards ordinary sanitary precautions, and a habit of eating many foods uncooked, or imperfectly cooked. Thus, milk got infected through the milkman having children with the measles in the parlour behind his shop: bread, through the baker being a convalescent from scarlatina; and sugar, through the grocer's apprentice having returned to work too soon after an attack of small-pox.

Although disease might thus be spread by food in three ways, its influence in disseminating disease might be easily exaggerated. Books, letters, clothes, public vehicles, would all serve as carriers of infection just as well as food; and such germs as were capable of active life outside the body, could propagate no less in middens and foul water than in milk. The danger, however, such as it was, must not be ignored, but resolutely faced, and every exertion made to reduce it to a minimum.

The measures which, in the judgment of the reader of the Paper, appeared to be necessary to this end, were:—

1.—A more intelligent and scientific system of meat inspection.

2.—Giving effect generally to the Dairies and Milk-shops' Order.

3.—The provision of free hospitals for infectious convalescents.

4.—The thorough cooking of all meat or milk to be used as human food.

The Influence of Milk in Spreading Zymotic Disease.

Mr. Ernest Hart, London.

I beg leave to submit for the information of the Section an abstract in a tabular form of the various epidemics due to infected milk that have been recognized in this country. In view of the large amount of business that will probably have to be transacted to-day, and the number of gentlemen who will no doubt wish to speak, it will be impossible for me on the present occasion to go into the subject of the spread of infectious disease by milk so minutely as the importance of the subject deserves. This is, however, the less to be regretted because the tabular abstract which I have prepared to a great extent tells its own story, and teaches its own lessons; and it will probably be sufficient briefly to recapitulate the facts I have gathered together, leaving to others the drawing of any deductions therefrom.

The particulars of all the epidemics that I have been able (after much diligent search in official records and medical journals, as well as by direct inquiries) to discover, are given in the abstract under the following headings:—

1, Date of outbreak; 2, Locality; 3, Reporter: 4, Total number of cases; 5, Deaths; 6, Number of cases amongst drinkers of infected milk; 7, Percentage to total cases (col. 6 to col. 4); 8, Number of families supplied by milkman; 9, Number of such families invaded; 10, Percentage (col. 9 to col. 8); 11, Sanitary circumstances of farm or dairy from which milk was derived; 12, Exciting cause of outbreak; 13, Circumstances implicating milk; 14, Facts showing special incidence of the disease; 15, References to sources of information.

The three diseases which have as yet been recognized as capable of being spread by milk are typhoid fever, scarlatina, and diphtheria. There is nothing in the analogy of epidemics to limit the list permanently to these; and already we are seeing, as in the obscure and as yet unexplained epidemic that occurred last April at Aberdeen, indications of other cognate diseases being spread by the same agency. The number of epidemics of typhoid fever recorded in the abstract as due to milk is fifty; of scarlatina, fifteen; and of diphtheria, seven. The total number of cases occurring during the epidemics traced to the use of infected milk may be reckoned in round numbers as 3,500 of typhoid fever, 800 of scarlet fever, and 500 of diphtheria. When it is remembered that barely ten years ago we were utterly ignorant of milk being a carrier of infection, and that consequently these epidemics have all occurred within one short decade, it will be seen how vitally important is the safeguarding of our milk-supplies from contamination. That so essential and common an article of food as milk is, should be so readily capable of absorbing infection as it is now known to be, is a question of the gravest moment.

It would be foreign to the purpose of this paper to attempt to give any historical account of the various stages by which we have come to recognize milk as a carrier of disease. My own more immediate personal interest in the matter dates from the great Marylebone epidemic of 1873, when the fever attacked my own household, and those of many of my friends. In conjunction with the late Dr. Murchison, to whose sagacity and indomitable energy the tracking of the epidemic to the milk was chiefly due, I took a large share in the elucidation of the outbreak, and since then my attention has been continuously directed to the subject.

The mischief caused by the Marylebone epidemic was so widespread, and the proofs of its connection with a particular milk supply (although so fiercely contested at the time, that I was threatened with an action for libel for publishing the facts), were ultimately made so clear that the then sceptics became perforce converted to a belief in milk infection. The demonstration of this infection has indeed, by this time, become so complete in this country, that its acceptance has now become practically universal.

The mode of access of zymotic poison to milk is not always possible of ascertainment: nor is this to be wondered at. Not only do persons inevitably forget apparently trivial actions or events which may, nevertheless, have a most important bearing upon the causation of epidemics, but, even where such events are remembered, there is often the strongest inducement of personal interest, on the part of persons concerned, to concealment of them. The fact, therefore, that in certain of the epidemics it has been impossible to prove the actual contact of the infecting material with the milk, by no means invalidates the conclusion, based upon irresistible evidence derived from other sources, that the milk was actually the medium of the poison. The real test is the evidence derived from the incidence of the disease upon the customers of the suspected dairy. If it be found that whilst a particular district is otherwise almost, if not wholly, free from fever, a large number of persons fall ill about the same time, and that the only thing they have in common is the supply of milk from a particular dealer, the inference is tolerably obvious that the poison is distributed with the milk.

As regards *typhoid fever*, the most common way in which the poison has been observed in these epidemics to reach the milk, is by the soakage of the specific matter of typhoid excrements into the well-water used for washing the milk-cans and for other dairy purposes, and often, *it is to be feared*, for the dilution of the milk itself

—for which, in official reports, "*washing* the milk-cans" has become a convenient euphemism advisedly employed to avoid raising unpleasant questions.

In twenty-two of the fifty epidemics of typhoid fever recorded this is distinctly stated by the reporters to be the case, and in other cases it was more or less probable.

When a dairy is unwholesomely or carelessly kept, there is obviously a great variety of ways in which the poison may reach the milk. In at least ten epidemics of typhoid fever and four of scarlatina, we read of the individual attending upon a sufferer from infectious disease, being the same person who habitually performed the operations of the dairy and milked the cows. It is not necessary to descend to details to point out with what dangerous elements milk dealt with in this way may become infected. Fouled hands or dirty clothes, or the mere clinging of poison germs to the person, may suffice under these circumstances to distribute disease amongst customers of the milk.

A case is recorded of the milk-cans being washed with the same dish-cloth as was used for the patients suffering from fever at the farm (No. 24). In another case (No. 18), there is a story of infected body-linen washed in the place used for dairy operations, and of a subsequent considerable outbreak of enteric fever amongst the customers. (See also Case 35.)

The milk-cans are often taken into the house while disease is present in it. In Case No. 4, the milk-cans were taken for washing into the kitchen—which opened direct into the sick-room—to be washed and dried, and "unclean linen was seen placed side by side with the lids of the cans." (See also Case S. 7.) Other cases of a similar nature will at once strike the eye on a perusal of the abstract.

In dealing with *scarlatina* spread by milk, we have of course a widely different set of circumstances to consider. Scarlatina being almost invariably spread by contagion, and by the inhalation of the bran-like dust which is thrown off from the body during the disease, we should expect, in epidemics of milk-scarlatina, to receive evidence of this dust having access to the milk. And it will be found on reference to the abstract that in the majority of instances it was proved, and in the other cases circumstances did not always negative the suspicion, that persons employed about the dairy operations were in attendance at the same time on persons sick of scarlatina.

As regards *diphtheria* spread by milk, our knowledge is unfortunately less precise. Prior to the large epidemic of that disease which attacked the north of London in 1878, and which was shown by Mr. Power to have been due to the drinking of milk supplied by a particular dairyman, the spread of diphtheria by milk had not been suspected, or at least demonstrated. But since Mr. Power's inquiry, a number of other outbreaks, amounting now to some six in number, have been recognized as due to milk. In none of these cases has it yet been possible to trace the exciting cause of the outbreak, though, as to the disease being spread by milk, there could be no doubt whatever. Mr. Power has, indeed, suggested for consideration, whether a disease of the udder of the cow, called "garget," may not so effect the secretion of milk as to give rise to diphtheria in the human subject. So far, this notion is a mere conjecture unsupported by fact, though Mr. Jacob, Medical Officer of Health for East Surrey, who has observed two or three outbreaks of diphtheria due to milk, reports that in certain instances there was reason to suspect the existence of "garget" at the farms whence the milk was supplied.

The great majority of the cases give statistical as well as experimental support to the conclusion that the responsibility of the epidemic lay with the milk. But

even in the cases where an apparently small proportion of the milk drinkers suffered, argument against the milk supply, based on the statistical facts only, cannot be regarded as conclusive. For it must be remembered that disease-germs are in all probability particulate in their nature, and are not diffused equally through the whole mass of the milk, even if the whole came from an infected source —which is by no means invariably the case. Urban milk-dealers especially, are accustomed to receive milk from different farms, and to mix the produce in their cans before sending it out, This in itself is sufficient to account for the apparent caprice of the attacks in certain cases, to say nothing of the varying degrees of receptivity in the individuals drinking the milk. We find, moreover, wherever the facts have been recorded, that it is upon the largest drinkers of the milk (those namely, who, consuming the greatest quantities, have a correspondingly greater chance of imbibing some disease-germs) that the incidence of the disease chiefly falls. Thus, young children (ordinarily little liable to attacks of typhoid), who are accustomed to drink milk largely in the raw state; domestic servants, who, after children, drink the most raw milk; and large milk drinkers of every rank and station, furnish by far the largest quota of cases in each epidemic.

(For striking cases of this kind, see column 14 of Cases 3, 8, 35, 37, 50, and others.)

People, too, who drink exceptionally of the implicated milk, whilst on visits to other houses, are found to be attacked, although the milk taken at their own house is derived from other sources. Instances, too, are frequent of persons in the same families as those who are attacked escaping the disease, apparently for no reason than that they do not drink, as their relatives who were attacked did, their milk in an uncooked state.

The houses invaded during these epidemics are found to be commonly of the better class, and in healthy situations. The poor, who take very little milk, and that only in tea or coffee, commonly escape the disease (See Worthing, No. 50).

Numbers of the reporters speak of the striking fashion in which the disease picks out the streets supplied by the implicated dairy, and the houses in those streets receiving the milk. People in adjacent houses, and who drink milk supplied by different retailers, escape: and when supplies from two sources enter the same house, the disease only attacks those drinking that from the infected source. It is only to be expected that some secondary cases in families should occur, although the sufferer may not have drunk the milk; but the contemporaneous invasion of so many households at once, and of so many members of those households at once, can only be explained on the hypothesis of a common cause acting on a particular set of persons and no others.

Further evidence is to be found in the sudden onset of the disease, and in its equally sudden abatement when the supply of milk from the infected dairy is stopped. Indeed, the whole chain of evidence from first to last leads irresistibly to the conclusion that the milk has been at the bottom of the mischief.

This being so, the question of how such outbreaks can for the future be avoided, becomes a question of urgent concern. As this paper is not solely for an English audience, I refrain from giving details of the illusory character of our British legislation on the subject. I will only say that no assurance can be obtained that these epidemics will not be as rampant as ever unless and until some really efficient and carefully considered regulation and inspection of dairy-farms and milk-shops be insisted upon. We are constantly being told by Medical Officers of Health of

the insanitary condition of dairy-farms; and when an epidemic due to infected milk occurs, the reporter, whilst able to trace the epidemic to one particular farm out of a number that may contribute to the supply of the retailer whose customers are attacked, often tells us of insanitary conditions existing at the other contributory farms that, under circumstances favourable to the spread of infection, might equally have sufficed to have caused the outbreak.

There are now a few large public companies who, following the example, and imitating more or less accurately the system inaugurated under my advice by the Aylesbury Dairy Company, systematically inspect the sanitary condition of the farms whence they draw their supply, and whilst indemnifying their farmers from possible loss when infectious disease breaks out at the farms, impose a very heavy penalty upon them if they fail to report the existence of such a case to the company. Such an agreement could not often be made in the case of private retailers; but it should at least be compulsory upon these and upon dairy-farmers to conduct their business under a licence, and to be liable to lose that licence if they permitted a case of infectious disease on their establishments to remain unreported to the local sanitary authorities. This latter, in its turn, should have the duty of inspecting all dairy-farms and milk-shops, and of seeing that the business is conducted in a cleanly fashion, and that the sanitary arrangements of the premises are unexceptionable.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk.	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
1857. Oct. and Nov. 1.	Penrith	M. W. Taylor, M.D.	...	...	...	100	14	7	50	Cottage of milkman was small, ill-lighted, and unventilated, and contained only two rooms.	A case of typhoid was imported from Liverpool to this cottage. Several of the children were attacked by the disease. The milk was brought direct from the byre into the kitchen, where the children lay; the mother, who was the nurse, milked the cows, and the milk was taken out in tin measures and pitchers for distribution to the customers.	Cases occurred in rapid succession amongst children and young persons in seven houses in different localities in the town, which had, for a considerable time previous, been very free from fever. These houses were all supplied with milk from the dairy in question.	...	*Edin. Med. Jour.*, May, 1858. *Brit. Med. Jour.*, vol. ii. 1870, p. 623.
1870. July and August. 2.	Islington (part of)	E. Ballard, M.D., M.O.H.	175	30	175	100	142	70	49	Underground water-tank at business premises, where milk was sold, found to be in communication, by means of rat-burrows, with two old drains. Acci-	No evidence to show specific pollution of this tank water.	Cases mostly occurred within a limited area. Families attacked well-to-do. Nothing found common to houses invaded except milk supply. Outbreak was sudden, and came without premonition. In rest of parish there were but few cases. A total of 142 families were supplied with milk from the dairy in question out of considerably more than 2,000 families	97 females over 10 years of age had the fever, against 44 males. Of 47 families in which multiple attacks were observed, it occurred first 20 times in a woman over 20 years of age, and	*Brit. Med. Jour.*, vol. ii. 1870, p. 589. *Med. Times and Gazette*, vol. ii. 1870, p. 611 (complete paper).

tank. Milk cans washed at pump connected with this tank.	in 75 per cent. of the families. There was no instance of two families being attacked in the same house, except when both families used the milk.	and 2 servants, all had typhoid, except the father and mother; the children commencing. In another, a girl and boy who ate milk porridge, and the 2 servants, who preferred milk to beer, were the only ones attacked. A young lady, whose family was supplied by another purveyor, fancied this milk, and used to drink a glass daily. She was the only person in the house attacked. In a street of 50 villa residences only 2 were supplied by this dealer, and only one was invaded by typhoid. The family consisted of father and mother, 3 boys, a baby, 2 servants, and a governess. Two sorts of milk were supplied, the ordinary milk and "babies'" milk, the latter used solely for the infant. The occupier on one occasion tested the ordinary milk, and inferred that it was one-fourth water. All the boys had bread and milk for breakfast. They, their mother, and one servant, were attacked, the baby remaining well. At the second house in this street supplied	

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
													by the dairyman nothing but "babies'" milk was taken in, and no fever occurred.	
1872. July and August. 3.	Armley, near Leeds.	E. Ballard, M.D.	107	11	...	...	132	51 out of 68 invaded.	38	Sanitary surroundings of farm bad. Large dung-pit five yards from well. Privy full of excrement, and liquid matter soaking from it. Well sunk in porous soil, and evident oozing into it of black sewage matter. Deposit of mud and filth at bottom. Result of analysis very unsatisfactory.	Dairyman was ill in May with typhoid. In the latter part of his illness copious rains fell, and probably washed virus from dung-pit (where dejections were suspected to be thrown), or from polluted soil into the well. It was about this time that the cause of the fever began to operate amongst the consumers of the milk.	The cases were located within a limited area, where the houses were comparatively new, and built in airy situations. Epidemic began suddenly, and almost as suddenly abated. In the early weeks of its prevalence all but one of the families got their milk from the suspected dairyman, whose business coincided to a considerable extent with the locality of the fever. 38 per cent. of the families supplied by this dairyman were attacked with fever, while only about 5·3 per cent. of families supplied by 18 other milk-sellers, or not taking milk at all, suffered. [Handle of pump chained up. Epidemic suddenly ceased to operate amongst customers of dairy a fortnight later.]	A lady, aged 53, who very rarely left the house, was the only one in a row of 12 new houses who was attacked. This row was beyond the district supplied by the milkman, so a son of the lady used to fetch a quart of milk every afternoon, of which she took a considerable part every night for her supper. In the notes of 19 cases it was especially mentioned that the suspected milk was very freely used, commonly at supper, either cold or boiled.	"Reports of Medical Officer of Privy Council and Local Government Board," vol. ii. 1874, p. 79.
1872. Oct. and Nov. 4.	Leeds.	M. K. Robinson, M.D., M.O.H	93	14	80	86	...	...	...	At a farm from which a milk vendor obtained an important part of his supply, typhoid fever had prevailed to con-	Poison may have got into milk either (1) by being carried along with emanations from the sick, and deposi-	Many of the houses invaded were perfectly free from sanitary defects. A very large proportion were supplied with milk from this particular dairy. In one street only 4 houses, and in another only 1 house, took in this milk. All 5 were invaded with fever.	In some instances the only members of a family who suffered were those who habitually took milk or large quantities of cream in	*Brit. Med Jour.*, vol. i. 1873, p. 68.

										cases occurred at the house. Water supply pure, but sick room communicated immediately with kitchen and dairy, and was generally open, so that air of sick room was common to both kitchen and dairy. A huge heap of manure, very close to house, into which a drain from kitchen sink and slopstone poured its contents. The common privy, which received the typhoid excreta, was overflowing.	they remained in the house; or (2) by absorption of the typhoid miasmata on the premises. The milk cans were taken into the kitchen, which opened direct into the sick room, to be washed and dried. Unclean linen was seen placed side by side with lids of cans. Beds were placed down stairs, so that double duty of attending upon the sick and the ordinary work might be more conveniently performed. With such arrangements, abundant opportunity must have existed for contact of fever poison with milk-vessels.	supplied from other sources.		
1872. Nov. and Dec 5.	Moseley and Balsall Heath.	E. Ballard, M.D.	96	10	...	...	...	47 out of 50 invaded.	...	Two milk-sellers lived next door but one to each other. They both got their milk from farms in the country. Wells attached to houses situated within a few feet of pervious ash-pit privies. They were superficial,	A case of what was, no doubt, typhoid fever occurred in the house between these two milk-sellers early in November, and the dejections were thrown into the privy. Thence the poison must have found access	Outbreak was sudden in its onset. Of 50 families invaded, 47 derived their milk wholly or in part from the the two milk-sellers. A large number of other sellers, supplying families even in the same streets as those invaded, were free from fever amongst their customers.	Of all the cases attacked, only 6 were males above 15 years of age; the rest were all women and children. 61 cases were under the age of 14 years. two boys connected with the milk shops, who drank abundantly of the milk, were attacked.	"Reports of Medical Officer of Privy Council and Local Government Board," No. ii. 1874, p. 92. *Brit. Med. Jour.*, vol. i. 1873, p. 68.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
										and showed, on examination, signs of soakage.	to the water of the well. One of the milkmen undoubtedly added water to his milk, and the other made no profession of selling it pure.			
73-Jan. 6.	Parkhead (Glasgow).	J.B. Russell, M.D., M.O.H	53 in 39 families	0, all amongst the milk drinkers.	46	86	...	32		... ...	Typhoid fever occurred amongst children of dairyman in December. Person who habitually milked the cows also nursed the affected children. [Probably, therefore, her hands would be frequently soiled.] Loch Katrine water, direct from the main, used for all purposes.	Parkhead is virtually a country village. No community of drainage. Water supply from Loch Katrine. Fever usually conspicuously absent from suburb. In January, 1873, many families suddenly invaded with typhoid. Outbreak distinctly localized, and limited in duration to January, especially to fortnight between 8th and 21st. 46 cases amongst customers of 1 dairyman, X., selling milk of 10 cows. Amongst customers of 8 other dairymen, selling milk of 64 cows, there were only 7 cases. In 5 streets where milk supply of every family was ascertained, 32 out of 73 supplied by particular dairyman had fever; while of 146 supplied elsewhere, only 2 had fever. Of the 46 cases amongst X.'s customers, 6 sickened in first week of January, 17 in second week, 14 in third week, and 9 in the fourth week. In the 7 cases supplied elsewhere there was no such clustering together.	In two families supplied from implicated dairy, the two individuals seized were the only members of the family using that milk.	*Glasg. Med. Jour.*, August, 1873. *Brit. Med. Jour.*, vol. i. 1873, p. 291.

											ness, and subsequently the baby and the mother were attacked.	attacked were usually better off in this respect than the majority.	children, all of whom were attacked, except the baby at the breast. These children used to drink a good deal of milk (got from the implicated dairy).	
1873. July and August. 8.	Marylebone and adjoining districts.	J. Netten Radcliffe and W. H. Power.	244 in 143 households.	20	218	80	700	118	15	One of the farms supplying a London dairy company was situated in Buckinghamshire. Farm-house and diary old and incommodious. Water for dairy purposes obtained from a well in a yard adjoining the farm-house. Position of well such as to render soakage of foul matters into it not improbable. Water used cold for cleaning dairy vessels, and simply as taken from well. [No evidence obtained of any actual mingling of water with the milk.]	On the 8th of June the occupier of the farm died suddenly of ambulant typhoid fever. Stools and chamber-slops from this patient buried as a preventive measure in an ash-heap, but position of ash-heap such that its soakings must have found their way into the well used for dairy purposes. Heavy rainfall on 29th of June probably assisted to wash the infected soakings more copiously into the well.	All the cases occurred in the 10 weeks ending 30th of August, and within a circumscribed area. By far the greater number of families invaded were those of well-to-do people. Cases took place at a time when there was no corresponding increase of typhoid in neighbouring districts, and when the metropolis was, and had been for many weeks, remarkably free from the disease. The number of localities (streets, squares, &c.) invaded was 88, and of those 48 had only 1 case in them. The localities chiefly affected were 12 in number, and in them occurred 108 cases. 76 houses had solitary cases, 67, 2 cases or more. There was no community of sewerage, drainage, or water supply; but nine-tenths of the cases were in households consuming milk from a particular dairy. Wherever this milk was distributed, there the cases of enteric fever forming the outbreak were distributed. Where the milk supply stopped short, there, as a rule, the cases stopped also. The map of the distribution of the milk supply represented very closely the area of the outbreak. In certain localities where ramification of the milk supply extended, the cases occurring were, with one exception, among families using this milk. The disease picked out the streets to which the milk was distributed, and	93 of the cases (or 37·7 per cent.) were of domestic servants, 90 were children, and 119 were women, leaving only 39 adult men. Very numerous instances of milk-drinkers of families being the only persons attacked in the houses —*e.g.*, the mistress, and kitchen-maid were the only persons in a family who habitually drank uncooked milk. They were both attacked. A physician, in the habit of taking nightly half a pint or more of cold milk "as it came from the dairy," was the only person in his house who did so; and he alone had fever. A clerk to the dairy in question drank much milk in office-hours, and was attacked; but there was no illness among other mem-	"Reports of the Medical Officer of the Privy Council and Local Government Board," No. II. 1874, pp. 103-138. *Brit. Med. Jour.*, vol. II. 1873, pp. 200, 207, 290, &c. *San. Record*, vol. II. p. 390.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk.	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
												the houses in these streets which received the milk. Of 88 infected streets this supply went to 77, and of 132 infected houses in the 77 streets, 14 only did not receive the particular milk. In 118 houses out of 760 houses taking the milk, there were 218 cases of enteric fever, while in the 14 houses attacked out of the 23,000 in the same area not taking the milk there were 14 cases only. Striking confirmatory evidence was afforded by an isolated group of 12 cases in another part of the metropolis. Here there was a branch of the particular dairy, and 11 of the 12 cases purchased milk from it across the counter. The number of families buying milk from this branch-shop probably did not exceed 30, and of these 7 were invaded by fever. Partial incidence of fever upon customers of dairy explained by fact that implicated farm only supplied about one-sixth of the ordinary milk distributed by the dairy. It supplied, however, all the "nursery milk," which, when not wanted for its specific purpose, was mixed with the ordinary milk. In the early part of July, as the customers began to leave town, this "nursery milk," of which the same quantity was always sent to town, got to be more and more used for ordinary purposes. Inquiry tended to prove that the ordinary milk was infected only to such extent as "nursery milk" might have become mixed with it.	bers of his family, who did not take the milk. A child spent an afternoon with a family supplied from this particular dairy, and drank nearly 2 pints of milk. She, as well as 10 members of the family whom she visited, had fever. Many instances of seizure of large consumers of the milk, notably in servants who were in the habit of drinking the milk cold. These were the only persons invaded in the households—*e.g.*, in a family of 12 (7 adults and 5 children) the nurse-girl, the cook and 4 children had enteric fever. A brother, who was a teetotaller and a great milk-drinker, died of fever, and so did a clergyman who took habitually 2 tumblerfulls of milk daily. A duke and a physician living next door to each other were sup-	

												plied from the same can. In the duke's household there were 2 severe cases of fever; but the physician's family (who boiled all their milk) escaped, although there were children in it. [Examples of a similar kind could be multiplied.]	
1873. July, to 1877. November. 9.	Ascot.	E. Ballard, M.D.	69 in 40 families.	58	84	100	31	31	Business of dairy conducted in a slovenly manner. Drainage bad and offensive; cess-pit privy on the premises. Water supply derived from a well the pump-case of which was in atmospheric connection with the drains. Empty milk cans inverted over the sink and over the opening of drain pipe in the kitchen. Cans liable to befoulment even during process of cleansing, and water used for rinsing dangerous, being exposed to soakage of foul matters from privies and manure heap; and from absorption of the foul emanations from the drains which communicated with the pump-case.	No case of fever occurred at the farm till August, 1876, when the epidemic had lasted for 3 years. From this time onwards the water used for washing and rinsing the milk cans had an opportunity of specific pollution. Dr. Ballard could not account for the contagion reaching the farm, except that it had reached it in some way through the water entering the well in the yard. One of the cases of the great Marylebone epidemic of 1873 was nursed at the summit of the hill on which Ascot stands, and the evacuations were thrown carefully away, at a distance from the house, into a hole in the earth. The soil is a loose sand	There was no community of water supply or drainage: and insanitary conditions, though present, failed to account for the circumstances of the cases. Of 3 milkmen supplying milk in the village, 31 per cent. of the customers of one were invaded, 6·5 per cent. of a second, and 1 of a third. Cases infected in the district, and using the implicated milk, were 50 as against 6 amongst persons not using that milk. In these latter cases the attacks could all be explained, independently of the circumstances of a particular supply of milk having been drunk. [Epidemic only ceased when alterations were made in the drainage and pump of the farm.]	36 of the cases were of young people under 16 years of age. Different families inhabiting at various times houses customarily let for the summer season, were invaded, when milk was supplied from the implicated dairy, but not at other times. A family never used anything but condensed milk, but one of the boys, who resided with his grandfather in a neighbouring cottage supplied by this dairy, took the disease, and so did a younger brother who afterwards went to live with his grandfather.	*Brit. Med. Jour.*, vol. i. 1880, p. 83. "Report of Medical Officer of Local Government Board for 1877," p. 39.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk.	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
											and Dr. Ballard suggests that the specific matters thrown with the evacuations into the hole, in the very permeable sand, at a place not very far above the water-layer, could, with the help of rain also running into the hole, have scarcely failed to reach the water-layer beneath, and having reached it, would become more or less diffused in it; and, in the course of time, the movement of the mass of subsoil-water might have brought it to the well at the farm.			
1873. August. 19.	Brig-house (York-shire).	T. Britton, M.D., M.O.H.	68	8	65	95	...	...	..	Cows healthy, but cesspool, 9 ft. × 6 ft., on premises in connec-	... ...	Complaints of milk smelling badly and becoming offensive after standing awhile. In several instances, the only cases in a street or row were	... ...	*Brit. Med. Jour.*, vol. ii. 1873, pp. 267 and 334

									from this cesspool.		from suspected dairy being the only one falling ill with fever. [Supply of milk stopped, and 4 days after the last case occurred.]		
1873. August. 11.	Wolverhampton	J. H. Love, M.O.H.	63	14	...	...	...	...	Pump well of milk vendor's house within a few inches of an old flat-bottomed brick sewer which drained a locality where a number of cases of enteric fever had occurred.	On August 11, 1873, two of dairyman's children sickened of fever, and from this time the epidemic began to spread markedly in other quarters of the town. Disease may have been spread by addition of infected water to milk, or by absorption into milk of infected air.	The disease followed the course of the milk supply. Of 18 cases on the list of one practitioner, 15 were supplied with this milk. Majority of persons attacked well off, and living in good houses. [Epidemic stayed by cutting off the supply of pump water used by the milkman.]	...	*Brit. Med. Jour.*, vol. ii. 1873, pp. 267, 292, 334, 447.
1874. May and June. 12.	Brierley, Lanc.	R. Thorne Thorne, M.D.	65	4	50	76	38	...	...	Water of well of dairy analyzed, and found "little better than filtered sewage." No evidence, however, that well had become polluted with typhoid excrement.	First person attacked was the dairyman, who was in the constant habit of visiting Bradford, where fever is always present. During his illness his wife both nursed him and milked the cows, and his linen was washed at the same sink at which the milk cans were cleansed. To some accident arising under these circumstances, Dr. Thorne was disposed to ascribe the infection of the milk. The husband also, on his recovery, helped to nurse his	50 persons out of the 38 families supplied by dairyman took the fever, while only 15 out of the 49 families who did not purchase it were attacked. Unwholesome sanitary conditions of the place doubtless fostered the epidemic when it had been once started by the milk.	A young woman, not a customer of the dairy, used to visit and take meals (including tea) at a house where this milk was regularly used. The last of these visits was made a fortnight before she was taken ill with fever.	*Brit. Med. Jour.*, vol. ii. 1874, p. 391. *San. Record*, vol. i. 1874, p. 214.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
											wife, who had been meanwhile attacked, while he was performing all the business of the dairy.			
1874. June. 13.	Taunton.	H. J. Alford, M.D., M.O.H.	5	...	5	100	...	5	..	Well water of dairy "fearfully contaminated" with sewage, and smell very offensive. Well saturated with sewage, which was clinging to the bricks where it had trickled down. Privy choked, with no flushing apparatus. Milk cans kept in a small back kitchen, in which were a pump, a small lighted furnace, and a large barrel full of pig's-wash.	...	Sanitary circumstances of invaded houses good. In one house all the inmates but one drank milk from another dairy; but the one who suffered from fever drank this particular milk.		
1874. August. 14.	Queensbury.	T Britton, M.D., M.O.H.	24 houses.	1	34	94	...	31	...	Well very close to house drain, roughly made of stone. Earth between drain and	Farmer's wife sickened of typhoid during outbreak.	Drainage and water supply of infected locality fairly good. All the cases but 2 were supplied with milk from farm. [On drain being removed, and well	... ...	"Annual Report of Medical Officer of Health" for 1874

									to admit of percolation.				
1874. October and November. 15.	Dundee.	G. C. Pirie, M.D., M.O.H.	19	4	19	100	..	...	Well used for all domestic purposes, and for washing milk vessels and utensils, had near it two dung-heaps where fœcal evacuations of sick were deposited. Water reported by Dr. Stevenson Macadam to be "contaminated with the products of decomposing organic matter of the nature of sewage."	4 cases of typhoid fever at farm. The bedroom occupied by all 4 during their illness was on ground floor. Opening from this apartment was the milk store—subsequently moved outside house.	All the cases supplied by particular dairy. Cases all occurred within a period of 6 weeks, although, before that period, typhoid was very sparingly, if at all, observed in that district.	All cases those of young people whose ages ranged from 2 to 21 years.	*Brit. Med. Jour.*, vol. i. 1875, p. 225.
1875. Feb. and Mar. 16.	Crosshill (Renfrewshire.)	H. D. Littlejohn, M D., E. Duncan, M.D.	153 in 112 families.	...	...	...	...	94	...	Byre ventilated into house, and apartment in which milk and utensils were kept formed part of the house, and was situated close to byre. Water supply derived from well in close proximity to byre, and to a large collection of liquid manure from which much refuse escaped. Well furnished by supplementary supply of water from spring dangerously exposed to pollution from a ditch. Analysis showed water to be decidedly impure.	Typhoid fever was very prevalent in village (Eaglesham), where farm was situated, and 2 of the farm children had suffered from the disease. Their discharges were thrown either on the manure heap or into the ditch. Their attendants superintended the arrangements for the collection and disposal of the milk; and the connection between dairy and rest of house was free and open. Disease subsequently broke out in house of 1 of the retailers of the Eaglesham milk at Crosshill.	General sanitary state of Crosshill very good. Sewer ventilation defective, but this shown to have been but little concerned in epidemic. Of 504 houses, disease appeared in 112, 94 (or 84 per cent.) of which were supplied, wholly or partially, with milk from Eaglesham. No dairyman who drew his milk from Eaglesham escaped having disease amongst his customers; while there were 7 dairymen (in addition to others having a few cases, though not supplied from Eaglesham) who had their milk supply from other districts, and had no disease amongst their customers at all. At Langside, a village quite distinct from Crosshill as regards its drainage, fever appeared in 6 of the 46 villa residences, attacking 16 persons, but completely sparing the village proper. Of these 6 villas 5 were supplied with milk from Eaglesham. 42 other villas, and the rest of the village supplied by local dealers, escaped entirely.	90 of the cases under 12 years, 63 over 12 years. The cases in the 6 villas at Langside were 13 of them children, and 3 servants.	"Official Report to Board of Supervision." *Brit. Med. Jour.*, vol. i. 1875, p. 391. *San. Record*, vol. ii. 1875, p. 61.

1	2		4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk.	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
											The house adjoined the shop, with which there was free communication. Doubtless this gave an impetus to the dissemination of the poison.			
1875. Aug. 17.	Jarrow.	John Spear, M.O.H.	34	2	31	91	27	114	40	Water used at dairy derived from a well situated 18 yards from a privy and cesspit. The latter merely a hole in the ground. Well supply bricked loosely round; and unquestionable soakage into it from privy and cesspit. A quantity taken from the well found to be quite putrid two days after.	6 of the farmer's family (including himself), found ill with typhoid. Their evacuations were thrown into the privy. Direct communication between dairy and a room in which some of the fever patients were lying. Dairy used also as a washhouse, and clothes from the fever patients had been washed there. The person who milked the cows (a daughter of the farmer) was constantly about her sick relatives, if not in actual attendance on them.	District had been tolerably free from typhoid for some weeks, when suddenly, on August 13, it invaded houses in all parts of the town. Between August 3 and 19, 34 cases occurred in 20 different families. Houses invaded clean and wholesome, with but 2 exceptions, and their water supply and drainage were good. Nearly all had their milk from this dairy. When the disease was not traced absolutely to the milk, solitary cases only occurred; whereas, in a large number of the cases, undoubtedly due to milk, there were multiple cases. 18 other farmers supplied milk to the town in at least equal quantity to the implicated dairy; but their names, with one or two exceptions, were not heard mentioned during the inquiry. Milk locally distributed in small tin bottles for every customer (instead of capacious cans) holding, perhaps, a pint or less, and this bottle is supplied with a cork, which generally has a piece of rag tied round to make it fit closely. Danger of milk becoming contaminated largely increased by	More than half the number of cases were children. One man only in a family had typhoid, and he alone drank milk. In another case, 2 of 3 children suffered; the third, who did not like milk, and did not drink it, escaped.	*Brit. Med. Jour.*, vol. ii. 1875, p. 372; *San. Record*, vol. iii. 1875, p. 195.

											[Milk supply immediately stopped and epidemic at once stayed.]			
1875. Sept. 18.	Glasgow (*a*) Washington Street epidemic	J. B. Russell. M.D., M.O.H.	59 and 30 suspicious cases.	3	58	98	493 individuals.	58 individuals and 30 suspicious cases in 37 families.	11	Boiler-house of farm (situate in country) contained three boilers for clothes-washing, for scalding dishes, and for cows' meat, all built together. Close to the boilers was a pump well. Drinking water brought from a well at some distance. Both waters analyzed, and found not above suspicion.	On the farm there were 2 cases of typhoid, and in the washing-house washing was done for the sick persons on 3rd, 10th, and 27th of August. First explosion of fever in Washington Street took place in last week of August.	Washington Street mostly inhabited by a very respectable, cleanly, well-to-do class. Previous health—character good. Sudden and serious prevalence of typhoid in September, 1875, and many cases of slight derangement of health—nausea, giddiness, loss of appetite, slight diarrhœa. Between 25th of August and 27th of September, 42 persons seized with typhoid, and 27 with form of sickness described. This, in a population of 625, equals 11 per cent. ill during a month. In the whole area invaded, of 367 individuals using X.'s milk solely, 55 had fever and 14 had suspicious sickness. Of 116 using it partially, 3 had fever, and 16 were suspiciously ill. Of 579 persons not using the milk, only 1 had fever, and there were no suspicious illnesses.		*Brit. Med. Jour.*, vol. ii. 1875, p. 535.
19.	(*b*) Pollokshaw's Road and Kingston epidemics.	...	121	3	98	81	...	64	...	(See above.)	Bulk of milk from above farm had been sent to dealer supplying Washington Street, but part went to a dealer who served Pollokshaw's Road, &c. In September, dairy supplying Washington Street ceased to receive any part of the milk of the farm, but destination of milk was concealed.	In consequence of difficulty of getting information as to exact destination of milk from farm, the exact facts cannot be given; but of 199 cases of enteric fever registered in the whole of the Southern District of Glasgow (where these localities are situated), 98, or 49 per cent., certainly got milk from dairies supplied wholly or in part from farm X. In Pollokshaw's Road area, 55 cases occurred, of which 40, or 73 per cent., were supplied with X.'s milk constantly. In Kingston area there were 66 cases, of which 58, or 88 per cent., were supplied constantly. Acme of epidemic attained in second week of September, instead of last week of August, as in (*a*). Dates of infected washings at X.'s, along with diversion of milk from Washington Street to Southern District, probably explain this difference.		

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk.	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
1876. January and February. 20.	(a.) Eagley and Bolton.	W. H. Power (to Local Government Board). J. Robinson. M.O.H., Eagley.	195	13	...	...	63	55	87	Farmhouse depends for its water supply upon a brook which had been very much befouled with the excrement of men engaged in building a mill 200 yards off. All the families usually using the brook water for drinking purposes had on this account discontinued its use, except the farmer and one other family. The former were attacked by fever about the same time as the milk outbreak; the latter (who boiled milk before use) escaped.	Large quantities of fæcal matters found on course of brook, and there was "abundant evidence that some individual who had used the stream had sufferred from diarrhœa." This diarrhœa was very possibly of a typhoidal character, and may account for suddenness of outbreak. [Towards end of January it was generally noted that something was wrong with the milk. It turned sour almost at once, and is described as having been of a peculiar colour, to have tasted unpleasantly and even to have smelt offensively. In many instances, after standing, was left at the bottom of the vessel a sedi-	Eagley had been for some months comparatively free from fever or other illness. Towards the end of January a few dropping cases of fever preceded a great outburst on the 30th of that month, when the families were invaded, and twenty persons were attacked. In next six days, 103 more persons in fifty families were attacked, and after this the outbreak declined. Altogether there were in 63 households 195 cases with thirteen deaths. There was no community of drainage or water supply; but a very large proportion of the families invaded were customers of a particular dairy. Of 59 families thus supplied, 55—96 per cent.—were attacked; of 261 not supplied, 8, or 5 per cent., were attacked. In 6 of these 8 cases the members attacked had been, prior to their illness, partakers at neighbours' houses of the milk in question. Ten families had 5 cases; eleven, 4; and thirteen, 5 or more.	Particulars as regards habits of milk consumption were obtained from 35 families, comprising 161 persons. Of these 83 habitually drank milk in a raw state, whilst 75 took it in tea or coffee, or not at all. Of the former 79 (95 per cent.), and of the latter 18 (22 per cent.) were attacked.	*Brit. Med. Journ.* 1876, pp. 201, 233, 273, 293, 491,

	(cont.) Bolton.	W. H. Power. E. Sergeant, M.O.H.	144	8	..	...	50	47	94	(See above.) Half the total milk of the dairy was habitually retailed at Bolton (two to three miles distant).	sand, or dirt.] (See above).	Disease followed unerringly track of this milk supply. Not one household to which the milk was traced was found entirely free from disease. Houses, clean and dirty, were attacked indiscriminately. Of 900 families in the streets through which dairy cart passed, 50 were attacked, 47 of them being customers of the particular dairy. A total of 144 cases occurred, 8 of which were fatal. Eight families had 3 cases; three, 4; and eleven, 5 or more.	A person who drank half-a-pint of milk "just as it came from the can" suffered considerably for three weeks after. In a family of nine, 8 of its members who drank the milk had typhoid fever. The mother who did not like it, and took none, remained perfectly well.	
1876 February. 20.	Greenock.	J. Wallace, M.D. M.O.H.	20	...	..	...	...	...	...	...	A farmer allowed a case of enteric fever to be brought into his houses, and, after a short time three servants and several members of the family became affected, one after another.	Upwards of twenty cases occurred amongst the persons using this milk.	...	*Brit. Med. Jour.* vol. i. 1876, p. 425. *Sanitary Record*, vol. iv. p. 234.
1876. Autumn 21.	Churwell and Morley.	J. C. Clarke, M.O.H.	"A great number."	9	..	...	...	...	...	Water obtained from a well on the farm premises, found on analysis to be altogether unfit for drinking purposes. (Farmer denied having used it for dairy purposes.)	A case of fever at this farm.	Fever followed the course of the milk supply, picking out the families to which it was delivered, not only in Churwell, but also at the east end of Morley. Out of 25 to 30 cases occurring at the latter place, all, with one exception, drank milk from the farm. Many of the houses were healthy; some were new, with good drainage and water supply. (Epidemic subsequently spread by unwholesome middens and polluted water.)	A woman who drank much milk, "cold, or boiled, or anyhow," had a bad attack, while her husband and child, who took only a very small quantity, escaped. Another family (related to the first) in the same house, had milk from another source, and also escaped. Two cases at a clean, well-ordered house were both large milk drinkers.	

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1876. November. 22.	Great Coggeshall.	R. Thorne Thorne, M.D.	28	...	28	100	Cannot be stated.	...	...	Water taken for purposes of dairy from a polluted brook, only a few yards from where drain emptied into it.	A young woman came home from London with what turned out to be typhoid. The slop water (including the swillings of bed-linen saturated with the patient's bowel discharges) were poured, without disinfection, into a drain emptying into the brook already referred to.	Nothing common to the persons attacked, except their milk-supply. There were 4 milk vendors in the town, but only the customers of the particular dairy were attacked.	...	"Official Report to the Local Government Board."
1876. December. 23.	Salford.	J. Tatham, M.O.H.	13	...	13	100	...	...	...	One of the five farms from which a Salford retail dealer drew his supply situated on a very porous and sandy soil favouring percolation. Surface drainage very imperfect. Well close to privy-cesspool, and a yard or so off a sink for dirty water.	Typhoid had been epidemic at this farm for a long period, 16 cases occurring within 20 years.	Cases occurred almost simultaneously in houses inhabited by well-to-do people. Drainage and water-supply of houses perfect, and no intercommunication between the families. Milk supply, with scarcely an exception, from the same retail dealer. (Supply discontinued, and no further cases occurred.)	Those members of the various families who largely partook of the milk in its unmixed form, were alone affected. Those who only took it boiled or mixed with tea or coffee escaped altogether.	"Annual Report of Medical Officer of Health for Salford, 1875-1876."
1876-7. Dec. and Jan. 24.	Barrowford (Lancashire.)	T. Dean, M.D., M.O.H.	57	7	57	100	50	37	74	Supply of water to farm defective.	Farmer had had typhoid in his house for two or three weeks be-	Cases occurred within a very short space of time. Every one of them drank milk from this farm. (Milk supply stopped, cow-sheds	Ten of the sufferers under 5 years of age; 12 between 5 and 10; 9 be-	*Medical Times and Gazette*, vol. i.

											fore outbreak, and no precautions had been taken to prevent spread of disease. The milk was kept for some time in the farm-house before being sold. The milk-tins were washed with the same "dish-cloth" as used amongst the fever patients. Farmer himself nursed his children, and then went immediately, without disinfection, amongst his cattle, and milked them in the same clothes.	disinfected, milk and cream at farm destroyed. All customers of farm visited, milk emptied into sewer, and boiling water placed in every vessel that had come in contact with milk. These, and other precautionary measures, at once checked outbreak.)	tween 10 and 20 years.	1877 p. 72.
1877. 25.	The Gurnos Ystalyfera.	H.F. Parsons M.D.	7	.	7	100	...	...	.	Milk dealer lived in a small overcrowded house, and stored his milk, previous to sale, in a badly ventilated pantry leading out of the living-room of the family.	At the time of the outbreak the dealer's son was lying in the house ill of enteric fever.	All cases attacked simultaneously. All in the habit of using milk procured from this dealer.	Nil.	"Report on Sanitary Condition of Pontardawe Rural Sanitary District, 1880."
1877. January. 26.	Greenock.	J. Wallace, M.D., M.O.H.	20	2	16	80	...	13	.	Nil.	Nil.	Of 20 cases of enteric fever brought to the notice of the Health Officer, the outbreak was, in 16 of the cases, traceable to contaminated milk.	Nil.	*Brit. Med. Jour.* vol. i. p. 118. *Sanitary Record,* vol. vi. p. 63.
1877. Feb. 27.	St. Pancras (part of North East district of) parish.	T. Stevenson, M.D., M.O.H.	35	2	30	85	Ab't 400	21	5	Milkshop clean. Milk derived from three farms and the home counties. On two of these farms water	Nil.	Part of parish infected unusually free from fever up to January, 1877. Sudden appearance of a considerable number of cases of typhoid, a large proportion of the patients sickening within a few days of one another	Where there were both milk drinkers and those who drank no raw milk in the same household, milk drinkers	*Brit. Med. Jour.* vol. i. 1877, pp. 273 and 339. *Med. Times and Gazette,*

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk	Percentage to total cases.	No. of families supplied by milkmen.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
										supply obviously contaminated with "ordinary forms of filth." Medical Officer of Health thinks the circumstances of the milk becoming the vehicle of typhoid must "remain a mystery."		Between January 15 and end of February 30 fresh cases reported, 26 of which had milk from X. During five days, from February 5 to 9, 12 fresh cases appeared in 11 houses, all supplied from X. Area of epidemic conterminous with X's milk-walk. In only 3 of the houses invaded were sanitary defects found. A young woman who had been confined to her room for several months with a bad leg contracted typhoid; her brother, who was in the employ of the milkman, was in the habit of taking home some milk from his master's dairy to his sister.	were especially attacked.	vol. i. 1877. p. 653.
1877. August. 28.	Edinburgh, (Coltbridge).	...	...	Several.	...	...	...	...	...	Sanitary arrangements defective.	A case of fever at a dairy.	Twenty families suffering from the disease when the outbreak was first noticed; other cases afterwards.	Nil.	*Brit. Med. Jour.* vol. ii. &c. 1877, p. 392.
1877. October and November. 29.	Tunbridge Wells.	W. H. Rix, M.O.H.	68 in whole borough, during 2 months, in 49 houses.	...	...	...	...	39	...	Dairyman got his milk from six sources, and there was a considerable mixture of the milk. District supplied with milk from 3 of the sources did not furnish a single case. From 2 of the other 3 certain families were exclu-	Nil.	(Arrest of disease followed almost immediately on the stoppage of the infected milk. Local Board bought up milk for three weeks and made away with it).	Nil.	

1877-8. Dec. and Jan.	Glasgow and Hillhead	J.B. Russell, M.D., M.O.H.	72 at Glasgow, 71 at Hillhead, 16 among University students elsewhere.	166	16	...	...	...	...	...	...	...	...	...	...	...	...	...	...
30.																			

town, where the sewage of the village flowed through the cow-yard. In the village there had been cases of typhoid fever. Water that cans were washed with came from a hill on the opposite side of the stream. Farmer's wife and children were continually ailing.

At one of the farms supplying a milk dealer in Glasgow, kitchen adjoined the byre, beside the door of which, and against the wall, stood a privy. From the middle of the byre the washing house was entered, and through this the milk house. Water supply chiefly from a deep well, found on analysis to be "very decidedly contaminated."

A servant, sick of enteric fever, had been lying in a bed in the kitchen. A son of the farmer had also been ill of the same disease. The work of the dairy was carried on by the persons who attended to the patients, and the excreta were thrown into the channel running on each side of the central passage provided in byres for the reception of the cattle droppings. (Dr. Russell does not say whether there was any reason to suspect soakage into the well, but he seems to have had no doubt that, in some way or other was retained by "A." These circumstances were also invaded, but not so fatally. Of the 166 cases, exactly half were customers of "A," and half those of "B;" but the mortality amongst the first half was 13·2 per cent., against 6 per cent. in the other half. The explanation of this difference is probably to be found in the fact that in "B's" trade the various supplies were sent out, so that no one milk could reach the consumer day after day, or without more or less admixture with other milk. "A's" milk went out for the most part either in bulk, or so that they could rarely get commingled. Moreover, the freshest portion of the milk (the morning milk) from the infected farm went to "B," whereas the portion milked the previous evening

Epidemic was sudden in its onset. The fever attacked only the drinkers of the suspected milk. Of 779 houses in a large number of streets, 122 were supplied with this milk, and 29 were invaded; whereas, of the 657 other families, only one was invaded. A milk dealer, "A," passed on some of his supply from this farm to another dealer, "B," whose customers were also invaded, but not so fatally. Of the 166 cases, exactly half were customers of "A," and half those of "B;" but the mortality amongst the first half was 13·2 per cent., against 6 per cent. in the other half. The explanation of this difference is probably to be found in the fact that in "B's" trade the various supplies were sent out, so that no one milk could reach the consumer day after day, or without more or less admixture with other milk. "A's" milk went out for the most part either in bulk, or so that they could rarely get commingled. Moreover, the freshest portion of the milk (the morning milk) from the infected farm went to "B," whereas the portion milked the previous evening

The milk-dealer supplied milk to the refreshment room of the University. At least 16 of the students were attacked at various places during the Christmas holidays, and 2 died.

Brit. Med. Jour., vol. i. 1878, pp. 101, 165, 270.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk.	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
											the poison had access to the milk.	cumstances tended to give the customers of the latter a larger proportion of the enteric poison in the same volume of the milk, as well as to subject them to repeated doses. Branches of "A's" business to which none of the infected milk was sent had no fever amongst their customers. (One of "A's" children, who probably drank the milk, took fever, and lay, until removed to hospital, in a room used as a sleeping and dining room, in and out of which dairy-maids and others passed freely.)		
1878. Jan. to March. 31.	Moss-side, near Manchester.	E. Sutcliffe, M.O.H.	32	3	29	90	...	...	...	Water of farm, whence came part of supply of dealer, derived from well in close contiguity to ashpits. This water found on analysis to be polluted from sewage contamination.	Two deaths from typhoid occurred at farm in February.	Typhoid had not previously been prevalent in the district, only one death being registered in 1877. With one or two exceptions, ashpits and drains in connection with invaded houses were in good order. Of a total of 32 cases, 29 occurred in houses receiving their milk supply from the same dealer. The particular dealer supplied milk from his own establishment, and from a farm eighteen miles distant. Those supplied regularly from farm principally attacked. Houses in infected district supplied by other dealers enjoyed a remarkable immunity. Amongst 214 houses supplied from one of the principal dairies, in only one did disease make its appearance; and in this case the inmates had held direct communication with one of the houses already infected.	Of 25 persons infected at Moss side, all supplied with milk from particular dealer, 20 were under 13 years of age.	*Medical Times and Gazette*, vol. i. 1878, p. 517.

feet from the well. The cesspool was overflowing in all directions, and its contents were traced by a recurrent course distinctly into the well. Manure of all kinds, and in every stage of decomposition, lay about everywhere. The premises had no isolated dairy, but milk was kept in the common living rooms. The well-water was the only water for domestic use on the farm, and was used for washing the hands of the milkers, and for rinsing out the utensils. Analysis showed it to be very impure.

Her evacuations (no doubt still infectious) passed into the common privy. One of the farm servants was removed thence, ill, on August 1, and died afterwards of typhoid.

from this farm, these persons being the customers of several retailers, all of whom, without exception, were supplied either directly or indirectly from this farm.

one family, 4 children drank milk, but only one of them was allowed to drink it unboiled. He was the only one of the family who contracted the fever. In another family, there were two domestic servants, one of whom drank milk, and the other never tasted it. The one who drank the contaminated milk contracted the fever, the other escaped. "The fever appeared first, and most severely, amongst those who drank milk which there was reason to fear had been the most contaminated—viz., that which had passed through two different hands."

Record, vol. ii. 1878, pp. 100 and 166.

* There was also a supposed outbreak of milk typhoid at Bristol (8 cases in 5 houses) in the spring of 1880. The only connection between these cases seemed to be their milk supply.

1878. August. 33.	Croydon.	C.W. Philpot M.D., M.O.H.	48	...	37	77	...	...	...	Nil.	Nil.	About the third week in August a sudden but limited epidemic of typhoid occurred at Croydon. "The incidence of the disease, according to a certain milk supply, was so marked as to lead to the conclusion that the fever poison was distributed with that article." Of 48 cases, of which information was obtained, 37 lived in houses supplied with milk from one source.	Nil.	"Annual Report for 1878." *Brit. Med. Jour.* vol. ii. 1879, p. 675.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk.	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
1878. September. 34.	Portsmouth.	G. Turner, M.O.H.	153 in whole borough during third quarter.	...	78	64 of the cases whose milk supply could be learnt.	...	...	...	Dairy itself clean and well kept; but when supply ran short, extra milk was obtained from a farm a little distance out of the town. At the time of the epidemic the milk supply was actually supplemented in this way. Well of this farm within a few feet of cesspit of a common privy; and water proved on analysis to be unfit for drinking purposes.	Farmer's children were suffering from typhoid, and no doubt poisoned the well; for two children who had, while walking, obtained some water at the farm were subsequently attacked by typhoid.	Sixty-four per cent. of all the cases in the borough in the third quarter of 1878, whose milk supply could be ascertained, were customers of the dairyman who supplemented his milk from this farm. A large number of the cases fell ill on the same day. The farmer supplied, besides milkman, 20 persons in his neighbourhood; of these, 8 had fever. He also supplied milkman who contracted for mess at Eastney Barracks, which was also invaded by disease. A milkman who lived just outside the barracks supplied quarters of married officers where, too, there were cases of the disease. (Farmer forbidden to sell any more milk. All milk in possession of 4 milkmen supplied by him bought by sanitary authority, and "disposed of down nearest drains.")	Nil.	*Brit. Med. Jour.*, vol. ii. 1879, p. 675.
1878. September. 35.	Colston, near Glasgow.	J. Christie, M.D.	40	...	40	100	...	13	...	Premises in very good order, and everything about the house and dairy scrupulously clean. Water supply on the premises very deficient, and delivered from a rain-water cask, and from a shallow dip-well which received part of the surface drainage	A bundle of body-clothing used by a person who had died from enteric fever (though the disease of which she died was not known to her relatives), was brought to dairy-farm to be washed. On the 14th and 15th days later, symptoms of enteric	Forty cases occurred in a very small community. Distribution of an uncontaminated milk supply from another farm, among people living under precisely the same circumstances, was not connected with a single case of enteric fever. Previously to the outbreak there had not been a case of fever in the neighbourhood; and after it no fresh case occurred. Several families suffered very severely, one family having 8 of its members attacked, another 7, and another 6. All the families invaded were supplied with milk from this dairy.	In nearly every instance in which members of a family escaped, their immunity was due to the fact that they either used buttermilk, or a small quantity of sweet milk in their tea, or none at all. Thus, in a family of ten, all except three children were attacked. Those attacked used sweet	*Sanitary Record*, vol. iv. p. 342.

										water being carried for a considerable distance.	children of the deceased woman, who were brought to the farm, was suffering from typhoid fever, or was in the stage of convalescence, and was consequently capable of spreading the disease.			
1878. October. 36.	Perth.	...	Several	...	...	...	...	...	...	Nil.	Nil.	All the families in which the disease appeared had their milk from one dairy.	Nil.	*Brit. Med. Jour.* vol. ii. 1878, p. 645.
1878. Dec. (Christmas.) 37.	Dublin.	C. A. Cameron, M.D., M.O.H.	...	...	67	100	42	30	17	Dairy in a small and confined yard in Dublin. Neither water-closet nor latrine provided for either house or yard. At the end of the yard an accumulation of cow-manure and litter. Refuse from household (including excreta) daily deposited on this ash-heap; contents of which were removed once or twice a week. No regular dairy apartment in the place. Some of the dairy operations conducted in the kitchen, which opened di-	A little girl, child of the proprietor, began to be ill with what was probably typhoid, late in November. About the middle of December, the father had an illness, which was no doubt typhoid. Dr. Cameron thinks milk may have been contaminated by actual particles of the dejections getting into it. "A strong wind blowing into the yard would certainly waft particles of coal-ash, and similar light substances from the dung-heap,	Houses supplied by this milkman were of the best class, and a large proportion were in two fashionable roads. In one of these roads there were 30 houses, supplied by different vendors. Of these, 12 were supplied from the particular dairy, and 6 were invaded by fever. In none of the other 20 did any cases occur. In the other road there were 32 inhabited houses; 9 supplied by particular dairy (4 invaded), and 28 by other vendors (none invaded). In the families using the particular milk that escaped, the milk was used in tea or cooked. In other roads the only houses invaded were those where this milk was supplied. Of 120 houses about which exact information was procured, 25 were supplied by the particular dairy, and 91 by 25 other vendors, 4 being supplied from cows kept by the families. In 14 out of the 25 houses supplied by the particular dairy, fever attacked 31 persons in the space of a fortnight, most of them in a week. In the 95 other	Details given of all the cases; they were all amongst drinkers of the milk. 44 of the cases presumably drank milk raw; there was positive evidence that 34 of these did so. 27 of the cases were children, 27 adult females, and only 8 adult males, of whom 2 at least were raw milk drinkers. A child in a family, who was the only one attacked in the house, was the only one who drank raw milk. Several servants drinking the milk unboiled were attacked. A medical stu-	*Dublin Journal of Medical Science*, July, 1879, p. 1.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk.	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
										rect into the yard, but the major portions of the dairy processes conducted in the yard or sheds. Milk utensils, or at least some of them, placed on shelves against the wall, where particles of solid matter could hardly escape getting into them during the removal of the manure, and especially on a windy day.	and distribute them throughout the yard. To these particles minute portions of human excreta might have adhered. The milk vessels, and the milk itself, might have had some of these particles deposited therein." Moreover, the family of the sick man were the persons who managed the dairy. They tended the patients and manipulated the milk. Trade and domestic operations were simultaneously conducted in premises of very limited size.	households, not a single case of fever or serious illness of any kind, occurred during Dec. or Jan.	dent, who drank much milk, especially at dinner, when he drank no other liquid, had a very severe and prolonged attack. A family using large quantities of this milk raw, the mother and one of the children "almost subsisting on it," were all taken ill.	
1878. November. 38.	Huncoat	T. Dean, M.D., M.O.H.	12	...	12	100	..	...	...	Nil.	A small farmer had three children suffering from typhoid fever. The children had no regular nurse, but had been attend-	All the cases got their sole supply of milk from infected farm. There were no cases of fever except where this milk was used.	Nil.	*Sanitary Record*, vol. iv. p. 362.

	Chester.	Dr. Hubert Airy.	50	6	...	...	59	26	43	Well of dairy sunk in soil, giving facility for soakage from cesspools into wells. Privy-pit only 8 yards off. No history, however, of any recent typhoid having occurred in the house or its neighbourhood.	tend to his cattle. Operation of milking performed in a milking-hovel near stream, which receives large quantities of filth. Milkman washed udders of cows with water from this stream, which probably at the time of the outbreak contained the poison of enteric fever. Act of milking apparently the only opportunity for specific contamination of the milk; and this contamination probably derived from what remained from the previous washing on the milker's hands, or the cow's teats being brought into contact with the milk.	Forty-three per cent. of the customers of the particular milkman had cases of typhoid fever, more or less distinct, during the epidemic. In the same locality there were 40 other families supplied by other dairymen, 9 of them being also part customers of the implicated dairy. Among these families there was no case of fever except in 5 of the 9 families partly supplied from the implicated dairy. Seven families supplied by the latter were at the end of the milkman's daily round, and more often than not his supply had run out by the time he reached them, and he had to purchase from another milkman to complete the amount required. None of these 7 families were invaded by the fever.	First case that of a student of abstemious habits drinking chiefly milk.	*Brit. Med. Jour.* vol. ii. 1879, p. 475.
ir.	Bristol	Dr. Davies, M.O.H.	Several.	...	...	...	...	...	...	Water from pump in the dairy stank abominably when pumped, and was described as "simply poisonous." Place totally unfit for a dairy.	Nil.	All cases were clearly traced to polluted milk supplied by a mixed retail dealer who got some of his milk from the suspected farm.	Nil.	*Brit. Med. Jour.* vol. ii. 1879, p. 625.

										heap.	front over washing-house, and stains on floor bore witness to the severity of her illness and its attendant risks. Excreta thrown over dung-heap. Soiled discharges from sick bed washed at dip-well. Suspicion that water from this well may have been used for scalding tins and other domestic purposes, pump of other well being somewhat out of order.	sociated with fever in dairyman's own household, in persons served direct from his own cart; it poisoned the families of the dealers through whose shops it passed to the public, and it poisoned the customers who dealt at those shops." (Milk supply stopped, and epidemic then died out.)		
1880. April. 43.	Possil-park, Glasgow	J. Christie, M.D.	92	...	90	97	252	56	22	See above (No. 42.) Dairyman supplied milk-shops in Possilpark also.	See above (No. 42) and Col. ii.	After very careful examination, no relation could be traced between drainage or water supply of district and the outbreak of fever. Epidemic began just at the same time as at Glasgow. Among persons receiving daily 172 quarts of cream and milk from implicated dairyman, 75 cases of fever occurred; whilst among persons receiving 532 quarts from other, or mixed sources, only 17 cases occurred. In only 2 of these 17 cases could the probable drinking of the infected milk be excluded.	Nil.	*Brit. Med. Jour.* vol. i. 1880, p. 864.
1880. July-September. 44.	Mill-brook, Corn-wall.	E. Ballard, M.D.	19	...	19	100	7	6	85	Part of a slaughter-house, not used as such, but as a wash-house, boarded off to constitute a dairy, on shelf of which milk was habitually	Six cases of enteric fever occurred in the butcher's family in less than three weeks; but the sale of milk was not stopped, even for a single day.	Six of the 7 families supplied from this source were invaded, and only one escaped. Other persons were occasionally supplied, and 3 families, at least, were invaded. (These cases formed part of a large outbreak in the village, caused, in addition to infected milk, by defective sewers and polluted water supply.)	Nil.	*Brit. Med. Jour.* vol. i. 1881, p. 20.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk.	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
										set in pans; in one corner of the slaughter-house, nearest the dairy, was a badly trapped and very offensive drain-inlet; and close to this inlet ran the wooden partition between the slaughter-house and the dairy, which near the inlet had been long broken away. The drain was in communication with another which had received infected excreta, so that the infected sewer air from the inlet had free access to the dairy and the milk which stood in it.				
1880. September.	Rochdale.	Joseph Henry, M.O.H.	55	9	26	74	...	...	...	Well at farm, used for all culinary purposes, is underneath the kitchen, and about fifteen	In a cottage between the farmhouse and shippen, a woman had been suffering from typhoid	Outbreak occurred suddenly, and ceased as suddenly. Of 35 cases of fever in a particular district, 25 were found to have had their milk from the suspected dairy. Cases occurred about a fortnight after the	In a family of 8 persons, 4 males who partook of the milk were all attacked, whilst all the females (who	*Brit. Med. Jour.* Vol. II. 1880. p. 597.

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										(Cattle walked about this cesspool, and may have carried infection to other parts of the farm. Difference of ten per cent. in milk at farm, and as supplied in town; so that milk was not improbably diluted with water from this well—which was also used for rinsing the milk cans.)			
188c September, October. 46.	Portsmouth (Cambridge Barracks).	Surg-Major Jameson.	7	...	100	...	...	...	No properly constructed dairy, but a back kitchen used for the purpose. No water-closet, but a privy at further end of a small garden, in a very offensive condition. Water-tap placed above the opening of a drain, which, leading to the privy, was carried on to the cow-shed.	Son of milkman had been suffering from fever with typhoid symptoms for some weeks. Patient lay in a room upstairs, and everything, such as stools, &c. had necessarily to be carried through the back kitchen used as a dairy. Infection may have reached the milk by absorption, through standing in same room as that through which stools were conveyed, or perhaps temporarily deposited. Or it may have been imparted by act	An officer's wife, 2 children and a nurse; and a woman and 2 children attacked. Sanitary condition of barracks good, and no defects discovered by careful examination. All cases served with same milk. (Milk supply discontinued to barracks, and no further cases occurred.)	In the officer's family all the members attacked were in the habit of taking a glass of milk before going to bed. The officer himself, and 2 children took milk in tea very sparingly, and escaped.	*Brit. Med. Jour.* vol. i. 1831, p. 61.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk.	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
											of milking being performed by attendant on the sick boy.			
1880. October. 47.	Bridlington.	J. Allison, M.O.H.	48 and others not stated.	8	48	100	83	45	54	Water used in operations of dairy drawn from a well 18 feet deep, sunk through a gravelly soil, in a low-lying, and, in wet weather, swampy under-drained field, where a downward percolation would readily take place. Well but a few feet from cow-shed, and from a huge pile of manure. Two privies with covered middens were only 13 yards distant. Analyst reported water as very bad, and "evidently largely contaminated by sewage."	The dairyman's son's wife had suffered from typhoid fever in August, and had been to her father-in-law's house after her recovery. These cases of fever occurred in the early part of October, in the house adjoining the dairyman's.	In 83 households supplied by particular dealer, there were cases of enteric fever, besides 7 doubtful cases. The outbreak was sudden and widespread, and attacked only the milk-walk of this vendor. When attention was called to the state of the dairy, the well was filled up, and company's water substituted. After this, the number of new cases of fever in fresh households rapidly declined.	In several houses it was stated that the persons attacked by fever were the only ones who had drunk milk from the suspected source, or that they had been especially fond of drinking milk, while other members of the family who consumed less milk escaped.	*Brit. Med. Jour.* vol. ii. 1880, p. 786.
1880. October. 48	Marylebone (Clifton Hill, &c.)	A. W. Blyth, M.O.H.	9	2 1	7 9	100	11	5	45	Nil.	Nil.	Cases all occurred about the same date. Invaded houses not on the	In a house where the father and 2	*Brit. Med. Jour.*

												vants and children mostly attacked.	
1880. October. 49.	South-port.	H.H.Vernon M.D., M.O.H.	32	2	32	100	...	...	...	Well in close proximity to premises, and much exposed to excremental pollution. Water declared on analysis to be nothing but liquid sewage.	Nil.	Houses invaded in good sanitary condition, no defects being recorded, except in 2 instances, and these very trifling. All cases occurred amongst customers of implicated dairy. (Milk supply stopped in 3 days, and fever was stayed at once).	Nil.	*Brit. Med. Jour.* vol. ii. 1880, pp. 820 and 934.
1880. October, Nov. 50.	Worth-ing.	C. Kelly, M.D., M.O.H.	44	8	44	100	...	...	...	**Shallow well** used for dairy purposes, especially for washing out of milk-cans, near a defective drain. Examination after closure of well showing distinct evidence of soakage from this drain into well.	A case of enteric fever in house to which defective drain belonged. Excreta from patient thrown down the drain.	Cases occurred in all parts of town, and in many houses where no sanitary defects existed. All the cases supplied by particular dairy. Evidence that milk became infective after it got into cans of dairyman, as other persons and milk-sellers supplied from farms whence supply was derived had no fever amongst their customers. (Pumps removed from well. Last case occurred 7 days after.)	Those who habitually drank considerable quantities of cold milk were the chief sufferers. Two teetotallers, drinking half-a-pint night and morning, were both attacked. A boy at school, ordered to have a pint daily, caught the disease. Those who only drank a small quantity in their tea or coffee, or partook of milk previously boiled, escaped. Thus, amongst the poor, who only bought a a very small quantity daily, disease hardly appeared.	*Brit. Med. Jour.* vol. ii. 1880, p. 934.

EPIDEMICS OF MILK SCARLATINA.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk.	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
1867. June. S. 1.	Penrith.	M.W.Taylor, M.D.	111	3?	11	100	14	6	42	Nil.	Scarlatina prevalent in a particular quarter of Penrith, and at first confined to that quarter. Among those earliest attacked was the child of a man who carried on a small business as a milk retailer. His cows were milked sometimes by himself, and sometimes by his wife, who nursed the sick child. The milk was brought direct from the byre to the dairy or back kitchen of the cottage.	Up to the time of the illness of the dairyman's child, scarlatina was epidemic only in the quarter of the town where the dairy was situated. But on a particular day the disease broke out in 3 of the houses supplied by the dairy, and later on attacked 3 others. families of young children, ceased to take the milk when it became apparent that infection was conveyed from the milk vendor's premises; 4 were tenanted by middle-aged and elderly persons without young families, and	At one house 2 young children were attacked; at a second, 5 out of 6 young children were attacked, and 3 died; at a third, a young servant girl, who was the only inmate that consumed milk; at a fourth, a young lady who was a great milk drinker, and subsequently other members of the family; at a fifth, a young child who used milk regularly at her meals; and, at a sixth, a young woman, who, though not in the habit of using milk, drank a mugful on a special occasion. Of the houses that escaped, 3, with	*Brit. Med. Jour.* vol. ii. 1870, p. 624.

											(see next column, 13) remarked to the servants, on hearing them talking of the illness there, that "he himself had sore throat, and peeling off of the skin," while carrying milk to the institution. Inquiry showed that the wife of the farmer had nursed her child whilst it was lying sick of scarlet fever, and milked the cows at the same time. A second child took scarlet fever a week or fortnight after the first; and, after a while, the milk-boy had sore throat, though not sufficient to confine him to bed, and in due course had desquamation of cuticle on hands and feet.	case occurred in parts of the town entirely separate from, and apparently in no way connected with, the institution cases. When the institution dispersed for the holidays, the milk was sent to 2 houses that were subsequently invaded by the disease. A servant who had been only a week in the town took the disease, and, next day, a nurse in another family of visitors, 5 members of which subsequently had it. A list of the houses in which scarlet fever appeared corresponded exactly (with but one exception) to the list of the farmer's customers. Every house, with this one exception, was invaded by fever. The exception on the side of the milk supply had reference to a house where 2 elderly ladies lived with no young people near; that on the side of the scarlet fever referred to a mild case of a child in a large family, none of whom took the fever.	that month from this farm, and during that time the younger members of the family took scarlet fever, though there was none known to be in the neighbourhood.	p. 409
1872. S. 3.		M. K. Robinson, M.D., M.O.H.	21	...	21	100	...	...	...	Nil.	Nil.	Twenty-one children in a particular locality attacked within a fortnight with scarlatina, when previous to, and during the time of their attack, they were supplied with milk from a house where several children were ill of scarlatina. In the houses where more than one case occurred, the sufferers in each	Nil.	"Annual Report of Medical Officer of Health for 1872."

												taken ill, a majority had not used cream in any form.		
1876. July. S. 5.	Hands-worth.	J. B. Welch, M.O.H.	37	4	37	100	Could not be ascertained.	20	...	Nil.	Scarlatina appeared in the family of the dairyman in the middle of June, when a servant lad living in the house became ill. This lad was kept in the house for 9 days after the disease appeared. At the date of the Medical Officer of Health's visit there were in the house 2 children suffering from the disease. The people were in the habit of taking the milk-cans into the house at night, and a portion at least of the milk sold was taken into the house. The Medical Officer saw in the dairy 4 tin-pans full of milk, evidently waiting for the cream to be removed, the dairy being in direct communication by a door-way with one of the living-rooms.	With two or three exceptions, the houses invaded were all of the higher class. The conditions of drainage, water-supply, and washing were different, but all the houses without exception were supplied with milk from this dairy. In one road, served by 4 dairies, 4 out of 5 houses supplied from this source had cases of scarlatina; the house that escaped containing no children. (In addition to cases of undoubted scarlatina, many cases of sore throat are said to have occurred in the infected district.)	All the cases but 5 were those of children.	*Brit. Med. Jour.* vol. ii. 1876, p. 225.
1877. April, May. S 6.	New Barnet.	C. E. Saunders, M.D., M.O.H.	140, and 12 of sore thrt.	...	131, and 12 of sore thrt.	95	135	58	43	Nil. (in report.)	Dr. Saunders says that the cause of the epidemic must be	The epidemic burst upon the district very suddenly; 128 cases occurring between April 29 and May 4. All but 9 of the 130 cases of	A lad drank the suspected milk, and was attacked, but a younger child in	*Sanitary Record*, vol. vii. 1877, p. 69.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
											regarded as an accident. He has "no conclusive facts to adduce as to how the poison of scarlet fever reached the milk;" but he has "certain facts warranting a very short belief on this point," which he has withheld from the public.	scarlet fever, and all the cases of "sore throat," occurred amongst the 135 families supplied by one dairyman, whilst 3 other dairymen, who supplied 286 houses, had only attacks in 9 of them.	the same family, who had other milk escaped. Three members of a family drank this milk, and all had fever; the rest of family drank "condensed" milk, and did not have fever. A typical case occurred in a person who had not been outside his house for a considerable time, but drank this milk. In a family consuming 3½ pints daily, there were 3 cases of scarlet fever; and in 3 families, each consuming a quart, there were 2, 3, and 4 cases respectively.	
1878. June. 8. 7.	High Ashurst and Headley	E. L. Jacob, M.O.H.	20 in 7 houses	2	...	...	...	...	...	Nil.	First case appeared in a mild form in the family of a cowman, some of whose children had lately had the disease in another part of the country, and had recently returned home. Cowman	Between June 1st and 7th, there were 15 cases in 3 distant houses, the inmates of which had had no communication with infected persons, but had all been supplied with milk from the farm where the cowman worked. Several families escaped who consumed only the scalded skim-milk from this dairy.	Nil.	*Brit. Med. Jour.* vol. i. 1880, p. 139.

											he did not himself have the fever, and the milk was not taken into his cottage.			
1879. May and June. S. 8.	West-gate, Newcastle-on-Tyne.	H. E. Armstrong, M.O.H.	23	10	...	...	350	14	4	Nil.	The water-supply of both farms, referred to in column 13, was found to be very polluted, and the drainage was bad. The arrangements for cleansing the milk-vessels were defective, and the tins sometimes smelt offensively, or were dirty. Moreover, these tins were not always washed by the customers before being returned to the dairyman, even though the house was infected. Several times during May and June the milk was complained of as having an unpleasant taste, and as not keeping well. But as to the manner in which the specific infection of scarlatina was introduced into the milk no evidence was forthcoming. A most minute and careful inquiry did not elicit the	In June, 1879, several remarkably sudden and rapidly fatal cases of malignant scarlatina occurred in a particular district, which had for some time been free from that disease. Several of the children attacked were struck down at or near the same time. In this particular area, 14 households were invaded by scarlatina during May and June, and 23 persons (10 of whom died) were attacked. Many of the invaded households were supplied directly with milk by the same dairyman, who kept a farm in the country, and others obtained their milk, through retailers, from the same source. These two farms (see col. 12) adjoined each other, and the two dairymen were in the regular habit of accommodating each other with milk. With one single exception, all the households attacked within the area derived their milk directly or indirectly from one or other of the dairies—all the households except 2 being supplied from one of the dairies.	Most, if not all, of the patients were in the habit of taking the milk in an uncooked state. The most rapidly fatal case was that of a child who fell ill soon after drinking freely of the milk. In another family, 3 children fed with the milk fell ill, and 1 of them died; the fourth child, supplied (for special reasons) with milk from a particular cow at a different dairy, escaped.	*Brit. Med. Jour.* vol. ii. 1880, p. 671.

										cer's own observations, that on several occasions it was so contaminated. Although there was no direct evidence of any contact of scarlatina poison with the finest milk, the conditions and surroundings of the dairy business at the two farms justify the conclusion that there was great risk of such an occurrence.			
1879. August. S. 9.	Fallowfield, near Manchester	H. Airy, M.D.	35 in 18 families.	0	35	100	60 or 70	18	27	Nil.	No reason for suspecting any wilful dilution of milk, or want of cleanliness in the vessels. Cows free from disease; but one of the milkers (an old man) was lodging in a house where his grandchild was lying in the full height of desquamation after scarlet fever. "Having regard to the confined space, imperfect ventilation, low standard of cleanliness, and want of sanitary intelligence, it is difficult to believe the old man could come from the house without	The 35 cases occurred within a period of a month, no fewer than 24 being attacked within a space of 36 hours. Every family attacked had received its supply of milk from the same dairy, while neighbouring families, taking milk from other dairies, escaped. The houses were, with only 3 exceptions, of the better class, and were widely distributed in the village. They had no community as regards social gatherings, nor attendance at places, nor employment of laundresses, nor receipt of letters or newspapers, nor drainage. The milk seems to have been specially infected on one day only, and the period of incubation in the majority of cases was only about 48 hours.	One child, who was fond of milk, had a single drink of the supply from this dairy, and was attacked by scarlatina. A lady who took a draught of milk for supper had violent vomiting and diarrhœa the same night.	*Brit. Med. Jour.* vol. i. 1880, p. 107.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk.	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
											carrying about him the taint of scarlatinal infection."			
1880. July and September. S. 10.	Paddington and Bayswater.	J. Stevenson, M.D., M.O.H.	84 families invaded.	...	...	...	...	42	...	Nil.	On 2 of 8 farms supplying a London dealer, "many cases of scarlet fever, both just past and then prevailing," were discovered. At one farm the families of 3 milkers had all had the disease, which displayed itself in some one or more members, and one child had died of it about 3 weeks before Dr. Stevenson's visit. On the second farm, where there were 5 milkers, the families of 3 had suffered.	There were 2 distinct outbreaks—one at the end of July, the other at the end of September. One medical man, who had not seen any cases of scarlatina for some months, was within 3 days in July called to 7 houses in which there were cases; in all these instances the milk was supplied from the same dairy. This was the experience also of other practitioners in the neighbourhood. Scattered cases occurred during August and the early part of September; and, at the end of the latter month, the second outbreak occurred. One practitioner was called in the last week of the month to cases in 13 houses, 11 of which were supplied with milk from the formerly implicated dairy; and another practitioner was called to 18 cases within 6 days, all supplied by the same dairy. The milk supply of 52 of the 84 families invaded with scarlatina was ascertained, and in 42 instances it was from this dairy.	Nil.	*Brit. Med. Jour.*, vol. ii. 1880, pp. 596 and 632.
1880 October. S. 11.	Dundee.	...	...	...	...	...	...	...	...	Nil.	Female servant employed at a dairy visited a house in which was scarlatina, and was attacked by disease, which	Scarlatina spread amongst customers supplied with milk from each dairy. Families attacked in good circumstances and with healthy surroundings.	Nil.	*Brit. Med. Jour.*, vol. ii. 1880. p. 790.

										cated to a person connected with another dairy.			
1880. November, and 1881, January. S. 12.	Ilkley, Yorkshire.	T. Scott, M.D., M.O.H.	10 November 9 February	...	10 8	100 88	...	...	...	Nil.	No case of scarlatina could be discovered on the 2 or 3 different farms from which the milk was derived in this neighbourhood. But milkmen at Ilkley customarily take their supplies in bulk into the houses of their customers, and the purveyor who furnished the milk to the 10 houses in which the fever suddenly developed itself, had also provided a family in which there had been a recent case of scarlatina. As the outbreak occurred in the end of November, and the children were seized a month previously, they would then be convalescent, and in the stage of desquamation, so that the milkman, following his usual custom of taking his milkcan into the house with him, had it most probably on some occasion infected from this source.	(November.) — The disease appeared suddenly in 10 houses, all of which were supplied by milk by one purveyor. (February.) — Of the 9 houses attacked, all but 1 were provided with milk by 2 individuals (one the same as before) who customarily interchanged and supplemented each other's supplies.	Nil.	*Brit. Med. Jour.* vol. i. 1881, p. 604.
1881. January. S. 13.	Bromley (Kent).	C. O. Baylis, M.D., M.O.H.	...	...	...	...	...	18	50	Nil.	Man employed at the farm supplying disinfected dairy with milk was engaged there while 4 of his family were suffering from scarlatina at home. He milked the cows, and attended generally upon them; and it was no doubt he who was the agent for the dissemination of poison in the milk.	Epidemic was very sudden in its onset, all the families being attacked in the course of a few days. All the infected families, exceptionally, had their milk from one milkman, who stated that the families in question constituted the larger half of his customers, and included the largest consumers. Nearly all the sufferers were in a good social position, and belonged to a different class from those who are actually most affected by infectious disease. Other circumstances conspired to make it very improbable that there could be anything common to the households invaded except their milk supply.	Nil.	*Brit. Med. Jour.* vol. i. 1881, p. 314.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk.	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
1881. January. S. 14.	Halifax.	D. Ainley, M.O.H.	510	86	...	...	135	53	39	Nil.	The farmer's man, who milked the cows and brought the milk to the customers in Halifax during the illness of the usual distributor, had 4 children ill of scarlet fever. Having 6 children (1 a baby) the mother could not attend to all her household duties and nurse the sick as well, and the probability therefore is that the man himself would do what nursing he could, and would put his clothes on the bed at night for extra covering, which is an almost invariable rule in persons of his class. Straight from his home, with his person and clothing alike infected, he went to milk the cows.	On the first 3 days of January, upwards of 50 cases commenced simultaneously in a certain part of the town. Neither the air, drains, nor water accounted for the circumstances of the outbreak. Of a total of 135 households supplied by the dairyman, 53 were invaded, 72 escaped infection, and in 10 the facts as to invasion were not ascertained. Of the 53 households invaded, 35 had scarlatinal cases, and 18 cases of sore throat; and amongst the 35 households in whi the regular scarlatinal eruption was recognized, sore throat occurred in other members of the family in 9 instances. Of the 72 families known not to have been invaded, 51, or 70 per cent., were more or less unlikely to receive invasion, either because there were no children, or because the family had been invaded on a former occasion: and of 14 other families, the children in them were more or less protected in at least three instances. All but one out of 51 households whose date of invasion was known, manifested the disease on or after December 15th, and all but 11 out of 51 on or after December 29th. The large majority of household invasions—viz., 39 out of 51—occurred during the eleven days between December 29th and January 8th—*i.e.*, from about two days from the time when scarlatina was recognized in the milkman's family, to the third day after, which was the last day on which the man was permitted to milk the cows, or to carry out milk to the customers. Further evidence as to the connection between the outbreak and the milk was furnished by a house-to-house inspection of thirteen streets, which were selected on account of some families in them being supplied with milk from this source, while others were supplied from other sources. In these thirteen streets were 272 families altogether, of which 82 were customers of the implicated dairy, and 188 customers of other dairies. Of the 82 families supplied by the former, 45 were found to have been invaded;	Nil.	*Brit. Med. Jour.* vol. i. 1881, p. 255 vol. ii. 1881, p. 485.

												of four of the thirteen streets, the particular dairyman had only one customer, while the customers of other dairyman numbered 64. Each of the four families supplied by the former were invaded—all before January 9th; whilst of the 64 other families, only two were invaded, and these invasions were as late as January 24th and 30th.	
1881. April. S. 15.	Keswick	J. Robertson, M.D., M.O.H.	?	?	?	...	30 to 40	...	...	A dairy closely adjoined a house where scarlet fever had existed for several weeks. The cows were milked, every night and morning, in an open tin can, carried across an open yard, past the affected house.	The children who first caught scarlet fever in the locality played about the yard whilst in a state of desquamation.	On one particular day a general epidemic of scarlet fever broke out in the town, between 30 and 40 families being invaded. All those suffering from the disease received their milk supply from this particular dairy-farm. Some member of every family supplied had either a scarlatinal sore throat or scarlet fever on this day. Other families supplied from a different source escaped the disease.	A lodger had the milk raw for supper and was attacked. His landlady boiled her milk the same night and escaped.

EPIDEMICS OF MILK-DIPHTHERIA.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk.	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
1877. June. D. 1.	Sutton, Surrey.	E. L. Jacob, M.O.H.	15, in 11 houses.	...	...	...	...	11	10 (about)	Nil.	Nothing could be learned as to how the milk became infective. The water supply of the farm was good, the cows healthy, and there had been no sickness amongst the dairyman or his servants' families.	In 3 days 15 persons in 11 households were attacked with diphtheritic sore throats. The houses were in all parts of the town, and 10 of the 11 were of considerable size. Nearly all had good sanitary arrangements, but all were supplied with milk from one dairy. There were 5 or 6 other milkmen supplying milk to the town.	Nil.	*Brit. Med. Jour.* vol. ii. 1879, p. 740.
1878. May. D. 2.	North London (Kilburn and St. John's Wood &c.).	W.H. Power.	264 households. in 118	38	...	...	573	74	13	Nil.	Nothing satisfactory could be ascertained on this point. No trace could be obtained of any opportunity for infective matters contained in water, in refuse on the premises, or in air, gaining access to the milk. It was most positively asserted that no diphtheria, or throat illness, had occurred during 1878 among families resident on the premises, or among persons	Middle-class families suffered most, and area of prevalence was circumscribed. In an inner circle of half a mile radius (including a population of some 15,000 persons, living in 2,700 houses) there were 230 cases in 98 houses supplied by the implicated dairy, and in an outer circle of a mile radius, 34 cases in 20 households. No less than 78 of the 118 invaded houses got their first attacks during the four weeks ending May 25, the number of newly invaded households amounting in the third week of May to 46. The fall was almost as sudden, and thenceforth the outbreak abruptly terminated. It could not be accounted for by direct infection, or by unwholesome conditions, or by sewer air, or in any other way. But a very large proportion of the cases occurred amongst the customers of milkmen obtaining their supply from a particular dealer, who had two businesses (one at Kilburn and another at Muswell Hill). Of 236 houses in the inner circle supplied by one of these milkmen from Kilburn, 37 were invaded, or 15 per cent.; of	124 (or 53 per cent.) out of 233 cases investigated were of children between the ages of 3 and 12 years.	*Brit. Med. Jour.* vol. i. 1879, pp. 48, 58. "Transactions of the Pathological Society of London for 1879."

											(or their families) employed among the cows in the dairy. At neither place had serious illness of cows occurred during the year. (But Mr. Power subsequently raised for consideration the question, whether a disease called "garget" in cows might not induce such changes in the milk as to give diphtheria to the human subject).	237 supplied by a second, from the same source, 31 were invaded, or 13 per cent.; whilst of the remaining 2,227 houses in the circle, 31 were attacked, or 1·3 per cent. (*i.e.*, a difference in the proportion of 10 to 1 between the two sets of customers). In the outer circle, the incidence of the invasions on customers of these and other milkmen was 12 to 1. The customers of these milkmen bore almost the whole brunt of the sudden outburst of disease that attracted attention. Similar evidence was forthcoming as to distribution of milk from Muswell Hill farm, though the attacks in this case were earlier than those amongst the drinkers of the Kilburn milk, the period of attack beginning, for one business only, about the time it had ceased for the other.		
1878. July. D. 3.	Weybridge, Surrey.	E. L. Jacob, M.O.H.	60	2	60	100	150	30	20	Nil.	Among those who suffered from the throat illness were the dairyman, his female servant, the wife of his cowman, and one of the milk-carriers, the illness of all but the last (who died) being extremely slight. But careful inquiry failed to elicit that any case of sore throat had occurred amongst those employed at the dairy, or in any member of their families, *prior* to the day on which the disease appeared suddenly in eight houses. The well water of the dairy was	On a particular day, 10 persons in 8 houses were attacked with diphtheritic sore throat, and fresh cases occurred daily in these and other houses for 9 days, when the epidemic suddenly stopped. During that time about 60 persons (nearly half of whom were adults) living in 30 houses, came under medical treatment for the disease. Those infected lived in all parts of the village, and principally in wholesome houses of the better class. They all had, however, their milk from the same dairy, whilst 350 families supplied with milk by 3 or 4 other dealers entirely escaped.	Eleven families had stipulated for a supply of milk every day from the same cow. To meet these requirements, the milk of 2 or 3 particular cows was set aside and distributed separately, under the name of "nursery milk." Of these 11 families, 9 (or 82 per cent.) were attacked with sore throat, while of the remaining 139 families who drank the rest of the milk from this dairy, 21 (or only 15 per cent.) were attacked. It would seem, therefore, that only this nursery milk had infective properties, and that the admixture	*Brit. Med. Jour.* vol. ii. 1879, p. 739.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk.	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease	Reference
											found to be much contaminated with sewage matters, but there was no suspicion that any of it had been added to the milk. In the yard where the milk-cans were cleansed and left to drain, was an untrapped inlet to the house-drain; and in the floor of the dairy was a gully leading to an unventilated drain. Nothing was apparently the matter with the cows.		of it with the general mass of the milk explained the illness of those who did not stipulate for nursery milk.	
1878. August and Sept. D 4.	Leatherhead.	E. L. Jacob, M.O.H.	55, in 36 houses	5	...	...	280	29	10	Nil.	As to how the milk became infected, nothing definite could be learned. None of those employed in the cowsheds or dairy had had any throat illness before or during the epidemic, and all the cows were, and had long been, in good health. The water in use at the dairy was derived from a soft water tank, and had at times smelt offensively; and the pipe from the sink, at which the milk cans were cleansed, was not properly trapped and disconnected.	In August, 1878, cases of diphtheria began to be observed with increasing frequency in the place, and fresh cases continued to occur till the latter part of September. The infected houses were situated in all parts of the village, on every variety of soil, and were chiefly of the better class. They had nothing in common except their milk supply, which was the same in nearly all the houses invaded. Up to the end of September, 30 families had suffered, and 29 of these were supplied by this dairy.	Nil.	*Brit. Med. Jour.* vol. ii. 1879, p. 740.

										to be impure. One of the cows which supplied the milk had lately been suffering from "garget."	reasonable explanation of the outbreak was that which ascribed it to the milk supply. The epidemic began rapidly to decline about 8 days after this supply had been discontinued, no other measures being simultaneously adopted which would account for its sudden subsidence at the time. Infants, who were the greatest milk consumers, escaped, but their milk was scalded before use.		
1879. September. D. 6.	Little Horton, Bradford.	H. Butterfield, M.O.H.	7	1	7	100	...	...	...	Milk cans dirty, and kept in a scullery in which was a sink with an untrapped pipe. Under this sink was a chamber utensil containing excreta. Near the milk was a wash-tub half-full of dirty water, and resting on the milk cans were two bundles of dirty linen from the bed and person of the infected child.	At the farm, a child had been ill with sore throat, commencing on August, 18. Another child was found ill in bed with unmistakeable diphtheria, having been taken ill on August 31.	Houses attacked not in a bad sanitary condition, and there had been no communication between the families, who were strangers to each other. But they all obtained their milk supply from this source. Four cases commenced in one day.	Nil.	*Brit. Med. Jour.* vol. i. 1880, p. 953.
1880. December. D. 7.	Surbiton.	O. Coleman, M.O.H.	42, in 34 houses.	8	...	...	...	26	...	Nil.	Means by which the milk became contaminated not clearly ascertained. Dairy in a satisfactory condition, and no evidence of infection by human agency.	One day 3 households were simultaneously invaded, and afterwards fresh cases appeared almost daily in different directions. Defects of sewers did not account for the outbreak. Houses attacked were mostly of the better class. Of the 34 families invaded, 26 were supplied with milk by the same dairy, and 6 by others; and 2 took no milk at all.	Nil.	*Brit. Med. Jour.* vol. i. 1881, p. 140.

THE ABERDEEN EPIDEMIC OF 1881.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	
Date of Outbreak.	Locality.	Reporter.	Total number of cases.	Deaths.	No. of cases amongst drinkers of suspected milk.	Percentage to total cases.	No. of families supplied by milkman.	No. of such families invaded.	Percentage.	Sanitary circumstances of farm or dairy from which milk was derived.	Exciting cause of outbreak.	Circumstances implicating milk.	Facts showing special incidence of disease.	Reference.
1881. April.	Aberdeen.		30	3	All.	100	100	80	80	A reformatory school near Aberdeen has attached to it an extensive dairy, where over 40 cows are kept. The water-supply of the dairy was unsatisfactory, all the water for the cows coming from a large and practically uncovered cistern, which was placed in the byre where the cows were kept. The water on analysis was shown to be bad. (Milk analyzed and found to contain new organisms, the nature of which has not yet been ascertained.)	No information.	About the beginning of April, without the slightest warning, the milk seems to have acquired poisonous properties. Over 300 persons were attacked with illness, and only 20 of the families supplied with the milk escaped. No case of the disease was found in any family not receiving its milk from this source. The symptoms of the attacks were severe rigors, which might last several hours or even for a day; this was followed by marked febrile symptoms. The tonsils were slightly enlarged, but there was no false membrane; and a marked feature was the swelling of the lymphatic glands of the neck, and those above the clavicle. The prostration was extreme. The febrile symptoms passed off in a few days, but in a great many cases there was a sudden relapse, and the relapse was often more severe than the primary attack. Sometimes 3 or 4 relapses occurred, and the patients were in all cases left extremely weak. In a few cases the attack was slight; but it proved fatal in 3 cases where the patients were old and frail, and unable to recover from the prostration.	Only a few of the officials of the reformatory were affected; while the boys seem almost entirely to have escaped. The boys got skimmed milk, whilst the officials were supplied with sweet milk. The supply of milk was ordered to be stopped, and the spread of the disease stopped almost as suddenly as it began.	*Brit. Med. Jour.* vol. i. 1881, pp. 657, 693, 729, 935.

L'Influence des Mesures Internationales pour maintenir la Bonne Qualité des Boissons et des Aliments.

Dr. Louis Grósz de Csatár, Buda-Pesth.

La question de l'alimentation publique a déjà été traitée dans les différents congrès scientifiques.

Le Congrès International d'Hygiène de Paris en 1878, s'est occupé très sérieusement de cette importante affaire; l'excellent rapport de Mrs. Bouchardat Boulay et Nocard a répandu de la lumière sur la nature de beaucoup de questions douteuses.

Le Congrès Médical de 1879 à Amsterdam continuait les travaux commencés par celui de Paris.

Deux faits de haute importance ont rendu témoignage, de ce que les délibérations des congrès scientifiques ne restaient pas dans les voies stériles de la théorie, et que les gouvernements prenaient au sérieux les avis des experts.

L'un de ces faits fut la confiscation et la destruction par ordre de gouvernement hongrois d'une quantité considérable de vins colorés artificiellement par la fuchsine; ces vins furent introduits de la Dalmatie, et on les trouvait chez différents marchands, qui voulaient les exporter.

L'autre fait qui engageait le controverse des différents points de vue est la défense de l'importation des viandes et des graisses de porc des Etats-Unis de l'Amérique.

Déjà à l'occasion du Congrès d'Amsterdam M. le Conseiller Docteur Finckelnburg de Berlin nous a fait la communication qu'une dixième partie des jambons importés et soigneusement visités à Hamburg contenait des trichines.

Nous avons fait la même expérience en Hongrie; une partie de la graisse importée des Etats-Unis de l'Amérique dans des tonneaux, avait la consistence molle presque liquide oléeuse et répandait une odeur désagréable; le mode de préparation de ces graisses obtenues par compression à froid et non pas par la fusion, n'est pas conforme aux exigences de l'bygiène, car les hauts degrès de température appliqués pendant la fusion sont seuls capables de détruire les germes des parasites et des contagions.

C'est par ces raisons, que les gouvernmements de la France et de l'Autriche-Hongrie ont décrété la défense de l'importation des marchandises mentionnées.

Il faut trouver un moyen d'éviter l'enchérissement inutile des victuailles, aussi bien que les attaques contre la santé publique causées par la fraude.

C'est uniquement la santé qui doit être protégée par l'application bienfaisante des principes de l'hygiène qui n'ont rien à faire avec ceux de la politique commerciale et de l'économie nationale.

La science ne devrait jamais fournir d'armes pour les guerres de douane et de concurrence des différentes nations.

Pour atteindre les buts signalés il est nécessaire:

1° D'établir des conventions internationales pour le commerce des aliments et des boissons.

2° Constituer en même temps un contrôle mutuel et continuel pour surveiller le maintien des dites conventions.

Si les gouvernements des différents Etats européens et transatlantiques pouvaient constituer des unions postales, télégraphiques, monétaires, etc., pourquoi ne pourraient-ils pas s'unir relativement aux questions hygiéniques?

Une convention hygiénique serait autant dans l'intérêt de la santé publique, que dans celui du bien-être des nations.

Il va sans dire, que le premier postulat d'une telle convention serait l'établissement de lois égales relativement aux questions de l'hygiène.

Par conséquent j'ai l'honneur de proposer à la Section honorée l'acceptation des conclusions suivantes :

1. Tous les Etats doivent garantir mutuellement par des lois égales la bonne qualité des aliments et boissons exportés.

2. Les traités conclus ne pourront jamais contenir des mesures en contradiction avec les exigences de l'hygiène publique.

3. La question d'alimentation publique doit être considérée internationale, par conséquent toute difficulté doit cesser dès qu'il ne s'agit pas de la défense du point de vue de l'hygiène publique.

4. La science ne devrait jamais servir de subterfuge aux intérêts politiques et commerciaux.

5. Il faut établir des lois semblables et rigoureuses contre les fraudes et les falsifications.

La Chasse aux Trichines.

Dr. Guilherme José Ennes, Lisbonne.

Parmi les affections déterminées, dans l'espèce humaine, par des implantations phitobiotiques, et par le passage, à travers notre économie, d'êtres vivants qui y séjournent, qui y existent, et qui se régénèrent au sein de notre organisme, compromettant la vie et la santé de ceux où ils s'introduisent, il y a la trichinose, qui inquiète beaucoup, depuis quelque temps, et les médecins et ceux qui, par leur devoir et leur position officielle, décrètent les mesures d'hygiène publique. Depuis quelques années, l'Amérique rend les marchés de l'Europe pleins d'énormes quantités de viandes de porc, et le copieux emploi alimentaire de ces produits, frais ou conservés, devient dangereux, car ces substances peuvent être contaminées par la trichine, qui se transmet à l'homme et à quelques animaux. La trichine est un petit ver d'une grande ténacité vitale, résistant à la putréfaction de la viande et à un froid de plus de dix degrés au-dessous de zéro, dont la femelle peut pondre jusqu'à dix mille embryons, et qui ne meurt, d'après les expérimentateurs, qu'à la température de cent degrés.

Et rien n'est plus facile à croire du moment que la trichinose peut être confondue avec la fièvre typhoïde, le rheumatisme musculaire, ou certaines formes d'empoisonnement par des produits alimentaires altérés. Ces accidents, dont l'étiologie est l'ingestion, et l'émigration de la trichine, ont été fréquents en Allemagne depuis quelques années; en Espagne, en France, en Algérie, et aux Etats-Unis, où on a positivement constaté des cas; et au Portugal, on a maintenant acquis la certitude que des porcs infectés de trichines ont dernièrement traversé la frontière.

Et, signalons encore que cette importation de viandes américaines va toujours en progression! Devant un si redoutable danger, nous demanderons s'il faut décréter la prohibition radicale des viandes, fraiches ou conservées, importées des localités où apparaît la trichine, comme on a fait en Espagne, en Italie, en Prusse, en Hongrie, en Autriche, et même dans d'autres pays? Ce serait une bien triste mesure d'hygiène publique, parce que du moment où il y a des viandes contaminées par ce parasite, on ne saurait certifier qu'elles le sont toutes; et encore parce qu'on nuirait ainsi au commerce international et à l'alimentation des gens pauvres. Il y a, en Europe, deux écoles, vraiment caractérisées: celle qui prêche la prohibition absolue des viandes provenant d'un pays soupçonné d'avoir la trichine, les vrais terroristes; et l'école de la

fanfaronnade, qui méprise toute espèce de danger, et qui se moque de toute cette alarme. Mais, le sage juste milieu doit être l'investigation du fait et la surveillance sévère et continuelle :—

En résumé, nous adoptons sur ce grave sujet les conclusions suivantes :

1.—La prohibition radicale de toutes viandes de porc, fraîches ou conservées, provenant d'un pays où règne la trichine, nous paraît une exagération fatale au libre commerce international et à l'alimentation publique, une grave erreur médicale, et une lourde privation pour les classes pauvres.

2.—Au lieu d'une prohibition absolue, c'est la surveillance active et incessante, et la révision des viandes de porc, exposées en vente, qu'il faut déterminer.

3.—Comprise à la façon d'une affection gastro-intestinale aiguë, produite par les parasites ingérés, la trichinose peut guérir ; à l'état de généralisation dans l'intérieur des muscles, le fait de la guérison demande à être confirmé.

4.—Un microscope donnant le grossisement de cent diamètres est le simple moyen logique pour résoudre l'investigation du sujet et nous mettre à l'abri des dangers de la consommation de la viande de porc.

5.—Les viandes reconnues trichinées doivent être portées à l'équarissage ; et les porcs, vivants ou morts, reconnus trichinés doivent être détruits par la crémation.

6.—Une cuisson complète et la température très-élevée, sont les sûres garanties contre l'émigration ou l'introduction du parasite dans les tissus.

7.—Les cas mortels proviennent toujours de l'ingestion de la viande crue.

DISCUSSION.

Dr. GÜNTHER, Dresden : I am unable to agree with the fifth conclusion put forward by Dr. Ennes. Since the discoveries of Zenke and others have shown how important a *rôle* the rats play in the extension of trichinosis, and in how greatly an increased amount trichinosis has been met with in the rats of knackers' shops, it is our duty to keep trichinosed flesh as far as possible from the knackers. From an economical standpoint also we are not justified in burning the flesh of living or dead swine in which trichinæ are found ; it should rather be placed in a concentrated solution of sulphurous acid and boiled, so that the fat may be employed for technical purposes, while the flesh may be used as manure.

Sur la Falsification et l'Inspection des Substances Alimentaires.

M. WALTER DOUGLAS HOGG, Paris.

. . . . Permettez-nous de vous entretenir de l'organisation française sur cet objet, des avantages de cette organisation, de ses lacunes, enfin des améliorations qu'elle appelle impérieusement, et qui consistent, selon nous, en l'établissement sur des données toutes nouvelles de l'inspection des substances alimentaires.

Il ne faut pas s'y tromper ; en effet, Messieurs, cette inspection, c'est la clef même de tout l'édifice. Est-elle sérieusement, pratiquement organisée ? Vous avez les mains pleines de résultats. Au contraire, n'a-t-elle que des apparences d'organisation ? Ne repose-t-elle que sur des intentions ? fussent-elles excellentes ? l'institution reste stérile, les meilleures lois demeurent à l'état de lettre morte. Qu'importe, en effet, les commissions d'hygiène les mieux composées ? Qu'importent les laboratoires les plus complètement, les plus savamment outillés ? Qu'importe, en un mot, l'arme contre la fraude, si l'ennemi peut se tenir constamment en dehors de la portée effective de cette arme ? . . . .

Si l'inspection fait activement et conciensieusement son métier, si ces opérations sont dirigées et contrôlées par une administration spéciale, autonome, mais responsable dans une mesure donnée de tous les gardiens autorisés de la santé publique, nous entendons les conseils d'hygiène, nous le répétons, la moisson sera inévitablement fructueuse.

Si, au contraire, l'inspecteur agit sur un champ trop vaste, si l'ennemi peut glisser facilement entre ses mains, si le fraudeur n'est pas perpétuellement sous le coup d'une visite et d'une saisie de ces produits, ces laboratoires en seront réduits au simple rôle d'épouvantails sans conséquence. . . . .

En France, comme ailleurs, on a commis l'erreur qui consiste à faire d'excellentes lois contre la fraude, mais en négligeant presque absolument d'organiser des services destinés à la découverte de la falsification.

De telle sorte qu'aujourd'hui le problème ne consiste pas à forger de nouvelles armes, on en possède, et d'excellentes. Ce qui reste à trouver c'est le moyen de les utiliser.

Eh bien ! Messieurs, ce moyen est tout entier dans une organisation pratique et sérieuse de l'inspection des substances alimentaires, et il n'est que là, dans cette voie que nous avons été devancés par nos honorables hôtes.

En effet, l'originalité des mesures inaugurées par l'Angleterre n'a pas consisté dans l'application au genre de délit dont nous nous occupons, de peines exceptionellement sévères, mais bien plutôt dans la reconnaissance et la pratique de cet axiome, qu'une des conditions essentielles pour réprimer la fraude, c'est d'abord de la découvrir et de la constater. L'expérience a fini en France par faire admettre cette vérité. Tous les efforts des hommes clairvoyants tendent aujourd'hui à combler les lacunes qui existent dans l'organisation de l'inspection des substances alimentaires. . . . . Inspiré de cet esprit, un projet de loi, sur lequel nous reviendrons tout-à-l'heure, sera déposé à la Chambre des Députés, dès que nous aurons des Députés.

Quel sera le sort de ce projet, nous le saurons plus tard ; en attendant Messieurs, nous abandonnerons ces promesses de l'avenir, ces previsions de ce qui sera ou devrait être, pour jeter un coup d'œil aussi rapide que possible sur notre régime actuel.

En France toute personne qui veut faire vérifier un aliment en dépose un échantillon au bureau du laboratoire Municipal de Paris. Les formalités de depôt sont fort simples. Le déposant signe un livre à touche et l'étiquette de l'échantillon qu'il remet. Il indique le nom, l'adresse du vendeur et le prix d'achat. On lui fixe une date à laquelle il devra venir chercher le résultat de l'analyse. Elle est gratuite s'il s'agit simplement de savoir si l'aliment est bon, mauvais, ou falsifié ; au contraire, une analyse qualitative et quantitative est payée selon un tarif dressé d'avance, et dans certains cas le prix varie selon la difficulté de l'opération.

Si le produit est reconnu mauvais, les inspecteurs vont faire une saisie chez le commerçant et dressent procès-verbal. Le parquet est informé et l'affaire suit son cours régulier.

Ajoutons que le Laboratoire Municipal de Paris est un modèle du genre, pourvu de tous les appareils nécessaires à des travaux de chimie analytique, et installé avec les derniers perfectionnements de la science moderne.

Sur les 2,291 échantillons analysés pendant le premier semestre de cette année 1881, ont été reconnus—

Bons	21·52	pour cent.
Passables	19·20	,,
Mauvais	59·28	,,
	100·00	

Cette proportion énorme provient, il faut le dire, de l'envoi d'échantillons de toutes les parties de la France, échantillons sur lesquels on a déjà conçu certains soupçons. Il s'opère donc comme une espèce de triage préalable et il serait tout-à-fait inexact de prétendre qu'en France près de soixante produits sur cent. sont falsifiés : on ne connaîtra la proportion exacte que lorsque les prélèvements se feront au hasard sur toutes les denrées exposées en vente et non pas seulement, comme cela est arrivé jusqu'à présent, sur celles dont la pureté avait déja été reconnue plus que douteuse. . . . .

Le succès qui a couronné les premiers efforts tentés à Paris sont d'un heureux présage pour l'avenir. Nul doute que ces tentatives, restreintes jusqu'à présent à certaines grandes villes, ne prennent bientôt un caractère plus général, et qu'une loi étendant ces excellentes mesures à toute la France, n'élève des barrières infranchissables devant les terribles et envahissants progrès de la falsification. . . . .

Voici Messieurs le texte littéral du projet de loi tendant à organiser le service d'inspection de substances alimentaires et qui a pour but de résoudre ce deuxième terme si difficile dont nous venons de parler, en prêtant aux laboratoires d'analyse un concours sans lequel ces établissements ne produiraient que des résultats incomplets.

PROJET DE LOI TENDANT A ORGANISER L'INSPECTION DES SUBSTANCES ALIMENTAIRES.

Art. I.—L'inspection des boissons et des substances alimentaires de toute nature est confiée à un service spécial relevant du Ministre de l'Agriculture et du Commerce.

Art. II.—Ce service comprend des chimistes experts (analystes) et des inspecteurs dont le nombre sera déterminé ultérieurement.

Art. III.—Les inspecteurs seront chargés d'opérer des prélèvements chez tous les débitants de boissons et de substances alimentaires, de constater les contraventions prévues par les lois, décrèts et ordonnances sur la vente des boissons et des substances alimentaires.

Art. IV.—Leurs procès-verbaux feront foi en justice jusqu'à preuve du contraire. Ils prêteront serment devant le tribunal d'arrondissement de leur domicile.

Art. V.—Les débitants ou leurs représentants seront tenus de leur livrer à première réquisition, et séance tenante, des échantillons de leurs marchandises exposées en vente ou vendues, sous peine, en cas de refus, d'une amende de 500 francs et de 1000 francs quand il y aura récidive.

Art. VI.—Sur ces échantillons il sera fait sans désemparer des prélèvements en quantité suffisante.

Ces prélèvements seront immédiatement divisés en deux parts scellées et cachetées en présence du débitant ou de son représentant. Une de ces parts restera entre les mains du vendeur pour servir à une contre-expertise, s'il y a lieu ; l'autre sera conservée par l'inspecteur pour être ultérieurement soumise à l'analyse du chimiste-expert (analyste).

Art. VII.—Il sera payé au débitant, par l'inspecteur, la valeur, au prix courant, du ou des prélèvements effectués.

Art. VIII.—Les chimistes-experts (analystes), assermentés commes les inspecteurs et nommés comme eux par le Ministre de l'Agriculture et du Commerce, seront chargés d'analyser les prélèvements effectués par les inspecteurs.

Leurs rapports feront foi en justice jusqu'à preuve du contraire.

Art. IX.—Une fois par an et plus souvent s'il y a lieu, le Comité consultatif d'hygiène publique de France, après avoir reçu les avis et les vœux du conseil central d'hygiène de chaque département sur les questions d'hygiène alimentaire et le mode d'exécution de la présente loi, notifiera au ministre sous forme de proposition, ceux de ces avis et de ces vœux qui lui paraîtront devoir être pris en considération. Le ministre fera connaître au Comité consultatif la suite qui aura été donnée à ces propositions.

Art. X.—Un réglement d'administration publique déterminera les diverses dispositions

à prendre pour l'éxécution de la présente loi et fixera les taxes à percevoir sur les commerçants et les débitants soumis à l'inspection.

Art. XI.—Sont modifiées en ce qu'elles ont de contraire à la présente loi, les lois sur les attributions de la police municipale, notamment la loi des 16–24 août 1790.

Nous le répétons, Messieurs, ce qui précède, consacre les principes dont il est impossible de s'écarter, si l'on veut arriver à une répression efficace et pratique.

On remarquera que les intérêts de l'acheteur et du vendeur sont également sauvegardés. Ce dernier ne se trouve en aucune façon lésé puisque les prélèvements des inspecteurs lui sont remboursés; il a, au contraire, tout avantage à ne recevoir de ses fournisseurs en gros que des marchandises pures. Si la bonne foi a été surprise, une action reconventionnelle suspend le jugement et la contre-expertise lui procure la facilité de prouver son innocence s'il se croit injustement poursuivi. Aucun commerçant honnête ne s'insurgera contre une si bonne loi.

L'unité de direction consacrée par l'article I. est indispensable à la marche régulière des affaires; d'où la nécessité de centraliser le service d'inspection entre les mains d'un seul pouvoir. . . . .

Tel est, Messieurs, le projet actuellement à l'étude en France.

Nous aurions voulu, avant de conclure, entrer dans quelques détails au sujet de l'influence, exercée par l'adultération des aliments sur l'étiologie de certains états pathologiques, notamment sur la production de quelques maladies de peau, dues à l'ingestion de la margarine, d'affections causées par la fuchsine, l'acide salicylique, etc.

Mais cela nous eut entraîné trop loin, et nous avons estimé que devant une assemblée dont les instants sont aussi précieux, il valait mieux envisager la question des falsifications dans son ensemble, à un point de vue général, pour qu'elle présentât un caractère d'intérêt universel.

Cette pensée nous amène à formuler les trois propositions suivantes auxquelles nous vous prions, Messieurs de vouloir bien vous associer, et sur lesquelles nous prenons la liberté d'attirer l'attention des gouvernements soucieux de veiller au maintien de la probité commerciale et à l'exécution des mesures sanitaires concernant la vente des aliments et des boissons.

Considérant—

Que la santé publique est de jour en jour plus gravement compromise par le nombre et la nature des substances alimentaires falsifiées;

Que ce déplorable état de choses provient de l'insuffisance de la législation touchant, l'inspection des denrées, et l'inefficacité des mesures prises pour assurer, par une sanction inévitable, l'exécution des lois existantes sur ces matières.

Nous émettons le vœu:

1° Que les pouvoirs édictent des lois dans le but de punir sévèrement la falsification des substances alimentaires;

2° Qu'ils créent un service d'inspection sous la dépendance d'une direction centrale, spécialement chargée de constater les contraventions prévues par les lois, décèrts et ordonnances sur la vente des substances alimentaires;

3° Qu'ils installent de nombreux laboratoires d'analyse destinés à vérifier la pureté et la qualité des prélèvements effectués par les inspecteurs.

Influence of Food on Health and Disease among the Working Classes in our Colonies.

Dr. GAVIN MILROY, London.

The object of the paper was to draw public attention to a subject which much concerns the true welfare of our Colonies, in an economic and political as well as in a sanitary and medical point of view.

No community can be properly said to be in a really sound or thinking state, if the physical powers of its labouring classes are at a low standard of vital energy, and if there be always an excessive amount of mortality from such diseases as are plainly indicative of marked debility and deterioration of healthy existence.

The evidence on which the illustrative *data* adduced in proof of the several statements in the paper are founded, has been derived from direct personal observation in the case of the two West Indian Colonies, and from semi-official communications in the case of the two Colonies in the Pacific.

DISCUSSION.

Dr. E. DEFFERNEZ, Jumet: expressed his entire concurrence in what had fallen from Dr. Grósz de Csatár as to the means of dealing with imported food, and referred to experiments at Charleroi, from which it might be hoped that trichinosis may be dealt with, as had been the case with the phylloxera. As regarded the salt meats imported from America, he pointed out that, according to Captain Clipperton, Consul at Philadelphia, over 1,000,000 pigs died of cholera in 1879, and nearly 800,000 in 1880; and he deemed it all but certain that a considerable amount of this meat had been sent over to Europe for consumption. Without pretending to confirm the view that pig-cholera was related to trichinosis, still it might be referred to in connection with a subject which was of such importance—namely, the prohibition of the importation of pork from America. The necessity of a much more complete microscopic examination of these foods was also necessary.

VI.—ON THE INTERNATIONAL CONDITIONS OF ADMISSIBILITY TO PRACTISE.

Dr. ACLAND, F.R.S., Oxford.

When I accepted the duty, imposed upon me by the distinguished President of the Section, of reading a paper "On the International Conditions of Admissibility to Practise," I felt that the subject was one which, in some respects, belongs rather to Parliaments and Ministers of State than to ourselves. There are, however, valid and manifest reasons why the medical profession should clearly express its views on this important question. No time or occasion could be found more fitting than the present.

And, further, there are reasons, not immediately obvious, which makes this subject one of some consequence to the world at the present period of civilization and of international intercourse. They are chiefly two. The one, that it is closely connected with the principle of reciprocity—a principle just now keenly debated among statesmen of all countries. The other, that reciprocity of licence to practise between two nations cannot be properly urged by anyone who is not prepared to lay down what are the essentials as regards medical education and examination, which justify a civilized country in granting privileges to its citizens in the form of diplomas, or titles, or protection. On this last point I would observe that, in this country at all events, we are in a state of

transition. It is certain that, as regards Great Britain, the conditions under which diplomas conferring State privileges can be obtained will, in some important respects, be altered.

Indeed, we may go further, and say that, but for the confusion which has been cast, under the forms of our constitutional government, into legislative discussion on this matter by the introduction of irrelevant topics, the question of international medical rights, as far as Great Britain is concerned, would have been settled by Act of Parliament more than ten years ago.

As the *history* of these discussions is long and complicated, and as it does not really affect the more serious duty before us—viz., to say what ought to be the conditions for international practice, and not how qualifications can be obtained in each country, which is the duty of local politicians—I will set the latter part of the subject aside, and put to you four distinct questions :—

1.—Are there conditions under which one civilized country should give to the holders of foreign diplomas privileges equal or analogous to those conferred on possessors of her own?

2.—Should these conditions necessarily imply re-examination by each country?

3.—Should the conditions between foreign countries be the same as between a mother country and her colonies?

4.—Should the conditions be reciprocal, or can a country properly allow freedom which is not accorded to her practitioners abroad?

As to the first, my answer is, Yes. There are conditions under which, in the present state of medical science and practice, one country should recognize the legal practitioners of sister countries.

As to the second, No. These conditions should not necessarily imply re-examination.

As to the third, the answer is, the conditions may or may not be identical, according to the voluntary understanding and relations between the countries respectively.

As to the fourth, the conditions need not be reciprocal.

The conditions which now exist in different nations vary considerably. In England, the principle of the law is that a Medical Register should be maintained, according to rules laid down by Act of Parliament, of the persons who are held to be qualified to practise—the object of the Register being to distinguish qualified from unqualified practitioners. Persons who are not on the Register may be employed at the risk of the employer; but it is an offence in the eye of the law for a person to pretend to be registered who is not so. Persons not on the Register cannot recover their fees in a court of law; cannot sign a medical certificate required by law; cannot hold any appointment in the public service, in a public hospital, workhouses, or in respect of any friendly society; and cannot act as medical officer of health. The medical licences of foreign countries are not at present admissible to our Register, and therefore all foreign physicians and surgeons settling in England without obtaining the licence of some licensing body in this country practise without the privileges of English practitioners.

In France, no one is allowed to practise on a foreign qualification unless he receives special authority from the Government. This authority has been, as a matter of fact, granted to persons who show sufficient grounds for obtaining it; but I am informed that this is no longer the case, and that very proper persons desirous to practice among English citizens living in France have lately been refused privilege. In France, therefore, the law has been more lenient towards foreign practitioners than in England. It has been proposed in France to deprive the Government of even the power of granting this privilege, and to make it compulsory on every foreign practitioner, of whatever age or standing, to pass an examination before an examining board in France. It has been

long felt in England that the English law required amendment in the direction of granting to persons possessing adequate foreign qualifications the same privileges as belong to our British registered practitioners. The subject was referred to the Medical Council, and was anxiously considered by it. Accordingly, the following clauses actually passed the House of Lords in 1879 :—

"Where a person shows that he has obtained some recognized medical diploma or diplomas (as hereinafter defined) granted in a foreign country, and that he is of good character, and either that he is not a British subject, or that if a British subject he has practised medicine or surgery, or a branch of medicine or surgery, for more than ten years elsewhere than in the United Kingdom (or if he was practising in the United Kingdom at the time of the passing of this Act, for not less than ten years either in the United Kingdom or elsewhere), and either continues to hold such diploma or diplomas, or has not been deprived thereof for any cause which disqualifies him from being registered under this Act, such person shall, upon payment of the registration fee, be entitled, without examination in the United Kingdom, to be registered as a foreign practitioner in the Medical Register. (Clause 8.)

"The medical diploma or diplomas granted in a British possession or in a foreign country, which are to be deemed such recognized medical diploma or diplomas as are required for the purposes of this Act, shall be such medical diploma or diplomas as may be recognized for the time being by the General Medical Council as furnishing a sufficient guarantee of the possession of the requisite knowledge and skill for the efficient practice of both medicine and surgery, including therein midwifery, and as entitling the holder thereof to practice medicine and surgery, including therein midwifery, in such British possession or foreign country.

"If a person is refused registration as a colonial practitioner or as a foreign practitioner, the Medical Registrar shall, if required by him, state in writing the reason for such refusal; and if such reason be that the medical diploma or diplomas held or obtained by such person is or are not such recognized medical diploma or diplomas as above defined, such person may appeal to the Privy Council, and the Privy Council, after communication with the General Medical Council, may dismiss the appeal or may order the General Medical Council to recognize such medical diploma or diplomas, or any of them, and such order shall be duly obeyed." (Clause 9.)

By these Clauses it will be observed that the Medical Council is required to recognize such medical diplomas as furnish a sufficient guarantee of adequate professional knowledge, and thereupon to grant to such foreign licentiates the same privileges as are accorded to the English. The Medical Council may find it difficult to accept or to refuse the licences of this or the other country, and may find itself exposed to a reversal of its judgment of refusal by the Government. But such responsibility the Council has declared itself prepared to take. And here I may be permitted to observe that, as far as the medical profession is concerned, no other principle can be honourably maintained. We do not in England ask for reciprocity in this matter. We say that science and knowledge, as applied to the relief of suffering and the bettering of "man's estate," are the common property of mankind. The exercise of skill gained by men possessing such knowledge is not to be restricted on any personal grounds, and a restrictive policy on the part of another country, which we do not approve, should not restrain us from offering to that country the privileges we do approve.

I have taken the case of France, because there has been considerable and friendly discussion between the Governments of France and England in this matter, and because the laws of France, as they affect medical men, are simple and precise. If we turn, however, to the United States, we find that the regulations of the several States con-

fessedly vary so much that no single or common rule could be adopted as regards that great country which speaks with us a common language, and uses substantially the same literature. Under the proposed Clause the Medical Council might be prepared to accept the diplomas—let me say, for instance—of Harvard University, and to watch with care the effect which such recognition might have within the United States upon other diploma-granting bodies in States other than Massachusetts, and which Harvard University would at present not consent to admit as equivalent to her own. I hope before long to obtain all existing regulations of the several countries, and to put them in a tabular form.

The above brief observations illustrate the first and second questions, and lay down that re-examination need not be counted necessary in respect of the minimum of professional knowledge. As regards personal character, the question must be handled in another way. The relation between colonial and home licences should perhaps hardly be discussed in full on this occasion. The Medical Council has had many communications on this subject. It is of opinion that the relations in question, as regards the British empire, urgently call for adjustment. At present, degrees granted in one British colony do not confer privileges in other parts of the empire; whereas licences in the United Kingdom have legal recognition in the colonies. It may be generally stated, in the words of a committee of which Mr. Simon was chairman, "that qualifications under legal authority in any part of Her Majesty's dominion ought to be regarded as presumptively entitled to legal recognition in the mother country."

It has been suggested that the examinations in the colonies cannot be under the same inspection as they are in Great Britain; but, in fact, if there were a desire for solidarity in this particular, so rapid and constant is the intercourse, and so intimate the relations between scientific men of all parts of the empire, that there could be no difficulty in obtaining the most trustworthy and accurate information as often as required. Accordingly, the Medical Council agreed to the following Clause, which passed the House of Lords in 1879:—

"Where a person shows that he holds some recognized medical diploma or diplomas (as hereinafter defined) granted to him in a British possession, and that he is of good character, and either that the grant of such diploma or diplomas occurred when he was not domiciled in the United Kingdom, or in the course of a period of not less than five years, during which he resided out of the United Kingdom (or if he was practising in the United Kingdom at the time of the passing of this Act, that he has practised medicine or surgery, or a branch of medicine or surgery, for not less than ten years, either in the United Kingdom or elsewhere), such person shall, upon payment of the registration fee, be entitled, without examination in the United Kingdom, to be registered as a colonial practitioner in the Medical Register." (Clause 7.)

There are various details with which I would not burden this communication, and for some of them I would refer to the published *Minutes* of the Medical Council. Many of the details are to be found in a separate pamphlet, published by the Medical Council entitled, "Resolutions and Papers relating to the Amendment of the Medical Acts."

I am well aware that there is another view which may be taken of the course proper to be adopted by each Government for the people committed to its charge, and that, in that view, every country is bound to hold in its hands a strict administration of its local police. This view is wholly foreign to the temper which lies at the root of the present Congress. It is our object to enlarge the boundaries of knowledge; to break down any barriers that restrain it; to make attained truth, whether in science or in practice, available to all of every country, race, or creed. Here, if ever, *salus populi, suprema lex.*

It might be well to discuss in detail what should be the nature and extent of general,

scientific, and practical training and examination which each country ought in the present day to require of her practitioners, in order to receive the approbation of good men, and the recognition of law in other countries. Professional study for seven years, six, four, three, two, eighteen months—the limits of the general, the scientific, and practical curricula—the relative value of principles, facts, practical skill—the length as well as the extent of the examinations—the necessity or otherwise of the German Bedside Examinations, so prolonged, so searching, so complete. But it is clear that our time forbids this. The subject belongs rather to a commission or a committee than to a large meeting. But we know only too well how much the minds of examiners or teachers of students are exercised herein; and we know that there is a risk of memories being overtaxed, and of the powers of reflection not being evoked. I speak, be it observed, of the minimum qualification which might be asked for, and which can be obtained. The history of great men teaches that freedom in study was to many a condition of growth, and that a uniform absolute rule would have stunted that growth. In any scheme of international agreement as to the fitness of one or another qualification to be recognized, a liberal interpretation must be allowed, and generous mutual confidence, so almost universal among the best and strongest, must be both accepted and deserved.

In conclusion, I would venture to lay before the Congress the following categorical statements, with reference to the four questions proposed at the outset:—

1.—There are conditions under which a civilized country should give to the holders of foreign diplomas privileges equal or analogous to those accorded to possessors of her own.

2.—These conditions do not necessarily imply re-examination.

3.—The arrangements between nations, or between the mother country and her colonies, are in each case subject for adjustment.

4.—A country may, with due self-respect, grant a privilege to a State which nevertheless withholds the same.

DISCUSSION.

Dr. L. Markusovszky, Buda-Pesth: would only mention that in Hungary the law permits (1) That those who have passed their examinations and taken their diplomas in Austria can practise as if they had taken their diplomas in our universities; there is thus reciprocity. (2) In the case of foreign diplomas, if anyone can prove that the applicant has acquired the same preliminary education, has learned theoretical and practical medicine for the same term of years, as is required in our country, and that he has passed similar examinations to ours, he is received without any examination, or with such examination as will suffice to prove his identity. Otherwise he is required, before his diploma can be certified, to study one or two years more, according to circumstances, and to pass the ordinary examinations. In conclusion, he expressed agreement on all points with what Dr. Acland had proposed.

Dr. Billings, Washington: rose to explain briefly the situation of the United States as regards this question of reciprocity in the matter of recognizing medical diplomas. Everything connected with the regulation of the practice of medicine in his country came under the jurisdiction of the several States, and not at present under the jurisdiction of the general government. The United States, therefore, could not become a party to an international agreement on this subject, and no individual State had the power to make a treaty with a foreign country. In the majority of the States there were no regulations affecting the practice of medicine, but in all the States in which such regulations

exist, those diplomas which would entitle a man to registration as a medical practitioner in Great Britain would also be recognized as entitling him to practise in the State. It did not however seem to him that the question of reciprocity should influence the action of a government in this matter. Whether Great Britain should recognize American medical diplomas or not, should not in the least depend upon whether the United States recognized the English diplomas. The question of registration of physicians, that is, of defining who was to be considered a physician, was coming up in the United States to a great extent in connection with the registration of deaths. There could be no satisfactory registration of deaths unless the cause of death was reported by a physician; and when such registration was established by law, it became necessary to decide who should be considered and recognized as physicians. In Illinois this was done by the State Board of Health; in Alabama by the State Medical Society, which also acted as the State Board of Health.

Dr. Axel Spoof, Finland: explained that in this matter his country and Russia were not one. Re-examination was required in Russia before admission to practise.

Dr. Van Overbeek de Meijer, Utrecht: stated that between the Netherlands on the one hand and Belgium and Germany on the other, there was a convention enabling medical practitioners near the boundaries to practise within a certain limit in the adjoining countries without having to submit to any re-examination; a step in the direction advocated by Dr. Acland. He urged that Dr. Acland's conclusions should be adopted and strongly pressed forward with a view to their becoming general in all civilized countries.

Dr. Günther, Dresden: said that in the German Empire only those can style themselves Doctor who have successfully undergone the State Test Examination. An exception is made in those cases only where a State Test Examination has been successfully passed in another country, in cases of application for an official appointment. The Board of Examiners have to decide in these cases whether the applicant is duly qualified, and they can then dispense with any examination whatever if they see fit, in the German States.

Dr. Deffernez, Jumet: whilst admitting the advisability of such an arrangement as existed on the borders of the Netherlands, could not admit that it was right for one country to receive foreign practitioners merely on the presentation of their diplomas, unless the same was done by the adjoining country. Reciprocity was right, but not exclusive privileges. In Belgium, Germans and Englishmen have, in virtue of their diplomas, practised medicine, whilst Belgians found the door closed against them in those countries. He believed that Belgium was quite ready to meet the views expressed, but it ought to be on the conditions he had named.

Prof. F. de Chaumont, Netley: took exception to the views of M. Deffernez, who urged reciprocity. He agreed with Dr. Acland that reciprocity might not so be insisted upon; but that free trade, as far as possible, should not be carried out by this country. Reciprocity is a fallacy to be avoided. He added that we need not wonder that foreign nations should refuse our practitioners the right to practise in their countries—seeing that there is such a multiplicity and confusion of medical titles in this country. We had no less than nineteen licensing bodies, and sixty-two medical titles. A foreigner could not possibly understand that each of these was a legal title to practise? He trusted that the time was not far distant when our individual and corporate differences would be got over, and some single test examination would be established, by which it would be

at once and easily recognized that the holder of the same was a man of education and fit to be trusted to practise his profession both in his own country and abroad.

Dr. CHARBONNIER, Brussels: said, that apart from the privileges allowed to practitioners on the borders of his country, those who desired to practise in the interior were obliged to provide themselves with a certificate of moral character, and with a diploma which afforded proof of proper study at a recognized university. Failing this, the applicant was obliged to submit to an examination before a specially appointed Commission.

Mr. S. SAVONA, Director of Education, Malta: said that after the just and most liberal principles laid down by the learned President of the General Medical Council, in the eloquent Address that he had just delivered, he should not have thought of troubling the section with any remarks of his; but as none of the gentlemen who had spoken on the subject before the meeting had alluded to the vast Colonial Empire of Great Britain, the inhabitants of which, as far as the medical degrees conferred by Colonial Universities were concerned, were treated just as if they were foreigners or aliens, he hoped he should be excused if he said a few words on behalf of Malta, with which he was more immediately connected, especially as what he would say with reference to Malta would apply with equal force to all the British Colonies. Whatever might be said as to the justice or the expediency of recognizing the medical degrees granted by foreign universities—it was certainly most anomalous, not to call it unjust, that the degrees granted by a colonial university, under the British rule, should not be recognized by the British Government; and yet that was the present state of the law of England, according to which no medical appointment of any sort or kind could be held by any professional man unless his degree had been registered by the General Medical Council; and as the latter are forbidden to register the degrees granted by any foreign or colonial university, no Maltese, no Canadian, no Australian, whatever be the eminence they may have attained in their profession, could practise in England or hold any medical appointment in the Army or the Navy. He had been informed that a medical officer from Canada, who came to this country in medical charge of the passengers on board a steamer, had been declared to be disqualified to go back to Canada in the same capacity, because his degree was not registered in England. It was to be hoped that the anomaly would be speedily removed. Both the Government and the University of Malta were prepared to introduce any improvements that the Medical Council might consider necessary. But when the improvements required, if any, were carried out, it would be only just that the degree granted by the University of Malta should be recognized by the authorities of the mother country; and he hoped that the views expressed, all but unanimously, by so many distinguished British and foreign physicians and surgeons would hasten the solution of the difficulty proposed by Dr. Acland, and remove the disabilities under which not only foreign but also colonial graduates were suffering.

Dr. JOEL, Lausanne: said that the medical organization in Switzerland resembled in some points both that of France and of the United States. Not long since the cantons were free in this respect, but now there were four universities—viz., Berne, Geneva, Zurich, and Bäle—which issued diplomas conferring the right to practise in all parts of the Confederation. But at the same time each canton could admit to practise any one it chose, and some states were severe, others the reverse, in their restrictions. He thought that Switzerland was as yet not prepared for international freedom as to practise. Each year many foreign practitioners came to Switzerland for health and other purposes, and he thought it would be difficult to grant to them all a general permission

to practise; yet the Federal Council reserves to itself the right to grant such permission to certain distinguished men who decide to remain in the country, and several now practise under such authorization.

Dr. THORNE THORNE, London; read a communication which he had received from Dr. H. B. Allen, of the University of Melbourne, in which the writer complained that the colonial universities were treated as alien institutions. In Australia all graduates of British universities and colleges are registered as legally qualified practitioners without any delay or expense, but graduates of Australian universities cannot gain admission to the British Medical roll on any terms whatever. This absence of recognition is so thorough that though an Australian graduate may go to England as surgeon to a passenger vessel, he cannot obtain the same position in an outward-bound vessel. It was further submitted that the non-recognition of the Australian degrees was not justified by inferiority of the training and tests prescribed.

Dr. GRIMSHAW, Dublin: pointed out that from a British point of view we were unable to offer a standard by which the qualification of a practitioner on our Register might be measured, and stated that similar difficulties seemed to exist, in a minor degree, in several other countries. He referred to the former restrictions on practice throughout the United Kingdom, and which still exist between the mother country and the colonies. Until these anomalies were removed we had no *locus standi* upon which to enter upon a conference with a view to a medical union. Reciprocity could not exist, as many countries were so undeveloped in civilization that it was impossible that they could produce qualified medical practitioners. A permanent international commission was necessary to carry out a system of international right of practice. The only test of fitness to international medical practice should be registration, in an imperial register in each country, the international commission to decide where registration in an imperial register should entitle the registered persons to international rights of practice.

Dr. ACLAND, in reply: expressed his satisfaction that only one of the many speakers, had dissented from the general principles that had been laid down, and that one objection raised had been already removed by the other speakers, who all repudiated the idea that the ground of admitting a good practitioner to a country not his own should depend in any way on mutual recognition. Each country should discharge its own duty in its own way, and be judged by the consensus of scientific medical authority.

After the meeting Dr. ACLAND, as President of the Medical Council of Great Britain, with several of his colleagues, received the foreign members at the Medical Council Office, explained the detailed working of British Medical Registration, and showed the books, regulations, and publications used or issued since the passing of the "Medical Act" in 1858.

VI.—PRECAUTIONS TO BE TAKEN IN MEDICAL NOMENCLATURE AND CLASSIFICATION, TO GUARD AGAINST FALSE STATISTICAL CONCLUSIONS.

Dr. A. R. Rabagliati, M.A., Bradford: read a Paper on this subject. The author dealt with the following points:—

1.—Objections to the received classification and nomenclature.

2.—Need for a formal medicine as distinguished from the ordinary or applied medicine.

3.—Three classes of diseases proposed, according as they are due to organic matter, to inorganic, and to long continued causes.

4.—The steps in all sets of diseases essentially three—viz., depression, reaction, depression again.

5.—Functional or symptomatic names entirely to be discarded, and only pathological or etiological ones to be allowed. New names suggested.

6.—The aim of doctors would thus be to discover the part affected, the cause of the affection, and to name the disease accordingly.

Dr. Charbonnier, Brussels, addressed the Section.

Dr. Ezra M. Hunt, Trenton, N.J.: said that nosology, as related to nomenclature, should recognize the necessity there is to select or arrange terms, so that a statistical nomenclature may be formed, which shall be allied to and largely dependent upon, but not identical with, a nosological nomenclature. Nosologists and vital statisticians should confer together and agree upon that portion of our classification which needs to be primarily used in the reporting and tabulation of disease in the returns known as vital statistics. For the sake of comparison there should be agreement between localities and nations, so as to secure all the uniformity consistent with unavoidable local variations. It should be recognized that the statistics of special outbreaks, or endemics, need revised study by experts, by local inquiries, in order to make deductions most informatory. Selected lives, watched for long periods, and selected deaths, need to be studied, where we would arrive at great generalizations. We need greatly to simplify our life-tables, and to closely examine whether our combinations of figures give results which are not misleading.

Surgeon-General Balfour, Wimbledon: desired to call the attention of the Section to the necessity, in statistical investigation, for publishing the detailed return of diseases, instead of merely the results by classes, and thus putting it in the power of individuals to re-arrange the diseases in such a manner as may be deemed by them most suitable for the investigations on which they are engaged, instead of being compelled to accept a classification which may not be suited to illustrate the questions under their consideration.

Dr. Grimshaw, Dublin: considered the question surrounded with great difficulties, especially as it is essential to such classifications that they should not only be comparable in contemporary publications, but should also be comparable from time to time. The latter conditions could not be fulfilled if classification were altered from time to time in accordance with the advance of medical science. He agreed with the last speaker that a dictionary of nosological terms should be prepared to explain vital statistical tables and to detail the class and order to which each belongs. Such dictionary should be revised from time to time in accordance with the advance of medical

science; but the statistical tables should continue for as long a time as possible in the same form.

VIII.—MISCELLANEOUS.

Extract of a Report lately addressed to the Federal Council in Switzerland.

Dr. JOEL, Lausanne.

The Pharmaceutical Congress, which met at St. Petersburg in 1874, proposed, for the first time, I think, the formation of a Universal Pharmacopœia in Latin, with uniformity of weights, measures, and thermometry, the Government being left free to complete the scheme according to their several requirements. A formula was drawn up and submitted to the Congress by M. Méhu, of Paris; and it was decided that the Government of Russia should be invited to take the initiative by convoking an International Commission in order to settle this important question. The Central Committee of Apothecaries, which was presided over by Professor Trapp, of St. Petersburg, began the work with alacrity. M. Méhu's formula was divided among the delegates; and, notwithstanding the delay occasioned by the diversity of language, there was every prospect of a speedy settlement. The question, however, was not solved in 1875. The International Medical Congress of Brussels ratified the decision of the Pharmaceutical Congress, advised the introduction of medical men into the committee, to edit the Universal Pharmacopœia, and expressed the desire that Russia would invite the other Powers to a mutual understanding for the adoption of the Universal Pharmacopœia. The last necessary measures to be taken, in case of any unfinished operations, were left to the Congress of Geneva. The American delegates, Messrs. Adrian and Harwood, pleaded, for the first time, before the Section of Apothecaries, that uniformity might be extended to medical practice.

In 1877, the Congress of Geneva found the affair no further advanced than in 1875. Upon this occasion, M. Gille, the delegate of the Belgian Government, gave an account of the resolutions adopted by the Congress at Brussels, and Mr. Madden, of Copenhagen, spoke of the labours and difficulties encountered by the Russian Commission. At the same time he declared, that the end in view was at hand, although "it would be impossible to succeed without the assistance of the several Governments and the Faculty. The Russian Government has not as yet acceded officially to our request; but we have good reason to hope that it will do so eventually. With respect to the concurrence of medical men, I am sure that it is at this Congress that the subject has most chance of making way, especially in a country which has always taken the advance in questions of progress and of civilization."

The efforts made by the Section of Apothecaries were approved by the General Medical Congress, while the question was considerably enlarged, thanks to the impulsion given by the American delegates. The Medical Congress of Philadelphia had, in fact, already in 1876, resolved to carry out medical reforms throughout Europe, and Messrs. Seguin and Marion Sims, of New York, with Mr. Drysdale, of Philadelphia, were sent as delegates for this purpose to the Geneva Congress.

Mr. Seguin asserted the necessity of uniformity in the methods of medical observation (weights, measures, thermometry); he insisted with great force upon the benefits likely to follow its adoption, and commended the efforts which had been made in different countries for obtaining this result. "It will be," he said, "to the honour of the

Congress of Philadelphia, that its venerable and respected President, Samuel Gross, insisted, in his opening speech, upon one thing only, namely, that his hearers should work for the establishment of an international uniformity in medicine; as it will also be the honour of the Geneva Congress that it confirmed this principle, and voted the first executive measures of this great idea, under the influence of the great sympathy with progress, expressed in the opening speech of its philosophical president, Professor Carl Vogt."

The Section of Public Medicine immediately appointed a Commission, which comprised Messrs. Seguin, Gille, Madden, and Brun; and the conclusions, drawn up and presented at the General Session, September 14, 1877, by these gentlemen, were adopted with unanimity. In the same session, Professor Pacchiotti, of Turin, renewed a proposition he had previously made at the meeting of the Section of Public Medicine, September 12—namely, that the intervention of the Swiss Government should be solicited: "It is desirable that the Congress should request the good offices of the Swiss Government, in order to obtain an understanding between different countries upon this question."

The fifteenth article in the Report of the Commission was therefore drawn up as follows:—"The Commission will communicate to the Swiss Government all documents that refer to this question, and beg that the same may be communicated to the other Governments, so that the official sanction may be more easily and expeditiously obtained."

Two years later another Commission was nominated, to prepare a report for the Congress of Amsterdam. It comprised Messrs. Pacchiotti, of Turin, president; Gille, of Brussels; Seguin, of New York; Critchett, of London; Wilkinson, of Manchester; Marion Sims, of New York; Gubler, of Paris; Méhu, of Paris; and Brun, of Geneva. It was not possible for this commission, whose members were dispersed over both worlds, to meet, but its Convocation at Amsterdam, a few days before the Congress met, was announced by its Secretary, Professor Gille.

M. Gille's report left the matter nearly as it was before; he explained that if no application had been made to the Swiss Government it was because overtures had already been made to Russia, which overtures having been favourably received, it would not have been advisable to request the intervention of another Government, especially as it was difficult to separate uniformity in medicine from the Universal Pharmacopœia.

Unfortunately, later events showed that Russia, pre-occupied with other ideas, manifested little interest in these meetings; while the Pharmaceutical Society of St. Petersburg sent back, without a word of explanation, the formula of a Universal Pharmacopœia to M. Méhu, the author. Thus, instead of advancing, the project became more remote.

The conclusions of the Commission, adopted unanimously in the General Session of September 13, at Amsterdam, contained, amongst others, the following article:—

"Article 4.—The Committee of the Congress of Amsterdam will enter into arrangements with a Government of their own choosing, which may be disposed to promote the nomination of international delegates, and to settle everything which concerns the meetings of the said delegates."

Notwithstanding the admitted importance of the Universal Pharmacopœia, the most important question brought before the Congress was that of uniformity in medicine, and the pharmacopœia was only one of its desiderata.

Mr. Seguin, in one of those hearty and humorous speeches, of which he alone has the secret, recalled what Professor Charcôt said to the English doctors at Cork in May, 1877, "Gentlemen, it is not your native language which prevents me from being acquainted with your works—it is your gothic weights and measures."

Mr. Seguin, among other things, demanded a metrical system as the basis of international medical uniformity, metrical gradations of instruments, the recording of personal and hospital observations, with tables of medical statistics, arranged according to a uniform plan, so as to give harmonious results. Further, the subject takes a still wider range, and other wishes are expressed. Dr. P. van Overbeek de Meijer, Professor of Hygiene, at the University of Utrecht, introduced this important question: "By what means can the several Governments afford immunity from contagious epidemics?" and he concluded that "all civilized Governments ought to act together in this matter." Lastly, Dr. Joseph Seco Baldor, Professor of the Faculty of Madrid, demands a uniform method of medical instruction, and that Latin be again used as a scientific universal language. "Dead language, it is universally called; immortal language, said Lamartine."

The Utilization of Town Sewage by Surface Irrigation.

Dr. ALFRED CARPENTER, Croydon.

I propose to establish the truth of several propositions connected with sewage irrigation by means of evidence which has been afforded to me by personal experience. Theory would be useless unless practice coincided. This is especially the case with regard to the first proposition which I submit for consideration, viz.:—

A.—That the application of sewage continuously to land in close proximity to dwelling-houses, is not injurious to the health of the inhabitants of those houses.

The practice of sewage irrigation has been under my own immediate observation for twenty-one years. In the year 1860 it was commenced in some fields at Beddington, which have been continuously irrigated, more or less, from that time to this. Since then I have heard the most serious charges made against sewage irrigation in both Houses of the Legislature, also before commissioners and judges, and juries and arbitrators, as well as at public meetings, and in special reports, and violent attacks have been made against it in the public press. The charges have been that dwelling-houses, even within a mile of a sewage farm, are prejudiced and damaged as residences, in consequence of the unhealthiness which must necessarily belong to such farms.

I propose to show that the application of sewage to land in close proximity to a large population is not injurious to the health of that population; and that none of the evils which have been prophesied as certain to follow have resulted to those who have been exposed to such influences; and that the alleged damage is based upon a fallacy.

The sewage farm, which belongs to the Croydon Local Board of Health, is in the parish of Beddington, to the west of the parish of Croydon, but in close proximity to the densest portion of the population of that parish.

To the south is the Beddington Female Orphan Asylum, with nearly 200 inmates, the gardens attached touching the irrigated land itself.

The parish of Beddington, when the farm was first established, contained 1,557 persons.

In 1881 the population had increased to 5,492; and the rateable value is £42,000.

It is true that the houses provided for the new population are not all close to the farm, but the major portion are within half a mile, and of the whole scarcely any are outside the mile circle.

The increase of the population has been 250 per cent. since the farm has been established.

The population of Croydon in 1861 was 36,000, and in 1881 it is nearly 79,000;

otwithstanding the large increase, it is only a little more than 100 per cent. in the same eriod as compared with Beddington.

I produce this evidence as proof that the farm has not injured the parish by reducg its value or interfering with its prosperity. The majority of the population are ot the very poor who are obliged to live where they happen to be, but they consist f well-to-do citizens of London, who live where they like.

The effect upon health is shown by the figures contained in the table of vital atistics which I have placed before you. A district which shows an average deathte of 14·3, and an average birth-rate of 32·6, cannot be seriously affected by any reat and overwhelming nuisance; whilst its injurious effect, if any, upon the dense opulation to the east of the farm must be nil, or even less than nil, as it is as low as, if ot lower than that which is to be found in any district around London. I shall deal ith the death-rate of these districts more especially when I deal with the effect of ie produce on the general health.

The evidence afforded to the parishes of Croydon and Beddington is supported by rge sewage farm areas in other parts of the country. In no single case has it been roved that disease has been propagated to any given neighbourhood by its proximity a sewage farm. The instances of Lunatic Asylums, Orphanages, Workhouses, and ther similar establishments which utilize their sewage close up to the walls of their wellings, and consume the produce in their establishments, without the least damage sulting from the custom, are now so numerous, that simply to name them would ccupy most of the time at my disposal. In the majority of these instances irrigation as been used, as it has been at Beddington, in a perfunctory way, and not in the truly cientific way which it requires to bring about the best result. Yet the sequence is to hem most satisfactory, both sanitarily and pecuniarily, for reports in which it is stated hat the whole of the vegetable produce required for the establishment has been produced upon their sewaged plots are very numerous; and many add to their reports the act of the beneficial influence of the produce upon their inmates.

The second proposition of my paper is that:—

B.—The judicious application of sewage to land will satisfactorily cleanse the ffluent water from that land, and fit it for discharge into any ordinary stream.

It is now twenty-five years since the riparian owners upon the banks of the Wandle rocured injunctions against the Croydon Local Board, prohibiting the discharge of ewage into that river. Those injunctions still remain in force. No application has een made to any of the law courts of the country for them to interfere with its working ince the Beddington farm has been in operation: proving that the law has been comlied with by the Local Authority, and that the water has been purified from sewage ollution.

Repeated analyses of the effluent have been made by various officials, such as the River Pollution Commissioners, the Rural Sanitary Authority, the Croydon Local Board f Health, and many private individuals, some being employed for the purpose of inding laches, without any one being able to state that the effluent was not sufficiently ure to pass into the ornamental waters which constitute the Wandle. It is to be emembered that this river is a peculiar one, running from the Croydon sewage farm ainly through the gardens and pleasure grounds of gentlemen.

Analysis of Effluent Water from Beddington Sewage Farm.

100,000 PARTS.

Authority.	Solids in Solution.	Organic Nitrogen.	Ammonia.	Nitrogen oxidised.	Chlorine.
Frankland Average of 29 Analyses	39·07	·124	1·49	·401	2·718
Hassall	44·0	·0297	·002	·56	2·0442
Odling	37·1	·026	·06	·598	3·4
		Albuminoid Ammonia.			
Wanklyn March 13, 1880.	...	·055	·04	...	4·28
Oct. 14th ...	...	·044	·08	...	4·1
Dec. 11th ...	...	·02	·04	...	4·1
Dec. 16th ..	...	·01	·12	...	3·0
London Water ...		0·24	·004	·192	1·6
Ordinary Town Sewage.	72·2	2·205	6·703	·003	1·26

I contend that irrigation is the only proceeding by which the ammonia can be extracted from the water. There is a point which has to be borne in mind in analyzing effluents, that ordinary water passed out and through soils will, under certain circumstances, bring away with it some nitrogenous matter sometimes in the form of ammonia from those soils, but it is in a different form to that which is contained in sewage. It exists as a harmless salt, and not in that form in which it goes on to the land as sewage; nearly 99 per cent. of the injurious form has been extracted from the water. So also as regards chlorine; its presence in effluent water is of no more moment than if the matter had come from salt marshes; it is separated from the organic matter which gives to sewage its serious aspect, and it is not in any way a proof of that contamination which chlorine is usually supposed to give. The same argument must be used regarding the organic carbon. If pure rain water is passed over a grass field, after the grass has been cut, there will be organic matter in the effluent, which is perfectly harmless as far as the public health is concerned, and its presence must not be taken as being a part of any previous sewage contamination.

The effluent from a sewage farm is for these reasons of a very different character from that which belongs to water contaminated directly by sewage; the materials in the effluent are just those which pure water will abstract from a soil containing ordinary organic matter, salts, and vegetable produce.

The third proposition which I submit is :—

C.—That vegetable products from fields continuously irrigated by sewage are satisfactory articles of food for animals and man; that animals fed entirely on sewage produce are as healthy as similar animals fed on ordinary agricultural produce.

I prove this by a reference to the fact that nearly the whole of the milk which has been produced on the sewage farm has been consumed during the past ten years in the town of Croydon. That a very large portion of the meat which has been raised on the farm has been eaten as food by the Croydon people. That much of the grass which has been grown on the farm has been consumed by animals in Croydon, and that if there was any injurious quality in the produce of a sewage farm, it must have shown itself upon the population who have eaten that produce. Besides this, the quantity of vegetables and watercresses has been excessively great.

[A table, setting forth the vital statistics of Croydon, was exhibited, and it showed a reduction in the death-rate from 20·46 in 1870, to 16·16 in 1880.]

Whilst the death-rate for London averages 22·5; that of Croydon shows a steady and regular diminution, which is more decided during this present year than any that have ever preceded it.

As regards another charge made against sewage farms, I have searched the reports of the medical officers connected with the destitution authority, and consulted my medical friends as to the presence of parasitic disease among the people. There is not a particle of evidence to be obtained in support of the idea, once generally entertained, that sewage farms would spread parasitic complaints.

The fourth proposition, D, regarding "the non-infectious character of the excretions of those suffering from epidemic and infectious diseases when distributed upon a sewage farm," is proved by the fact that there have been occasional outbreaks of infectious diseases in Croydon during the past ten years, including two epidemics of small-pox, several outbreaks of scarlet fever, occasional cases of diphtheria, and three periods of typhoid prevalence—two of which were distinctly connected with contamination of water supply in its distribution, and a third was distributed by means of milk. In the years 1875-76 the excreta of at least a thousand cases of enteric fever were utilized on the farm. In the majority of the cases the excreta were certainly not disinfected, and had they been capable of setting up the disease, some of the sixty-five persons at that time in the employ of the Local Board must have suffered from the infection.

I was then Chairman of the Farm Committee, I watched day by day very carefully for the manifestation of disease, but not a single case arose among those employed by the Local Board, and I could not hear of any person suffering from the disease whose avocation brought him on to the fields, and this included some sixty to seventy other persons. There is no history of any development of infectious disease among those employed on the farm. The neighbourhood is not subject to outbreaks any more than any other district which is not provided with a satisfactory sanitary supervision, but in which cesspools abound, and a portion of which is dependent upon surface-wells for the supply of water to its inhabitants. The condition of the district upon these points is unsatisfactory, and yet, notwithstanding this, the vital statistics of the parish of Beddington and the hamlet of Wallington, which I now put before you, will contrast favourably with similar districts in any part of the country. The average death-rate for the last ten years has not exceeded 14·3, and the zymotic death-rate not anything like so high even as in the district of Croydon itself. The average for Croydon, including infantile diarrhœa, being 2·67, whilst that of Beddington is only 2.

I contend that the excreta of patients suffering from infectious disease are not capable of spreading infection, unless they have undergone some kind of zymosis, a kind of change which becomes impossible as soon as the sewage reaches the soil upon the farm. The changes are then in a direction which is opposed to zymotic life: zymotic disease does not appear in consequence of the utilization of sewage upon land. The cases which did arise were not on the farm, or even, in the majority of cases, near to it, they were

on the hills, beyond the range even of subsoil water. The changes in sewage are not in any way similar to those which have been known to take place in Poudrette and other particular forms of dried ordure. There is no doubt in my mind of the destruction upon sewage farms of the germs of mischief, and which, when unaltered, are capable of setting up zymotic disease. They are not preserved as they may be in the dried ordure, or in other products in which so-called disinfectants have been been used, but which have simply preserved the germs from decay, but they are chemically and physically altered so that mischief cannot arise. This result has been also found to apply to the excreta of animals suffering from epizoötic disease. During the past few years there have been several outbreaks of infectious pleuro-pneumonia in the Croydon district, the infection being brought from the metropolitan meat markets. The cow-sheds in which the disease has arisen have drained into the Croydon sewers, and blood and excreta from the slaughtered animals has been washed down those sewers. The sewers have carried the morbid matter from the sheds to the farm, but there has been no corresponding disease among the cattle upon the farm.

I contend that this immunity will only be continued on condition that the sewage is kept near to the surface of the farm, not if it be allowed to percolate too deep into the soil and so escape from the reach of the oxidizing influences of the atmosphere, and the appropriating power of the cellular tissue of plant life. Neither will it be absolute unless the excreta are brought in a fresh state to the farm. The sewage must also be kept in motion until the organic constituents are altered.

E.—I contend that the changes which take place in sewage when kept in motion, and early brought into contact with soil and vegetable life, are in a direction which is contrary to that which is necessary for the development and spread of parasitic and zymotic diseases. This proposition is established by the evidence which I have put before you.

My sixth proposition follows from the premises already stated, and we may therefore infer:

F.—That sewage irrigation properly conducted in close proximity to dwelling-houses is sanitarily safe. Meadows may be left quite under water in severe frosts without any damage resulting to the crops, although the surface may be covered with ice. The utilization continues, and a store is laid up ready for use by plant life, the moment the temperature rises sufficiently for it to become assimilated in the plant; and it is an established fact, that fruits and vegetables of all kinds can be successfully cultivated by the aid of sewage.

The seventh proposition that I contend for will be put in general terms:—

G.—"That in consequence of the peculiar productiveness of properly managed sewage farms, it is wise political economy to encourage their formation in the neighbourhood of all those towns which have an abundant water supply." It has been established beyond contradiction that irrigated fields are peculiarly productive; and that if they are properly managed the quantity of produce is wonderfully great. I am not about to quote to you the productiveness of the Beddington fields, for they have been damaged by mismanagement. I had for a few years the control of the farm, as Chairman of the Committee, and I worked at it incessantly for several years.

The main object which I had in view was to develop its grass-producing power. Carefully considered observations showed me that 7,500 tons of Croydon sewage, representing the sewage of 130 persons, might be dealt with each year by one acre of land provided an average amount of forty-two tons of grass were removed from that land I found also that there was no difficulty in obtaining that amount of produce, or an equal amount of roots or cabbages from each acre, which was properly cultivated, and I also found that it was best for the interests of agriculture that each fourth or fifth year

should be devoted to a cereal crop, some of the long manure from the farm-yard being utilized upon the land directly after the crop came off. I found that the quantity of grass sometimes reached the amount of sixty or sixty-five tons in the year, whilst at times as much as fifty-two tons of mangold could be got from each crop. In bad years, that is, in cold and wet years, this diminished to thirty or forty tons. The result of my observations were: that the produce from a properly cultivated sewage farm is three-fold compared with that which is obtained from ordinary farm land. That as population produce sewage, so it produces a demand for the very material which a sewage farm can raise most satisfactorily—viz., milk; and that the direction in which alone the sewage farmer can properly work his farm is to supply that demand which a neighbouring population will always require. Secondly, that as the grass produced is probably more than a milk-producing farm will consume, the excess should be sold or used in growing young stock, ready, in the course of time, to take the place of the cows which have become barren, and which should be then fatted for the butcher. That in those years in which grass is not grown on certain plots, roots should be cultivated after the grass has been broken up. The cattle also require them for winter food, and after roots, cereals should be grown for the sake of the straw also wanted on the farm, but still more for the necessity which exists for allowing the irrigated land to have a rest, so as to be well broken up for those aerating purposes which are so absolutely required for properly oxidizing the sewage. That to grow cereals on land which has been extensively irrigated, and immediately after rye grass, is almost certain to bring direct loss to the farmer. That with a proper rotation of crops and sowing of rye grass at the proper time, it is possible to raise the necessary quantity of produce from a given acre of land, so as to exhaust the nitrogen in the soil. Departure from these lines must be loss instead of gain.

Notwithstanding mismanagement, the farm has been a perfect sanitary success, and its produce, comparatively little as it is, extremely beneficial to the population who have the opportunity of using it.

H.—The eighth proposition which I advance is also based upon experience—It is, that the rainfall and the subsoil water should, as far as practicable, be separated from the sewage. The former water should always go into the natural watercourses of the country, and sewage should alone be conducted on to the farm.

I do not insist upon the hard and fast line of the removal of all storm waters. The rainfall of crowded streets and dirty courts must go into the sewers; but sewers must be comparatively water-tight, and neither give out or take in from the subsoil if they are to be perfect sanitary works; and sewers without an abundant water supply are still more likely to be mistakes.

As a sequence to these observations I make my ninth proposition:—That the utilization of sewage should be as near as practicable to its own water-shed; and that a number of small areas should be aimed at rather than to make immense depôts of sewage, which cannot be dealt with from their very magnitude.

DISCUSSION.

Professor Corfield, London, said: I agree with almost the whole of Dr. Carpenter's observations; but consider it has been very conclusively shown that to ensure purification at all seasons of the year, it is necessary that the sewage should pass through the soil and not merely over it. The Sewage Committee of the British Association have shown that where surface action is relied on the sewage is not purified in winter, but only when the crops are growing actively; whereas when sewage is passed through the soil it is as completely oxidized in winter as in summer; that Committee have also shown that

where sewage is passed over two fields, and there is little or no percolation through the soil, the sewage may be more impure after it has passed over the second field than it was when it came into it from the first. We also made experiments extending over five years to determine the percentage of the manurial ingredients of sewage that were utilized by irrigation: to do this, we ascertained the amount of nitrogen in the sewage by taking samples regularly and frequently, and in quantities proportional to the flow of sewage at the time, mixing them, and then analyzing the mixture at stated intervals; we also ascertained the amount of every kind of crop, and there was a great variety of them, and calculated the amounts of nitrogen in them; the result was that we were able to show, that on an average of five years, 33 per cent. of the nitrogen which came into the farm in the sewage was utilized in the crops, or about the same proportion as is utilized in the case of the best artificial manures. With regard to the scare about entozoic diseases, there is no evidence at all that such diseases are spread in cattle or in men by sewage farming; the British Association Committee had an ox which had been fed for twenty-two months on sewage produce, slaughtered, and its carcase was examined by Dr. Cobbold, Professor John Marshall, and myself, without any trace of entozoic disease being discovered. The great difficulty with wide irrigation is the disposal of the sewage when it was not wanted on the land; this must be met by having a portion of the farm laid out as a filter bed to receive and purify the sewage when it is not wanted by the crops, so that the disposal of sewage on land will be accomplished in one of two ways, either by means of wide irrigation supplemented by a filter bed; or by intermittent downward filtration, supplemented where possible by more or less irrigation of neighbouring land.

Mr. Edwin Chadwick, C.B., London, said he quite agreed with what Dr. Corfield had said about the necessity of sewage passing through the soil, and not merely over it.

Harnuntersuchungen mit Rücksicht auf öffentliche Hygiene.

Dr. W. Zuelzer, Berlin.

Meine Herren!—Die Beurtheilung der individuellen Verhältnisse steht gewiss bei allen Fragen der Hygiene oben an. Selbst wenn man alle Schädlichkeiten kennen würde, welche die Gesundheit bedrohen, so bleibt immer zu untersuchen, wie sich dass Individuum dagegen verhält. Die Ausdrücke "verminderte oder gesteigerte Resistenz gegen Krankheit und Tod" sind wohl längst gebräuchlich, aber entbehren jeder anderen Grundlage, als der des subjectiven Urtheils. Hier kann die Untersuchung des Harns ebenso Vortheil bringen, wie sie in der klinischen Medicin als diagnostisches Hilfsmittel dient.

Es handelt sich hierbei um folgendes:

Wir müssen annehmen, dass jedes Organ des Körpers in dem Maasse zersetzt wird, in dem es Arbeit leistet, und dass es sich in der Masse regenerirt, in welchem ihm genügende und zweckmässige Nahrung zugeführt wird. Welches das Mass ist für eine genügende oder eine überflüssige Ernährung, darüber entscheidet nur die bis jetzt noch sehr unvollkommene Empirie. Auch über die Leistungsfähigkeit des Organismus fehlt jeder andere Massstab. Wir würden sehr in Verlegenheit gerathen, wenn wir die Leistungsfähigkeit des italienischen Arbeiters oder Soldaten mit der des deutschen, englischen, oder französischen unmittelbar vergleichen sollten.

Die Harnuntersuchung liefert nun eine objective Methode um unsere Beobachtungen über solche Fälle zahlenmässig auszudrücken. Nach den Principien, welche ich zuletzt in meinem "Lehrbuch der Harnanalyse" entwickelt habe, müssen wir annehmen, dass

jedes Organ des Körpers in demselben Masse, in dem es durch die Vorgänge des Stoffwechsels zersetzt wird, seine Urberstandtheile verliert und der Ausscheidung überliefert. Da unter den Excreten der Harn das wichtigste und am leichtesten zugänglich ist, so haben wir die Aufgabe, das Verhältniss seiner einzelnen Bestandtheile zu einander, d. h. ihren relativen Werth festzustellen, um sie auf die Organe, aus deren Zersetzung sie stammen, zurückzuführen. Eine nothwendige Voraussetzung ist dabei die Kenntniss der Zusammensetzung der einzelnen Organe, soweit sie durch deren Gehalt an Stickstoff und Mineralstoffen ausgedrückt wird.

Es wird alsdann darauf ankommen, durch möglichst ausgedehnte Untersuchungen festzustellen, welche Ausscheidungsgrössen bei dem gesunden Individuum von einem bestimmten Alter, von einer empirisch festgestellten, guten Ernährung u. s. w., für ein bestimmtes Klima, &c., vorkommen. Dies wird der Ausgangspunkt unserer Betrachtung sein müssen.

Nehmen wir an, um ein Beispiel von Prof. Edlefsen zu benutzen, dass wir bei der Untersuchung des während des Vormittags entleerten Harns eines gesunden Individuums, 4·6 Grm. Stickstoff und 9·408 Grm. Phosphrsäure, also einen relativen Werth von 8·8, finden, so lässt sich hieraus die Ursprungsstelle dieser Ausscheidungsproducte etwa in folgender Weise durch Rechnung feststellen (Nahrung: Morgens Milch und Brot:

			rel.
Aus den Einnahmen . .	0·78 Nu	...	0·164 $P_2 O_5$ = 21
Aus der Nervensubstanz .	0·146 „	...	0·064 „ = 44
Aus der Muskelsubstanz .	1·1 „	...	0·154 „ = 14
Aus den Blutkörperchen .	2·6 „	...	0·026 „ = 1
Summa:	4·6 „	...	0·408 „ = 8·8

Unter der Voraussetzung, dass die Versuchsperson ein normales dem entsprechenden Alter angehöriges, Individuum wäre, so würde jede Verschiebung in diesen relativen Zahlen für das Excret eine Alteration des Stoffwechsels bedeuten. Und zwar würde sich dann ermitteln lassen, ob das Blut, die Nervensubstanz oder die Muskeln, u. s. w., in einem höheren Grade dem Zerfall unterliegen, als der Norm entspricht. Mann wird dann leicht feststellen können, wie weit eine ungefährliche Abweichung gehen darf, oder wie gross die Abweichung sein muss, um bereits die Prädisposition für eine Schwächung des Organismus, oder für die Invasion einea Krankheit zu bilden.

Am nächsten lag mir die wichtige Frage des Alkoholismus an der Hand dieser Thatsachen zu untersuchen. Die Kürze der Zeit erlaubt mir nur einige Andeutungen. Wenn wir uns fragen, aus welchem Grunde greift der Mensch zum Alkohol, so lehrt ein Blick auf die Tafel, welche diese Verhältnisse graphisch darstellt,* folgendes:—

Der Genuss von Alkohol in nicht übermässigem Grade bewirkt zunächst, was den Antrieb zum Alkoholgenuss zur Genüge erklärt, eine gewisse Excitation; das Nervensystem wird angenehm angeregt und die Muskelkraft wird gesteigert. Die, diesem Zustand ensprechende Qualität des Harns—eine bedeutende Verminderung der relativen Phosphorsäure—bezeichnet eine Herabsetzung des Nervenstoffwechsels.

Der Excitationszustand hält aber nicht lange an. Nach mehr oder weniger kurzer Zeit macht die Excitation einer Erschlaffung Platz, und dem entsprechend zeigt der Harn eine bedeutende Vermehrung des relativen Werthes der Phosphorsäure. Letzteres deutet auf einen gesteigerten Zerfall der Nervensubstanz.

Wiederhohlen sich solche, den Nervenstoffwechsel einseitig alterirende Eingriffe ofter, so wird es klar in welcher Weise der Organismus Schaden nimmt.

* Die Tafel hat inzwischen im Parkes' Museum Aufstellung gefunden,

Beim Genuss von Wein treten diese Schädlichkeiten ungleich weniger hervor; den Grund davon suche ich in folgendem: Der Wein enhält bekanntlich neben Glycerin, mehrere Mineralbestandtheile, unter andern auch Phosphorsäure, so dass das Individuum im Stande ist, sich die, zur Ernährung der lecithinhaltigen Körperbestandtheile (Nervensubstanz, Blutkörperchen) nothwendige Glycerin-Phosphorsäure. selbst zu bilden. Ausserdem aber kommt darin, wie ich vorgestern in der Section für Medicin mitzutheilen die Ehre hatte, nach meinen Untersuchungen über den Ungarwein, auch präformirte Glycerin-Phosphorsäure vor. Weil dadurch dem Organismus ein fertiges Nahrungsmittel für dasjenige Organ, das Gehirn, dargeboten wird, welches durch den Alkohol am meisten leidet, so ist es klar, weshalb der Wein ungleich weniger schädlich wirkt, als der Gebrauch von reinem Alkohol.

Die Untersuchungsmethode, welchezur Erklärung sich einer verbreitenden Schädlichkeit diente, lässt sich, wie sie leicht einsehen werden, auch auf die Ermittellung des Einflusses anwenden, den die mannigfachen Agentien, mit denen der Mensch zu rechnen hat, auf ihn ausüben.

Es genügt hier, Ihre Aufmerksamkeit auf diesen Punkt gerichtet zu haben. Wie ich mehrfachen Mittheilungen entnehme, glaube ich, dass wir aus den Kreisen der Militärärzte und der Gesundheitsbeamten bald weitere Untersuchungen hierüber erwarten dürfen.

An important paper by Dr. TOSCANI on the Sanitary State of Rome was distributed amongst the members, but no time remained for any discussion upon it. Under similar circumstances a paper "On Premature Interment," by Dr. THOMAS EMBLING, of Melbourne, could not be fully read or discussed.

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